UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

FULL BOARD MEETING

EVALUATION OF RANGES OF THERMAL LOADING

FOR HIGH-LEVEL WASTE DISPOSAL IN GEOLOGIC REPOSITORIES

October 9, 1991

St. Tropez Hotel
Las Vegas, Nevada

BOARD MEMBERS PRESENT

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Nuclear Waste Technical Review Board

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Nuclear Waste Technical Review Board

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(Cont.)

P R O C E E D I N G S

8:30 a.m.

DR. DEERE: Good morning, ladies and gentlemen. I trust you all slept well. I had just a little trouble falling asleep. I remembered that we had requested this meeting on thermal loading. We wanted to learn more about it. But, I kept seeing it coming at me in blue and then red and that wasn't so bad. But then, suddenly, it stopped and it rotated and it rotated and by then I knew I was asleep and having a nightmare. It was a most interesting and revealing presentation. It was lengthy, but it was certainly appreciated.

Well, today, it's my pleasure to have Dr. Warner North who is a member of our Board and Chairman of the panel on risk and performance analysis to preside. Warner?

DR. NORTH: Thank you, Dr. Deere.

Good morning and welcome to the second day of the Nuclear Waste Technical Review Board's meeting on thermal loading. Yesterday, we were treated to an extremely informative overview of international repository designs. We heard about how the various repository designs have been affected by thermal loading issues. We also reviewed the U.S. repository design, its history, and rationale. And, as Dr. Deere mentioned, we learned about multi-colored frog-eye plots.

Yesterday's discussions provide an excellent base for the important issues we will be addressing today. We are all looking forward to the upcoming presentations by those working at the national laboratories. Today's presenters have been asked to talk about how alternate levels of thermal loading will affect specific technical areas. For example, the U.S. design calls for high temperatures to help keep water away from containers for hundreds of years by boiling away any moisture that approaches the engineered barriers. However, what will happen to the host rock as a result of the high thermal pulse and the resulting cooling? How will such high temperatures affect the geomechanics, the hydrogeology, and the other mineralogical characteristics of the surrounding host rock? What will happen as the rock cools? Will the natural system continue to provide adequate waste containment under this kind of high thermal loading?

During licensing, DOE as the applicant will have to demonstrate an understanding of the effects of the thermal pulse on the repository and the engineered barriers. DOE must demonstrate that the repository and its subsystems will perform according to the established standards.

We have asked that the speakers today address six questions about their respective areas of expertise that could be affected by low versus high thermal loading. The questions are: (1) What are the potential problems associated with each area; (2) What is the significance of each of the potential problems; (3) What are the uncertainties associated with each of the potential problems; (4) Can these uncertainties be resolved; (5) How much time and money will be needed to resolve these uncertainties; and, finally, (6) Will there be residual uncertainties?

After lunch, our afternoon session will center on the enhancements and other considerations associated with various alternative thermal loading concepts. Later in the afternoon, we will hear a presentation by Mr. Peter Stevens-Guille of Ontario Hydro who will discuss the candidate engineered barrier concept. I've asked the Board's staff to take notes on today's presentations and to identify the key issues to address tomorrow afternoon during our Roundtable Discussion.

We have a great deal to do today. So, at this point, I will turn it over to Mike Cloninger who is Branch Chief of Field Engineering at the Yucca Mountain Site Characterization Project Office. Mike specializes in waste package and repository design and he is DOE's technical lead for this meeting. He will make some opening comments and introduce this morning's speakers.

Mike?

MR. CLONINGER: Thank you, Warner. As Warner already said, I'm Michael Cloninger with the Yucca Mountain Project, Chief of Field Engineering. This morning's session deals with uncertainties associated with high and low thermal loading.

The DOE's position is that establishing and understanding these uncertainties is key, crucial in fact, to the success of the program. The DOE's program focuses on reducing the overall uncertainty to an acceptable level. And, by acceptable level, we mean that within the remaining margin of uncertainty at the end of site characterization, license application development that we, the Department of Energy, feel that within our design margin everything will work in compliance with the regulations and to our satisfaction that the public health and safety is adequately protected.

I might point out that for the Yucca Mountain site and unsaturated zone site unique around the world reducing the thermal loads may not necessarily result in reducing the overall uncertainty, as you will see in some of the following presentations and, in fact, current evidence indicates presently quite the opposite.

The Department considers this morning's session to be the real focus of this meeting and the subsequent discussion, particularly the Roundtable tomorrow. The presentations this morning do deal with the uncertainties regarding high versus low thermal loading and you'll note that these topics are arranged somewhat in the order of the key inter-faces among these general technical areas, although there are a couple processes that exist well beyond just the interfaces adjacent here. In other words, geomechanical response of the overall system will have impacts on the hydrologic system. The hydrologic system has responding to that, plus the thermal load, will have geochemical and mineralogical impli-cations. We are not going to address these coupled inter-actions and multiple interactions to any great degree at this meeting; however, within the next two to three years, we expect to be able to address those in some detail.

The program for this morning will start out with Larry Costin from Sandia National Labs discussing the geo-mechanical or rock mechanics, thermal mechanics uncer-tainties; followed by Tom Buscheck of Lawrence Livermore National Laboratory who will discuss some of the hydrologic uncertainties. Geochemical uncertainties, a related topic, will be presented by Brian Viani of Lawrence Livermore National Lab. That may be in error. Mineralogical uncer-tainties, very closely related to the geochemical and hydro-logic uncertainties will be presented by David Bish, Los Alamos National Laboratories. The waste form and waste package materials, repository materials uncertainties will be presented by Greg Gdowski of Livermore. And, a fairly new topic, I think, to the Board, the biological resource concerns at the surface due to any imposed thermal increase or temperature increase on the surface will be presented by Ken Ostler of EG&G.

The next slide in your package is missing one of the questions that Warner discussed earlier. I won't go through these in any great detail. However, will there be residual uncertainties? Yes, of course, there will be. We cannot do the 10,000 year test. If so, how much? Again, if there's too much, we can't go forward with the license application. If we feel that we can bound those uncertainties, show compliance with health and safety requirements for the total system, we will go forward. As far as how much time and money will be needed for resolution of the uncertainties, that is within the program.

The speakers this morning will be focused on technical subjects, not financial or schedule topics. If you do have questions regarding schedule or costs, please address them either to myself or to Max Blanchard here in the audience today. Along those lines of costs and schedule, the resolution of these uncertainties is included in our long-range plan throughout the period towards license application. The approach is included in the site characterization plan and it's associated study plans, test plans, and other plans. The currently planned budget and schedule can accommodate quite a wide variation in thermal loading. However, we need to point out that for very, very low thermal loading we would have to change our test program, performance assessment program, and probably our license application basis.

Without further delay, I would now like to go to the program and our next speaker will be Larry Costin from Sandia National Laboratories who will discuss geomechanical uncertainties.

DR. NORTH: Mike, before you leave, I think we would like to come back to the issue of schedule and budget and have some discussion of that at an appropriate time.

MR. CLONINGER: Certainly. Okay.

DR. NORTH: So, we will not consider that as a question we're going to put to the speakers as they go along, but I don't want it to drop off of our list. I want to make sure we address it and, given the budgetary climate in which the program is presently operating, ask the question given the budgetary reductions, what is the impact on this aspect of the program?

MR. CLONINGER: Fair question. Thank you.

DR. COSTIN: My topic is going to be addressing the geomechanical uncertainties. It seems a little bit of a redundant title. Anything with "geo" is definitely uncertain. So, you'll see in the next slide here that I've tried to put together a road map of how I'm going to step through this presentation. First, I'm going to talk a little bit about what the impacts of various loading schemes might be on the geomechanics of the repository. Then, we'll talk a little bit more specifics about how those uncertainties are translated into looking at the rock mass properties, the effects on the rock properties themselves, what you have to think about in terms of failure criteria. These topics then cascade on down into additional uncertainties that you have with the induced stress changes with time. Those, of course, depend on the rock properties and the rock mass properties. These have an impact then on drift stability, fracturing, changes in permeability that might come about as a result of these additional induced stresses. Those then cascade on down into another level which how do you deal with those in terms of your design? How do you deal with these uncertainties in terms of the design? You have to do additional modeling. This means looking at developing more advanced models, doing additional testing in order to validate those models, and finally to get into how you incorporate that into your design methodology to look at designing a repository so that you can take into account these cascading uncertainties.

So, I'll leave that road map up there for a little bit and we'll start out with the first box here and we'll principally address what's the problem? You know, we have potentially additional loading due to the thermal impact of the waste, but does that really present much of a problem for the geotechnical side? The major problem comes about really in our estimation because of the changes in the magnitude of the stress field with time. You not only get changes in the magnitude of the stress field with time, you get changes in the orientation of the stress field with time. And, that's perhaps a bigger impact than just dealing with the magnitude of the stress changes.

You have some thermal effects on the rock properties themselves. As you heat it up, the properties of the intact rock change. When you have these additional thermal loads, you have to consider what are the effects of temperature on your support structure? What kind of support materials can you use that will not be impacted in the long-term by additional heating? Then, you have to look more carefully at what's the interaction between the rock mass and your support structure as you go through this large thermal cycle. So, these are some of the additional things that you have to consider in looking at the geotechnical aspects of underground structure when you have to consider this thermal pulse.

Just to give you an idea of what I meant by those first couple of bullets, this is a viewgraph that, I think, both Tom Blejwas and I have used quite often. Just to demonstrate what we mean by changes in magnitude and direction of the stress field. This was a calculation done for a floor emplacement of a drift. At the time the drift is excavated, these little crosses here represent the horizontal and vertical components of the stress field. And, as you can see, in an unheated case after excavation, the principal stresses are--the greatest principal stress is vertical and of not that great of magnitude. As heating begins, you can see quite dramatically that not only the magnitude of this stress field changes, but it begins to rotate. And, if you have a highly jointed structure out here, vertical and horizontal or whatever kind of jointing structure, this rotation then is going to induce different shear stresses. You may activate joints that under this circumstance would be quite stable. You not only rotate the stress field and get much larger stresses--the calculation was cut off at 100 years--but then you can imagine what happens in the long-term as things begin to cool down, these things begin to rotate back. And, so you've initially done some damage. You may have shifted the joint structure here, and then as things cool down, you get residual stresses built up. Things may pop back the other way. So, this is the major concern that we have to deal with.

So, let's go and talk a little bit about what these kinds of uncertainties might be and I'll give some examples and try to go through this primarily by example. Look at what the thermal effects on the rock properties themselves, in terms of thermal conductivity, expansion, changes in rock modulus, effects on the failure of the intact rock.

If you look at thermal conductivity as a function of temperature, it's really almost temperature independent. The only effect is, is whether you have water in the system or you've dried the system out. As you dry the system out, the conductivity decreases somewhat in most cases, although there is few cases where it increases slightly depending on the mineralogic content of the rock. So, there's not really that much of an effect on the conductivity.

There's a slightly bigger effect on the thermal expansion behavior of the rock. Down here in the lower temperature regimes (indicating), thermal expansion is pretty much linear. As you get up into this region (indicating), it begins to go non-linear primarily because you're getting phase changes, I believe, in some of the minerals that cause volumetric expansion, as well as just the volumetric expansion due to the thermal component. So, you're getting alteration of minerals which will be talked about later which increases the apparent expansion of the rock beyond what you would predict from, say, the sort of linear thermal expansion coefficient. So, one of the things is keeping the temperature limits near the borehole away from this kind of a region to allow you to have a better borehole stability.

Modulus of the intact rock, there's some effect on temperature. This represents basically two series of tests, one at room temperature and one at 150°C. You can see there is a slight decreasing trend within the scatter of the data. These tests were uniaxial tests at a strain rate of 10^{-5}.

And, finally, we'll take a look at intact rock failure. These were from uniaxial compression tests. Here, the scatter in the data begin to kind of overtake the apparent decrease in the strength, although if you average a fairly large number of tests, you do see a slight decreasing trend. But, again, there is at room temperature a very large scatter in the data and it's not clear that any additional uncertainty would be introduced into this system as you heat it up because, as you can see, there's still a very large uncertainty even at room temperature or temperatures at the working area.

DR. LANGMUIR: Larry?

DR. COSTIN: Yes?

DR. LANGMUIR: Those tests, are you using the rock at any particular orientation or is it randomly; in other words, a cross bitting?

DR. COSTIN: Those tests were taken from a core that were recovered from some of the early core holes. And, also some tests were done on outcrops from Busted Butte (phonetic) and I'm not sure what the orientation was, but I suspect it was primarily--the loading axis was primarily a vertical orientation. I don't think there is that great, at least in the modulus and failure, that there is that great of an influence in the Topopah Springs, anyway, of orientation. I know Ron Price has done some tests to look at anisotropy and there is a small effect, but it's certainly again one of those effects that within the scatter of the data. So, it's really hard to detect.

Well, now that we've seen the effects on the intact rock, we now have to look a little bit broader horizon and say what's the impacts then on the rock mass itself where you have intact rock plus the rock structure that joints, et cetera. So, let's take a look at what the impacts might be on the rock mass. The interesting thing about considering a rock mass and having a highly jointed structure is that the joints really cause you to get this coupling between the thermal expansion and the rock mass modulus. In other words, the stresses that you can develop in the rock mass are highly dependent on the jointed structure. If you have a very poor quality rock, you can't generate the kinds of stresses even at the same temperature that you can with a far less jointed rock.

DR. DOMENICO: Larry, is that the bulk modulus? What is the rock mass modulus? --bulk modulus?

DR. COSTIN: The rock mass modulus is the equivalent of Young's modulus, but for, say, a large rock mass including the structure.

DR. DOMENICO: Okay. It's the reciprocal of the compressibility?

DR. COSTIN: Yeah.

DR. DOMENICO: Okay.

DR. COSTIN: In doing the design, what you would like to be able to do is to know the rock mass modulus so that you can use some simple elastic calculations to try to calculate what the stresses are around openings and things like that. You can't use the intact modulus because the intact modulus gives you a much higher value than the modulus that would see with intact rock plus joints because you have these fractures in there that cause the system to be much more compliant. As you get this volumetric expansion of the rock due to heating, it actually tightens up these joints and so you have an apparent increase in the rock mass modulus as you begin to heat things up.

The stresses then that you generate become a non-linear function of temperature and you can get changes in the permeability in the near-field because either joints are closing, or if you create shear stresses, you may, in fact, dislodge joints and create new pathways. So, these are the kinds of things that you have to look at when you're dealing with the rock mass.

To give you an example, if you look at--this was some experiments done on joints in tuff to look at the compressibility of a joint and it's basically a cylinder with a joint in it that's compressed and gauges are used to distinguish the difference between what the compressibility of the intact rock is and the compressibility of the rock plus the joint. So, you can subtract out the rock and get what the effect of the joint itself is. In our modeling, we try to simulate this behavior by again separating out the effect of the intact rock and the joint and we do put in a different compressibility for the joint itself. So, we treat those as two separate entities and try to combine them in order to understand what the behavior of the rock mass is. And, in the models, we have the ability to look at both orientation and numbers of joints in a continuum sense. This model is a continuum sense. So, that's why it's called a compliant joint model or a continuum joint model.

The effect of looking at that, the point I want to get to, is if you do a calculation, a typical calculation of floor emplacement, this one was done for an 80 kW/acre case. And, you look at the stresses that are generated based on the kind of constitutive behavior that you put in, either linear elastic or something more sophisticated like a compliant joint or a continuum joint model, and you look at the stresses generated. You can get quite different answers depending on the kind of model that you're looking at.

And, what I want to do is to show a comparison of what the stresses are along this center pillar line in the next slide just to give you an example. This upper curve here is a linear elastic calculation based on using a rock mass modulus, as the Young's modulus and the elastic calculations, and the modulus itself was taken from moduli that were measured in G-Tunnel. So, it's approximately half of the intact rock modulus, but it's kept constant, not a function of temperature. As you can see, when you account for both the intact rock modulus and how it changes with temperature and the fact that you have joints at a certain spacing and orientation, the more sophisticated models that include the joints calculate this behavior. And, this was done with a couple of different models, different codes, and they all tend to agree. You can see that away from the canister which is placed here, that below the canister and above it, that the two calculations will tend to agree in areas that are not heated because the modulus of the rock is changing considerably as you heat it because the joints are tightening up. In fact, the modulus of the rock where you get it quite warm is almost the intact modulus because the joints are compressed together so tightly that the rock mass behaves as though it were intact rock.

DR. DEERE: Question?

DR. COSTIN: Um-hum?

DR. DEERE: What was the assumption of the initial state of stress and how sensitive are your results to that assumption?

DR. COSTIN: Okay. The initial state of stress is just the gravitational loading plus we include a horizontal stress that's a fraction of that. So, the initial stress state--what we did was you have the model put together. You load it with an initial in situ stress of about 5MPa vertical and I think it's 2-1/2 or 3 horizontally. Then, you excavate out the cavity or you kill off those elements that are in the cavity, let it come to an equilibrium state. So, you now have an equilibrium state known for just the excavation. Then, at some later time, we turn on the heater and watch what happens after that.

DR. DEERE: Right. But, my question is if you used a half then for the in situ state of stress, the horizontal with respect to the vertical, what difference would it make if you had a value of 3/4 or 1 which might be present, or 1-1/2 even, horizontal times vertical?

DR. COSTIN: Actually, in this case, it would probably cause a bigger difference here because the fact that as time goes on you get large horizontal stresses, it tends to close these joints up. If you have an initial state of stress that's horizontally much larger or much smaller than the joint spacing that you can accommodate by this volumetric expansion would be more or less. So, if you had initially a much higher horizontal state of stress, then the difference between these calculations would be less. If you had a less horizontal stress to begin with, then the difference between these would probably be slightly more.

DR. DEERE: Right.

DR. COSTIN: But, you can see by the magnitude of these that the 2 or 3MPa initial horizontal stress probably would get lost in the noise.

DR. DEERE: I think it would be interesting when you're making these studies to use a range of values.

DR. COSTIN: And, we do in many cases.

DR. DEERE: And then, I think your test program for the access drifts do have incorporated a number of in situ test determinations.

DR. COSTIN: The point I really wanted to make with this was that we do intend to measure this rock mass modulus and to use that in design calculations. However, you've got to be careful about what the implications of that are. You're measuring this rock mass modulus at the nominal in situ temperatures. As those temperatures change, you've got to understand that that rock mass modulus is going to change, but it at least gives us a point to start pinning our calcu- lations to and then we'll have to predict how that's going to change or we'll have to be able to measure the rock mass modulus as a function of temperature in some of the heated experiments which we hope to be able to do.

DR. DEERE: Right. And, another comment, if I may, while we're on this because this is the appropriate forum since you're dealing with the geomechanics. You recall at our meeting, I believe it was in Denver, when we talked about the rock mechanics testing, we discussed a little bit the possibility of using the radial jack so that you could incor- porate a much larger volume which I think could be very beneficial to try to get some information on the directional properties of your modulus.

DR. COSTIN: Right.

DR. DEERE: It allows you to go in every direction. Have you given any thought to that?

DR. COSTIN: We've given thought to it and we have put it into our plans that we intend to investigate that. And, in our initial test planning packages, we've taken that into account, that we may do different kinds of tests under the same name. We may do some traditional plate bearing tests. We may do other kinds of tests depending on the location and the size of drifts that we can get access to.

DR. DEERE: Thank you.

DR. ALLEN: On that last graph, what causes the asym-metry in the curve on the two sides of the zero point?

DR. COSTIN: Because the zero point is the floor of the drift, not the middle of the canister.

DR. ALLEN: The floor of the drift, okay. Okay.

DR. COSTIN: And, in this calculation, we do take into account the fact that you have air in the drift and you have radiative and convective heat transfer across that drift and that makes a considerable difference in the temperature profiles that you get around the drift. It's a more truer near-field calculation than what you saw yesterday.

DR. CORDING: Question, Larry. Is that profile right through the drift?

DR. COSTIN: No, that's in the centerline of the pillar.

DR. CORDING: The centerline of the pillar. I guess, it seems that very sharp difference at that distance away from the opening seems--intuitively, it didn't seem so obvious to me.

DR. COSTIN: It's along this line. This is -100 and that's +100.

DR. CORDING: Yeah.

DR. COSTIN: And, as you look at--now, this is not an isolated drift, okay? This is a repository type calculation where this is a plane of symmetry. So, there's another drift over here being heated and these things are infinite in this direction. That's a 2-D calculation. So, you're actually getting the effect of other drifts or other neighbors that are on down the line or impacting the calculation there, as well.

Well, let's begin to work our way down in this part of the road map and talk about uncertainties with respect to how you go about considering these things in the design process. There are always uncertainties associated with in situ conditions and the rock quality independent of whether you're going to heat it or not and those probably would not change much. The thing where thermal effects probably introduce the most uncertainty is that in the more traditional design methods for underground structures they tend to be more in the line of empirical methods that have been validated by extensive case history. Some of those case histories do include very deep mines that have high thermal stresses, but they don't change with time or they don't change much with time except for effective ventilation. There is really very little experience with the kind of situation that we're dealing with here. So, we don't want to ignore these. In fact, we intend to use them, but we also have to back those up by some additional work if we're going to incorporate and understand what the thermal stresses are going to do to the excavations. So, we incorporate the thermal component by taking a look at more advanced numerical methods and that's going to require additional constitutive model development which then requires more in the way of validation in the field scale tests.

So, this is really where the bulk of the effort to resolve these issues is going to come in, in that you have to do these additional field tests to look at what the thermal effects are in order to help validate your models which you can then have confidence in in doing the design.

DR. CANTLON: In your empirical methods, has there been any look at natural analogs where you've had volcanic events through tuff where you had long thermal pulses and then cooling? Has anybody looked at the properties of tuff?

DR. COSTIN: That's a good suggestion. I don't recall specifically anybody putting together a case history like that that I've seen anyway.

DR. CANTLON: It would seem the natural place to look for--

DR. COSTIN: One can look at evidence of regions of tuff that have gone through this thermal cycle and look for what impact that has on the rock mass.

DR. CANTLON: Yeah. Sure.

DR. COSTIN: The problem is you don't know exactly what the conditions were before. You know what they are after, but you don't quite know what they were before.

Of course, more than just thermal loads have to be considered when you're looking at your design and I tried here to sort of give you an idea of what all goes in to developing a design methodology. The main thing you want to be able to predict is you want to be able to predict the rock mass behavior under whatever conditions you anticipate. In order to do that, you have to know a lot of things and those things have to be then synthesized into some kind of an approach to design.

I'll give you a quick idea of what I mean by the requirements block there. This has been discussed several times yesterday. So, I won't belabor the point, but we did talk about SCP goals and how they were derived, what impacts they might have on the design and the development then of design criteria. So that you can check your design against these goals. That's not to say that these goals then can't change. These were tentative goals set in the SCP. As we get into the design process, it's certainly an iterative process to look at what are the best goals that help you satisfy the requirements, the upper level regulations.

Another thing that's often done in the design process is to try to account for the uncertainty in the in situ conditions by looking at indexing the rock quality or developing some kind of an index measure of rock quality. Maybe I'll put this up here. What I'm doing is I'm going to talk a little bit about some of these bubbles here. This is just a couple of different index systems that are used quite often in tunneling and underground excavations; the Nick Barton NGI System, the rock mass rating system. And, you can see that to get a single measure, a number that represents the rock quality, you look at a variety of things that help you sort of judge what the goodness or badness of a particular rock mass might be. The interesting thing in the NGI

System is this stress reduction factor and we kind of keyed on that a little bit just to see whether we would alter that system in order to try to account for the thermal stresses, as well as the in situ stresses. And, what we did was we tried to put together a--this is a plot from some of Nick Barton's work and basically to look at what kind of support measures might be needed in an excavation of a given size versus the quality of the rock.

So, what we did was to try to superimpose on that just to give you an idea what kind of a region we figure we're working in. Based on preliminary looks at the core that's been recovered from the TSw1, we have a range of rock quality that's sort of this span (indicating) and the spans of the repository room look something like that (indicating). But, this block is actually bigger than the calculated rock mass quality would be at room temperature because in this factor here we tried to include what the impact of thermal stresses would be. So, what the impact is that really it tends to--if you go to higher thermal stresses--this one was done for 57 kW/acre. If you go to higher thermal stresses, say 80, it would tend to stretch the shaded area over into this region. It wouldn't have much impact over here because, as I mentioned before, what happens is the poorer quality rock can't support the thermal--or accommodates the thermal expansion much better because you can't generate the kind of stresses that you can at this end. So, what happens is you get a little more action at this end, not too much action at this end, as you adjust your thermal stresses. And, in that plot, really, the range of things that you're looking at doesn't change a whole lot.

A little bit better way to look at it might be from this plot and that is this is based--well, I have both indexes put here. This is based on some work by Hoek & Brown and we tried to shade in a square that sort of represents the ranges of values that we would be looking at, including the range of stresses at 57 kW/acre that you might experience around a drift. Again, if you did a specific case where you knew what the ranges of rock quality were, you would find that the shaded area or the area that you would be working in would not be a square, but it would be kind of pointed out at the top where you have the--the poorer quality rock simply cannot generate these kinds of stresses. So, this really part of the diagram kind of ends up getting missing or ends up being deleted because you can't have this poor of a quality a rock and generate these kinds of stresses. So, you get kind of an eclipse square there. But, anyway, it allows you to see that if you did increase or decrease the thermal loading what type or how you would affect the sort of the ranges of support that you might need in your repository.

As I said, that's kind of a look at the empirical point of view. If we go back to the numerical point of view, what are the uncertainties in being able to develop models that are going to be predictive of the kinds of loading and long term stability of the drifts that we need? What we've done is for the design analysis we've put together a scheme that basically allows us to do linear combinations of loads; in other words, we take into account all of these, combine them together, look at what the total load is on a drift, and then try to decide--of course, because of the thermal loads that's going to change as a function of time. The interesting part about it is that, of course, the stresses depend on time because the temperature is changing. They depend on the rock quality which, of course, depends on the drift location and the temperature history also depends on the drift location. So, you have to take all of those into account.

What I want to do is just go through a very quick example before my time runs out. This is just to give you an idea of what the coordinate system is and give you a comparison. If we look at the thermal stresses, this is at 100 years for the case of 57 kW/acre and we've categorized the rock mass quality into five broad categories from very poor to very good. If you look back at those charts, you'll see what those translate to. And, you can see that the stresses at the same point in time at the same place, depending on what the rock quality is, can be quite different. So, you have to take into account the uncertainties in the geologic structure and the uncertainties in the thermal history.

To look at it from a slightly different point of view, this should be a midpanel drift. This is as a function of time, and if you look at how the thermal stresses change as a function of time, this last one is intended to be a comparison between the 57kW case and the 80kW case. So, you can see what the stress impact of the thermal stresses would be locally.

What we do then is to take that information from those drift calculations and try to exercise that through a number of different codes to look at what the impact is as far as stability and joint structure. This is a calculation from the same configuration that I showed you earlier, what we call the benchmark calculation. This basically gives the regions at 100 years around the drift in which some joint slip has occurred. And, this is based on the calculations from a continuum model in which the criteria for joint slip is incorporated into the calculations and so we can figure out which one of these cells you saw some joint slip. It's interesting that the slip, while it appears to radiate quite far from the drift, actually the slip is occurring on hori- zontal joints, not on the vertical joints. The vertical structure is much finer than the horizontal structure in this case. I think there's about 10--we put in 10 joints per meter vertically and about one joint per meter horizontally. But, the predominant prediction of slip is on the horizontal joints primarily because at 100 years, you recall, you have a very high horizontal stress. That locks up the vertical joints. They don't do much. They just close down.

DR. CORDING: Larry, in that diagram, that doesn't mean that that slip continues. It may mean you've reached a peak and there might be a slight amount of--

DR. COSTIN: It means sometime in the past 100 years, a joint reached the condition where it could slip in that cell.

DR. CORDING: And, it might have moved a few tenths of a millimeter and relieved itself.

DR. COSTIN: Tenths of a millimeter, right. It makes no prediction about what the change in permeability might be. That, you have to look at the actual magnitudes of the slip. Then, you have to go back again to your experimental data and look at the roughness of those joints and how that might impact the changes in permeability.

This is just the same sort of calculation using a different criteria, a more classic Drucker-Prager-Young criteria and to look at the zones that at the same time satisfy that criteria. So, by using these kind of plots, you can get an estimation of what the damage zone is or what the zone of impact around these drifts are. What you want to do according to the criteria that were discussed yesterday or the goals was to limit that to some reasonable amount that can be contained within the near-drift or the near-field area. What you don't want to do is to create long preferential pathways or extend fractures away from the drift in such an extent that you might connect to them with upper horizons or lower horizons to allow water to flow much more quickly. You want to limit that to the near-drift field.

Next, I'm going to skip that and let's just summarize. From the geomechanics point of view, there really are some advantages to both higher and lower thermal loading. Certainly, the lower thermal loading or no thermal loading would make the designer's job a lot easier. He doesn't have to worry about developing more complex models, he doesn't have to worry about additional analysis, and he doesn't have to worry about more confirmation testing. So, it would make his job a little easier. However, some of the advantages are that, as I mentioned before, because of the volumetric expansion you can, in fact, tighten up the joint structure and make the rock more competent at higher temperatures.

The higher thermal loads potentially could decrease the fracture permeability around these drifts. And, so water trying to percolate away from the drifts would have a harder time or would have to go through the matrix rather than the fractures.

DR. LANGMUIR: Larry, does this also apply to heated waste that's producing vapor, steam under pressure? Is this going to--what does that do to you?

DR. COSTIN: That, you'll have to ask Tom Buscheck.

DR. DOMENICO: Larry?

DR. COSTIN: Yes?

DR. DOMENICO: Is Tom going to answer your question now or later?

DR. COSTIN: Probably later.

DR. DOMENICO: The thermal expansion that's going to close these fractures, the thermal expansion of the rock mass is going to be controlled by the thermal expansion of the individual minerals making up the tuff which may be ortho-clase, pyroxens, what is in there. At the temperatures you're considering, how do you know you have not passed the elastic regime and are basically having an inelastic thermal expansion which I think would not certainly increase the rock quality, but would decrease it? Because I don't see any resistance to thermal expansion into open fractures. It's not going to be working against any stress. And, at those temperatures, I would start to suspect that a lot of the thermal expansion you're seeing, the bulk is a result of inelastic response of the individual minerals. Have you looked at that, at all?

DR. COSTIN: There's been a number of tests done to measure thermal expansion on small cores. To my knowledge, it shows that that strain is virtually entirely recoverable, at least at the temperatures I showed on that plot up to about 300°. Now, when you get above 200 or so, then you begin to get phase changes which are nonrecoverable. That's why the thing begins to spread out. So, I think if you keep temperatures below about 200°, it's virtually all recover- able. If you get above that where you begin to see major volumetric expansion due to phase changes, then you have a problem in recycling.

DR. DOMENICO: Is it phase changes or inelastic behavior or it would probably be both?

DR. COSTIN: Probably both. There is some inelastic behavior everywhere, but from what I understand or what I have read from the people that have done the tests--and Fran Nimick would be the one to really ask about this--

DR. DOMENICO: Well, I didn't realize you were talking about 250°C into this entity of the--

DR. COSTIN: That is right and that is why the near-borehole region was limited to or those temperature goals were put on the near borehole region was to limit the amount of thermal expansion that you could get and not get into the region where you get very large volumetric strains due to phase changes.

DR. ALLEN: But, you feel confident that this strain is recoverable even at elevated temperatures over hundreds of years?

DR. COSTIN: That, we don't know. I wouldn't make any prediction about that. The only data that we have is from heating it up over a day or so and cooling it back down and trying to measure accurately the changes in dimension of a small sample.

DR. LANGMUIR: Larry, that last point you've got there behind your back, I have to look through you to see. Result in decrease in fracture permeability. What kinds of temperatures are we talking about there to make that happen?

DR. COSTIN: Oh, actually, if you look at--well, I don't have the right diagram. But, on that diagram where I showed you the difference in stresses near the heater, those kinds of stresses are--you only need to generate an additional stress of about 5 to 10MPa in order to significantly close down these fracture because the initial aperture is very small. So, if you generate thermal stresses that are on the order of 5 to 10MPa, you've significantly closed down those fractures. An additional stress of like 5MPa, you've virtually closed the fracture so that it doesn't show any additional deformation. Now this, admittedly, it not a very rough--this particular case is not a very rough fracture and it was exactly or is nearly exactly matched. If you have some mismatch and a lot of asperities, you can get a lot more compliant system, but typically if these fractures are very tight, closely spaced, this is the kind of thing you're going to see. They're going to have an aperture on the order of 25/50 microns and that's going to disappear as you apply a very small load very quickly.

DR. DEERE: There will be a set. There's no doubt of it when we're talking about the rock mass versus just the rock itself because the joints themselves are not going to behave completely elastically. Almost invariably there is a set.

DR. COSTIN: Yeah.

DR. DEERE: And, therefore, a permanent decrease in the aperture openings.

DR. COSTIN: This it the neat thing is that once you heat it up and you smash these things together, when it cools off again in the long-time, those joints may not open up again. In fact, probably will not open up nearly to the degree that they were initially. That's right. You get a--

DR. DEERE: And, I think this is where the radial jack which test a long zone to literally thousands of tons and then you have access in your boreholes to do any kind of testing that you want.

DR. COSTIN: You can do changes in permeability of joints into the rock mass.

DR. DEERE: At different depths and it has possibilities.

DR. COSTIN: Let me go to two more quick summaries and that is that's not to say that there aren't some problems that become more significant as you increase the thermal load. And, I've tried to list some of those. The primary one, of course, is that the higher thermal loads, higher stresses add some uncertainty and complexity to the design process and the fact that you need to do more testing, more confirmation, that you can demonstrate that you know what you're doing.

Finally, one of the questions was how do we see resolving these problems? We try to demonstrate that we can incorporate the thermal load in what we're doing and we can get a handle on it. The design methodology or the philosophy of doing a design is essentially independent of what the thermal load is. So, any time you can say, well, let's go through the same process at a lower or higher thermal load, the process that's been laid out for doing the design is essentially independent of that. What you have to add in is this part (indicating.) If you look at the magnitude of the stresses and the orientation of the stresses, certainly those magnitudes and orientations are well within known practice. I mean, there have been excavations constructed with those kinds of stresses. The thing that's unique is in those cases they don't change with time and they don't change orientations with time.

The joint slip and fracture propagation, at least from the preliminary calculations we have done, are not expected to extend beyond the near drift field which is one of the goals that we've looked at in order to prevent creating preferential pathways. None of the cases, even up to 80kW, that we've looked at is this the case. We don't anticipate creating long fracture networks from drifts because of the high thermal loads.

And, I'll leave it at that and answer any additional questions.

DR. DEERE: Well, just a comment. It would sort of seem to me there's just about a net--it doesn't make much difference one way or the other.

DR. COSTIN: Well, I would wait until you see the next few presentations before you make up your mind on that.

DR. DEERE: On the basis of what you've said.

DR. COSTIN: Right. But, there are some implications, as I think you'll see from Tom Blejwas' talk about the degree of testing that you have to do at the higher thermal loads in order to validate the models. There's certainly--from a technical point of view, it doesn't matter. You can handle high thermal loads. You can equally handle no thermal loads. That's not a problem. The problem is how much more additional work you need to do to handle the higher loads.

DR. DEERE: I think that's correct, yes.

DR. NORTH: We have a question from Russ McFarland.

MR. MCFARLAND: Yes. Larry, you make no distinction in the presentation between preclosure and post-closure. Would you comment on that?

DR. COSTIN: Okay. I tried to in one of the first slides to make a slight distinction in that really the impact of this rotation of the stress field is primarily on the preclosure when you're trying to keep drifts stable, open, usable for a 100 year lifetime. And, during that 100 year lifetime, as you saw from Eric's--those panel access drifts, et cetera--are going to be heated, not nearly to the degree that you may have thought previously, but they are going to be heated, they do need to remain open, stable, usable for retrievability. However, the post-closure issue again is dealing with are you creating preferential pathways? Are you satisfying the SCP goals not to do that? To contain the damage that's done around these drifts in the preclosure period before--or even after they're backfilled and closed--that you're not going to end up creating large fracture networks or preferential pathways or pathways of enhanced permeability.

MR. MCFARLAND: Thank you.

DR. DOMENICO: Larry, I see some things that you referred to as 80kW/acre. I saw that on one of your slides. I think the design is 57, but nobody really knows how much available space is down there and it may come in at higher than 57. Did I hear you say correctly that you think that 200°C might be an upper limit that you can take without perhaps destroying the integrity or hindering the integrity of the rock?

DR. COSTIN: Well, you can't simply name a temperature. I mean, you can take 200° in a very small volume and it's no problem. If you get 200° or 250° in a very large volume, then it becomes a problem. The 200 or 250° limit, you don't get those kinds of temperatures; even in the 80kW case, you don't get those kinds of temperatures over a very large volume. You do get them in the near-borehole region, but again one of the design goals was to limit the near-field and the near-borehole field to 250 or 275, I forget which. But, to keep that into a region where--the primary reason for that goal was borehole stability so that you could retrieve the canisters. If you maintained that, then the temperatures away from there are not going to see that kind of temperature over a very large volume.

DR. NORTH: Question from Ed Cording.

DR. CORDING: Yes. In going through these analyses and you can come up with very high stresses around the opening in some cases and it seems that we certainly need to think about what that really means in terms of behavior. And, I know that some of the tests that you did in G-Tunnel gave you some feeling for that, not as much as you'd like. And, there's other information on high stress environments not due to heating that show that you certainly can handle and build openings in which the stresses at the boundary, calculated at least, exceed the strength of the material. It's not so much you resist those stresses, but you go in and provide the support that just holds whatever you have up there in place or you allow some falls if that's, you know, a situation that you can accept. And, so I think that's one of the things of these high stresses at the boundary that I think is something that you probably have been concluding from the work you've done. But, a lot of it is trying to understand just what the behavior will be, given these analyses, because our analyses don't do a very good job of telling us this is how the rock is really going to look.

DR. COSTIN: That's why you need the--or why it's really essential to have the--if you're going to go to those kinds of thermal loadings, why it's essential to have those full scale field tests in order to validate your thinking.

DR. CORDING: And, to some extent that it's--to me, it doesn't seem to--in terms of stability, it doesn't relate to a site suitability question. It's a matter of just how you fine tune your design to take care of this.

DR. COSTIN: That's right.

DR. CORDING: One other point I was thinking about is you're talking about the closure of fractures and reduction of permeability and it would seem to me that when you're in an unconfined condition right at the boundary of the opening, in that fractured zone at the boundary, that heating will not even close fractures, it will tend to buckle and open frac- tures.

DR. COSTIN: Open. This is--

DR. CORDING: So, what you were describing was really a confined condition back in the mass.

DR. COSTIN: Back in the rock mass, right, in those big pillars in the--that those will tend to be tighter. You will get a zone of enhanced permeability around drifts. There's no question about that. But, it's very limited from our calculations.

DR. CORDING: Yeah. If I could ask you just one more on the drying effects, the effects of heating above 100°. You're focusing properly on the rock, the TSw2 rock at the repository level and I'm sure you've done tests on some of the nonwelded tuffs. Their behavior--the high water content material's behavior is much more dramatic when you dry above 100°. Isn't that your observation?

DR. COSTIN: Much more dramatic in one sense.

DR. CORDING: Much more dramatic in terms of changes in, for example, stiffness of a material or changes in strength. When you're taking out, say, material that has 20% water content as opposed to--

DR. COSTIN: The strength essentially varies or is a very couple function of the void volume. So, the higher void volume rock, the less welded tuffs have a much lower strength to begin with. And, I'm not real sure I can recall what the effect of drying in those tuffs, in the nonwelded tuffs, is. But, I don't think it's much more significant than what I showed here.

DR. CORDING: I recall some tests we ran where I think we dried it out just--just, you know, put it in an oven overnight or something. Dried it out and the strength was twice as stiff as it was when it was--

DR. COSTIN: That could be.

DR. CORDING: That's not necessarily--

DR. COSTIN: That may be a fact of an effective stress problem. If you have a very porous rock mass that's fully saturated and you test it, it's going to be weaker than what you dried out if you don't allow it to drain.

DR. NORTH: I think at this point we want to move on. Oh, Ellis?

DR. VERINK: I just have a very brief question. Since the temperature limit was associated with the stability, borehole stability, if there were a shift towards a drift emplacement, what influence would this--

DR. CANTLON: I would think with a drift emplacement, you could probably go to even higher canister loading because then you'd have the ability to ventilate it. You'd have the ability to--the coupling between the thermal load and the canister and the rock would be far less. You have to radiate this heat out. You would automatically spread it out over a much larger area to begin with and not deposit it in such a local area so you could go to significantly higher canister loadings.

DR. VERINK: Some advantage to that then?

DR. COSTIN: There would be some advantage to doing that.

DR. DEERE: A followup question. If you were going to make a drift emplacement, wouldn't you consider that a circular opening made by tunnel boring machine would probably enhance stability even farther? He shook his head yes.

(Laughter.)

DR. COSTIN: I think no matter how you emplace it, a circular hole would enhance stability.

DR. DEERE: Thank you.

(Laughter.)

MR. CLONINGER: Don, if there are no further questions from the Board, I'd like to introduce Thomas Buscheck, Lawrence Livermore National Laboratory, who will be discussing hydrogeologic uncertainties.

DR. BUSCHECK: Hi, I'm Tom Buscheck and I'd also like to point out that a lot of this work is also based on the work of my colleague, John Nitao who developed and enhanced the V-TOUGH Code which was based on the TOUGH Code that was developed by Karsten Preuss at LBL. This is my structure of my talk. Where is that pointer? Do we have it? Okay. The structure of my talk is that I'm going to first give a brief overview of the Yucca Mountain hydrology, particularly those features of Yucca Mountain which are most critical to the repository performance. We're going to briefly describe hydrothermal flow phenomena that occurs at a variety of thermal loads at the repository horizon and then talk about the impact of that flow on the temperature profiles as a function of thermal load and then talk about the impact of the hydrothermal flow on temperature distribution; namely, the differences between a conduction only model and a model which accounts for hydrothermal flow effects. We're going to then talk about the impact of thermal load on the repository performance on the waste package environment and on the environment which may or may not lead to the transport of radionuclides to the water table. We're also then going to talk about the impact of thermal load on the significance of the various hydrologic uncertainties we've identified. I'd like to point out that a number of my slides that I may show as backup will appear in the appendix and don't necessarily appear in the order of the slides in the packages.

First of all, the most obvious important performance considerations depend on hydrology. You have to bring water to the waste packages to degrade them and also to dissolve the waste form. And, you also need to have liquid water to transport the radionuclides down to the water table.

An overview of the Yucca Mountain hydrology, this is a simplified conceptualization of the key features of the Yucca Mountain unsaturated zone at the repository block. The key consideration, we feel, is the impact of thermal load and fracture-dominated flow. At the last Review Board meeting, I talked at length about fracture-dominated flow and its impact on transport. First of all, I think most of us agree that if the entire mountain had matrix-dominated flow, we will not see a significant vertical displacement of radionuclides. So, therefore, we feel that if there is no fracture-dominated flow in the mountain, there are no hydrologic performance problems. However, there is field evidence that indicates that fracture-dominated flow can occur to considerable depth. And, moreover, as I just stated, fracture-dominated flow is the only credible mechanism to bring water to the waste packages and transport radionuclides to the water table.

I'm going to spend a lot of time today talking about boiling behavior, and then after boiling ceases, how persisting dry-out of the rock mass will greatly enhance the ability of the matrix to attenuate fracture flow. And, these effects, I believe, have a significant impact on the hydrologic uncertainties.

Basically, Yucca Mountain is comprised of two relatively distinct groups of matrix properties. If we just for the time being assume that there's no differences in fracture properties, that somehow there are preferential fracture pathways through the mountain, let's just talk about the impact that the matrix properties have on a flow down of preferential fracture pathway. We have the welded units which have a W behind them which comprise about 85% probably of the repository block. The Tiva Canyon, the Topopah Springs unit, the lower Topopah Springs unit which is the vitrophyre, these units have very low matrix permeability, as does the zeolitized nonwelded Calico Hills. They all have about the same very low matrix permeability which results in minimizing the impact on imbibition on retarding fracture flow. Therefore, in these units, the likelihood of fracture-dominated flow, given the presence of fractures and an adequate source of water, is significant.

Now, in the nonwelded vitric units, there is a much higher matrix permeability there and because of that imbibition is much stronger. It's much more likely to dominate fracture-dominated flow and will probably result in very large lateral flow which may preclude most fracture dominated flow from even getting to the repository horizon. The vitric--

DR. DEERE: A question?

DR. BUSCHECK: Yes?

DR. DEERE: Tom, I recall this similar diagram you showed before and I was a little confused by your high permeability and low permeability.

DR. BUSCHECK: I'm talking high permeability with respect to the matrix. I'm assuming--

DR. DEERE: I think that's the key, yes.

DR. BUSCHECK: If you were looking at the bulk permeability, the bulk permeability is dominated even in the high matrix permeability units by the fracture permeability.

DR. DEERE: Right.

DR. BUSCHECK: And, right now, I'm assuming that all things are equal. We're not considering differential fracture properties and we're focusing on the matrix permeability. I think the most important unit in the Calico Hills--and, actually, this unit could be much thicker than I've depicted this. Dave Bish has given me an update on this. This can actually be 100 meters thick or more rather than the 5 meters I'm showing here. But, it does not extend over the entire main repository block. This unit is, I think, one of the primary barriers of physical retardation below the repository.

Quickly, some of the evidence which indicates fracture-dominated flow can get to some depth. It comes about by looking at the saturation distribution. Now, we're looking again at the same cross-section. We're using the same Klavetter and Peters data that I based all my subsequent calculations on and it is just--it's a representative cross-section, but it does not rigorously pertain to the entire mountain, but it's good for a starting point.

Going from the ground surface to the water table, we have imposed several different effective recharge fluxes. The general nominal flux which seems to--and then, what I've also shown is the mean value of saturation in the RIB and one standard deviation away from that mean value. A number of analyses have indicated that the best correspondence with most of the RIB data is obtained at a zero-effective moisture flux through the mountain. And, as we can see in the low matrix permeability units, we have pretty good agreement between zero flux and the existing data. However, there's a significant problem; that being in the high matrix perme-ability nonwelded vitric Paintbrush tuff, this unit here (indicating), and the nonwelded vitric Calico Hills, the existing data is substantially wetter than what you would predict with a steady-state matrix-dominated flow model. So, therefore, we feel the adequate explanation is there is some occasional episodic flow to depth which is recharging these units, but due to the low matrix permeability in these units, does not have adequate time to be reflected in the saturation profile in those units.

DR. DOMENICO: Tom, what's real and what's predicted there?

DR. BUSCHECK: Pardon me?

DR. DOMENICO: What's real and what's predicted?

DR. BUSCHECK: What's predicted are these three blue lines. What's the existing real data is, as I said, this is the main (indicating) and that's the standard deviation. So, in the low permeability units, it appears the effect is as though there's a zero moisture flux to the low permeability units. That's what the data indicates. In the high permeability units, it's as though there is some significant positive flux which has minimal impact on the low permeability units.

DR. WILLIAMS: Tom, could you describe the data that you're referring to?

DR. BUSCHECK: The date? The Klavetter and Peters data? That data--

DR. WILLIAMS: The data about which the standard deviation is calculated?

DR. BUSCHECK: You know, that data is in the RIB. It was a--I cannot vouch for, you know, whether it was contaminated by drilling fluids or whatever. It's what we have currently.

DR. WILLIAMS: I just want to point out that there are some problems with the data that may not be--it may change as more data are collected.

DR. BUSCHECK: Correct. I don't want to unequivocally say that we're going to find this wet of condition in the vitric tuff. It may not be the case. But, even so, my feeling is that we need to address--the analysis needs to address the most problematic performance aspects of the mountain, irrespective of whether we have conclusive evidence whether those conditions persist today.

DR. WILLIAMS: Well, I'm just trying to point out you should be realistic about the data that you're comparing the predictions to. That's all.

DR. BUSCHECK: Right. But, as we'll see in a little--what we have done in our subsequent calculations is that we have utilized these three different saturation profiles as initial conditions for our hydrothermal calculations. This particular condition gives rise to the 96% repository saturation which I feel is very unexpected. Nonetheless, we ran calculations out here to see how robust high thermal loads were with respect to initial saturation.

Basically, with respect to fracture-dominated flow, there are three basic mechanisms which mitigate the impact of fracture flow. The obvious one is that the fracture networks are not connected vertically to the water table. Another effect is that if you have a connected pathway to the water table, there will be a lot of tributary fractures. So, there's liquid-phase dispersion and tributary fractures which will not make it to the repository horizon or to the water table. The third area is fracture-matrix interaction, the ability of the matrix to attenuate fracture flow. For low areal power densities, we'll only have matrix imbibition tending to retard fracture flow. At higher APDs, we'll get boiling effects and we also find that due to the persistent rock dry-out that imbibition is also enhanced for quite a long period of time.

Slow me down if I get too fast.

DR. DEERE: Maybe 10% reduction.

DR. BUSCHECK: Okay. Anyway, now each of these sections I have, I have a conclusion to the front. So, if you want to review it afterwards, I put all the main points up front.

For hydrothermal flow, we have found that unsaturated fracture tuff promotes rock dry-out by boiling. And, I'll go over these one by one. This is just the conceptual to show that whether it's borehole of drift emplacement that if we have a fracture tuff rock mass that boiling is facilitated. What happens is that we found in G-Tunnel experiments that boiling preferentially occurs along fractures and then progresses into the matrix. And then, once the water vapor reaches the fracture system, it moves radially or spherically away from the heat source until it reaches condensation temperatures, whereupon it condenses and it could either drain back towards the boiling zone and reflux or, if it's below the boiling zone, we've found it very quickly drains away from the boiling zone.

This is not in your package, but I wanted to show the impact that we have to have fractures in the unsaturated zone to promote rock dry-out. What I'm showing here is at 60 years, we have the drift emplacement calculation which pertains also to vertical emplacement. About three to five years after vertical emplacement, the boiling zones between the packages coalesce and it acts as though it's a line source. So, this pertains to that calculation, as well. What we're showing here in red (indicating) is completely dried-out and the very dark blue would be 100% saturation. What we find is that there are no fractures, that the bulk permeability is that of the matrix itself, 2 x 10^{-18}, that we get a very small region of dry-out at 60 years.

The nominal case that I've considered to the effect if we had 300 micron fractures per meter, but more importantly the bulk permeability is 2-1/2 x 10^{-13}, which is five orders of magnitude more permeable than when you have no fractures, we find that the--and this is the nominal boiling isotherm at this altitude--we find that the dry-out zone follows the boiling point isotherm very closely. So, there's no throttling of dry-out by virtue of flow effects with the fractured rock matrix.

In this example, we've increased the bulk perme- ability by another 2-1/2 orders of magnitude. We have found that the dry-out volume is largely unaffected, that it's still following the boiling point isotherm. In fact, the boiling point isotherm is more compact for reasons that I can't explain right now, but basically what is happening is that we have enhanced heat flow by virtue of some additional natural convective effects which tends to keep the boiling isotherm closer. However, if you look at the impact on dry-out conditions--and what I'm plotting here in time out to 10,000 years is the near-field environment saturation. The initial saturation is assumed to be 69% which is actually 4% higher than the existing RIB data. What we find is with no fractures that we get minimal dry-out and essentially the rock remains at the initial saturation. Regardless of whether we go to this particular scenario or increase the bulk permeability by another 2-1/2 orders of magnitude, the dry-out behavior is largely insensitive. So, it's being controlled by the thermal properties which is the second point of my summary.

The volume of the dry-out zone is dominated by the thermal load and the thermal properties given adequate fracture spacing.

DR. LANGMUIR: Tom, we don't have a left hand overhead. I presume you can get that for us?

DR. BUSCHECK: Pardon me?

DR. LANGMUIR: We don't have that left--

DR. BUSCHECK: I can give that to you. The point that I also want to make is that the existing, though it's limited, bulk permeability data for the Topopah Springs tuff actually pertains to this case. So, we're well above the threshold for significant dry-out. This also shows that according to what Larry is talking about that if we could have a very substantial change in the bulk permeability and not have a significant change in the dry-out. It's being dominated by the thermal properties and the thermal load. We would have to reduce, relative to the current characterization, probably on the order of seven orders of magnitude in bulk permeability before we would start to pick up a reduction in the amount of dry-out.

The other thing that we observed in G-Tunnel was that--I'd like to point out that our models have been validated to a reasonable extent. Our temperature predictions in G-Tunnel using the same modeling approach that I'm using in this talk, we got a very good agreement with the temperature profiles that we observed. We also got an outstanding agreement with the dry-out volume in time and in space. What we didn't predict was using the equivalent continuum model. The equivalent continuum model assumes that as soon as the flow condenses, it is imbibed in the matrix and then its condensate drainage is confined by the very tight matrix permeability. In reality, that flow will be in the fractures out of equilibrium with the matrix and will tend to shut off the sides. And, that's what I'm trying to show here. That once the vapor reaches the fracture system, it moves down thermal gradient, through the fracture system, out to where it condenses, and drains vertically downward under gravity. And then, subsequent refluxing cycles will be radially away from the heat source and you can see how this water is eventually shut off the side of the boiling zone. And, in G-Tunnel, we saw no increase in saturation out here in what we expected to be the saturation halo. We didn't see any below, as well, because it's--that should be obvious; if the flow is in the fractures, then it's out of equilibrium. It will quickly drain from the boiling zone.

DR. DOMENICO: Tom, hold it. Wouldn't that be--that's black magic, man. Wouldn't that be a function of the orientation, the geometry of the fractures? I mean, if you're sending this vapor up these fractures and your refluxing comes down and goes back up and condenses, it seems to me that unless you have a very specific geometry, eventually when things cool off, all you've done is remove moisture from the matrix in the vicinity of the repository, put it up into the fractures, and it's available to percolate down through the repository one more time.

DR. BUSCHECK: Well, the fracture networks are indeed very tortuous and, you know, chaotic. In some cases, there may be perched conditions if there is no ability for a packet of water to shut off. But, our finding--you know, this is one of the reasons why we need to go underground and do substantial testing. This thing--

DR. DOMENICO: Yes, but this is a small scale experiment compared to heating on a repository scale, agreed?

DR. BUSCHECK: It is a small scale experiment.

DR. DOMENICO: I mean, this is a very small scale exper-iment.

DR. BUSCHECK: But, it was conducted--the heating period was conducted over 128 days. There was a long period of time during which some moisture could have appeared in the matrix and it did not, even above the boiling zone.

DR. DOMENICO: Well, what your scheme says is that you drive all the water out of the matrix into the fractures and then it's shed away from the repository and goes down along the sides and never percolates through the repository again. I think that's what you're suggesting here.

DR. BUSCHECK: Well, that is one--you know, that's one outside interpretation of that. We'll show in the calculations that we've done that we have in no way accounted for that favorable outcome. In fact, the equivalent continuum model that we've used assumes that the condensate stays above the boiling horizon. And, so we've conservatively--well, the model conservatively allows the water to stay up there and is continually refluxing. So, our calculations do not depend on this feature and, in fact, this feature does not have to be prevalent in order to get some of the favorable performance. But, I'm pointing it out that that it is something that we need to examine underground because I think it will--at least, half the condensate will drain freely from the system. I think that is not arguable. And, for a period of time, there is something between zero and 100% of the condensate above the boiling zone will drain, somewhere between those extremes. What I'm showing here is at 30, 60, and 100 years the progress of a dry-out zone. The main point here is that if condensate drainage and shedding were going to occur between rooms, it would occur sometime out to about 80 years, whereupon the coalescence would probably preclude that from occurring. However, as Eric Ryder showed, between panels, there's very persistent cool spots in the repository. So, there are areas for persistent shedding if, in fact, we find the phenomena to be of significance.

It's also interesting to note that I predict very similar progress of the dry-out zone that Eric predicts and I'll get into that a little bit later. Now, regarding the conservative aspects of our model, this is a reference 57kW/acre calculation. What we're plotting is from the ground surface down to the water table the dimensionless dry-out. Red pertains to like 0% saturation essentially; up to the very dark blue is 100%. White pertains to no saturation change. So, by dimensionless saturation, I mean change about initial saturation. So, out here in the far field (indicating), there is no change in the far-field saturations. We find at 1,000 years that about 100 meters of the rock above and below the repository horizon is dry with the exception being out at the very edge of the repository. This model, unlike Eric's model, has homogenized the impact of the waste package heating into a three kilometer diameter disk which has the same area as the Reference SCP/CD design. And, so we've smeared out the thermal load into this disk. We've used access symmetry in this model to do a lot of these subsequent calculations. And, also you were asking about perching yesterday. That by virtue of this conservative aspect of the model, this is plotting from zero to 100% the liquid saturation profile. This is not in the package. The blue curve is the initial curve; the red curve is at 1,000 years. You can see the 100 meters, 50 above and below the repository, is largely dry. The condensate which is above the boiling zone has not drained from the system and is being accounted for as we subsequently dry out the system. What we find is that for this scenario, boiling conditions ceased after 1800 years, but at 5,000 years, much of the repository remains near zero saturation. So, the time for rewetting the environment is much longer than the time it takes to cool down below boiling conditions.

We also found that using drift emplacement scenarios and varying spacing between drifts, the volume of the dry-out zone can be enhanced by alternative configurations. And, as I've stated, the numerical models used in this study are very conservative with respect to the predictions of the dry-out volume.

Now, we're moving to the next section where we discuss the impact of APD on the temperature profile. First of all, now we're again using the same profile going from the ground surface down to the water table and now we're plotting temperature from zero to 180°. What we find is that the temperature profile and the region of the boiling front is very much flattened at around 96° which is the nominal boiling temperature at that horizon. And, as you can see here (indicating), that pertains to this zone right here where we're getting a lot of refluxing occurring. Vertical vapor flow upward and downward imbibition of liquid flow back to the boiling front. The net dry-out rate is the net effect of vaporization minus return flow by imbibition. This model is also conservative because we use the drying curves that were obtained under drying conditions to represent wetting behavior. We've run experiments where we've shown that it over-predicts the rate of rewetting by a factor of 40 for welded tuffs. So, again, the net dry-out rate is not reflecting the true hysteretic nature of the characteristic curves.

Yes?

DR. DOMENICO: One thing on that diagram. You apparently are getting saturation at 250 meters below, is that correct, if you take a look at that?

DR. BUSCHECK: At this step (indicating)? We are actually elevating the saturation.

DR. DOMENICO: No, no. 250, go to 250.

DR. BUSCHECK: Below?

DR. DOMENICO: Follow your red dotted line. There you are. Okay?

DR. BUSCHECK: Yes.

DR. DOMENICO: How does that happen? There's your original. Your original says that you were originally 60 or 80% saturation.

DR. BUSCHECK: Correct. So, what was happening is--

DR. DOMENICO: So, you drove it somehow out of the fractures back into the matrix?

DR. BUSCHECK: Well, this water is also partly in the fractures above about 97%. In the equivalent continuum model, there is water freely draining in the fractures. So, the matrix in this region is filled and some of the water is indeed draining back through the fractures.

DR. DOMENICO: Preheating, I see the saturation of 80% which meant all the water was in the matrix. Preheating.

DR. BUSCHECK: Correct. Correct.

DR. DOMENICO: After heating somehow, we drove some more water into the matrix and made it almost saturated.

DR. BUSCHECK: This volume here (indicating), half of that volume has been draining to the water table. Half of that volume is also occurring up here (indicating).

DR. DOMENICO: And, it's actually being sucked into the matrix?

DR. BUSCHECK: It has been, yes, for--

DR. DOMENICO: Out of the fractures into the matrix?

DR. BUSCHECK: Yes, but because the saturation is being driven above the critical saturation the equivalent continuum model predicts for fracture flow, this water is draining back to the boiling front in fractures.

Okay. Another thing to observe here is that we're showing profiles at 100, 300, 1,000, and 5,000 years. It takes about 300 years before any thermal disturbance reaches the ground surface. We can see that at 1,000 years we still get persisting boiling conditions occurring out here about 50 meters above and down here in the base of the Topopah Springs unit.

With respect to thermal loading, we find that for a given fuel age that the temperature rise is proportional to the areal power density and this is again for average conditions at the repository. And, what I've plotted here is the nominal boiling temperature at the repository horizon. We find that for 100kW/acre we get about 4200 years of boiling conditions within the inner two-thirds of the repository. Again, this is time from zero to 10,000 years; the temperature from zero to 180.

This is a radial profile of temperature from the center of the repository out to the edge at 1500 meters at various times. We find that the temperatures within roughly the inner two-thirds of the repository are at roughly uniform temperature and that the edge effects pertain out at the outer third of the repository. We also find that calculations that I'll show in a moment on the drift scale model, which accounts for the local thermal load distribution unlike the large scale model, that we get very similar predictions of temperatures as those predicted by the large scale model within the inner two-thirds of the large scale model.

The impact of hydrothermal flow on the temperature field. Yesterday, we were hearing about the impact of convection, hydrothermal flow versus conduction. This is another plot that is not in your package, but I felt it was important to show. This is the temperature profile at 1,000 years. We can again see that boiling conditions prevail out in the rock here. Now, keep this in mind when I show this other plot. This is the plot. Effectively, the Nuselt number which is the ratio of a heat conductive flux to the total heat flux. So, a value of 1 means the heat flow is completely dominated by conduction. The yellow line is that one line. And, as we can see here (indicating), this is where the two phase boiling effects are occurring, right here. Out beyond the boiling front, we find that heat flow is dominated by heat conduction because the ratio is very close to 1. Within the boiling refluxing zone, we get to the point where convection is dominating flow within this relatively narrow band. Inside the boiling zone where the rock is completely dried out, it is again dominated by heat conduction as it is below here (indicating). We find there's a positive component of natural convection going upwards. Therefore, the conductive flux is less than 100%. But, below the repository horizon because natural convection is going against thermal conduction which is going down, we find that thermal convection is working against heat flow. So, the ratio of the conductive of the total flux is greater than 1. And, again, going down in the water table is largely dominated by heat conduction.

This is a plot at 100 years comparing a heat conduction model which assumed TSw2 properties throughout the mountain. I used the exact same properties that Eric used in his model. Again, we're plotting temperature from zero to 180. The heat conduction model is in purple and then I've shown a couple cases of the hydrothermal model at various recharge fluxes. What we find is, is that in the very near-field, the conduction model conservatively predicts high temperatures because it doesn't account for boiling effects. It also conservatively under-predicts the boiling point isotherm. So, it under-predicts the extent to which the dry-out zone is extending into the rock. It also under-predicts the temperatures at the top of the Calico Hills; the reason being is that there's hot condensate flow in the hydrothermal model which is adding to the total heat flow of the Calico Hills. So, there is some under-prediction in the conduction model versus the hydrothermal model at that point. But, with regards to the other two points, the conduction model gives conservative performance calculations.

So, therefore, summarizing also some of the other things we have noticed, that temperatures in the vicinity of the waste package decreases with increasing recharge flux. The impact of hydrothermal flow increases with the initial saturation which is a function of the recharge flux. Boiling also results in lower temperatures in the vicinity of waste packages, as we see here (indicating). The heat conduction model again yields conservatively high temperatures in the very near-field and conservatively low temperatures with respect to the extent of the boiling zone.

Am I going too fast?

DR. DEERE: Fine.

DR. BUSCHECK: Okay. This is, I think, the most important section, the impact on repository performance. First of all, what we're plotting here again throughout the 10,000 years is the total dry-out volume of liquid water versus time for 30 year old fuel and a nominal recharge flux. We're plotting it for 20, 36, 57, 80, and 100kW/acre. One thing that we've noticed is that for 20 and 30, there is no significant dry-out benefits, at all. And, somewhere between 36 and 57, there's a threshold for significant dry-out benefits to pick up. If you divide this volume by the area of the repository, you find, first of all, that the peak dry-out volume peaks around 800 years. Therefore, the net condensation rate which is the first derivative of this curve is positive out to about 800 years. Without the 800 years, this volume of water pertains to 8 meters of water for this case, 15 meters of water for this case, and 22 for this (indicating.) So, therefore, if you were to average the net condensation rate over time, that pertains to an average flux of 1 centimeter per year, 2 centimeters per year, and 3 centimeters per year which is much, much higher than any effective moisture recharge flux currently considered for the Yucca Mountain horizon. So, therefore, the hydrothermal flow, the net condensate flow, grossly dominates the natural system in terms of the natural flux through the mountain.

The other thing to consider is the fact that this is net condensate flux, that we have a lot of recirculation occurring. So, the actual condensate flux is going to be much higher than the net flux. So, if you consider the impact of episodic pulses due to rainfall or snow melt, my feeling is that it has a relatively small impact on the overall amount of water that the boiling zone has to accom- modate to remain dry.

We can also derive some increased dry-out benefits by using the same initial APD of 57kW/acre, but by using 60 year old fuel and packing it closer together. In this case, we've almost doubled the dry-out benefits with minimal impact of the waste package temperatures which I could show in a minute if there's interest.

For a high areal power density of 1,000 years-- okay, going back to this other plot, you can also calculate an equivalent volume or height of dried out rock. For this case, it pertains to 100 meters of dried out rock, for this case, it pertains to about 200 meters, and for this case it pertains to about 300 meters of dried out rock (indicating) at the maximum time or maximum dry-out. But, you can see that the rewetting of the rock occurs very, very slowly. So, that dry-out volume will persist for a long period of time.

I'm showing you an example of a high APD. At 1,000 years as we get about 250 meters, which at maximum time it extends to 300 meters, you can see the repository horizon here (indicating). The edge of the repository is dried out to almost zero saturation. Again, the condensate zone, we're seeing on a large scale some of the shedding, but it's very much retarded by the low matrix permeability. And, this condensate has been continuously shedding down to the water table for some time.

Now, for this example (indicating), the boiling stopped after 4200 years; yet, at 5,000 years, we have still a substantial volume of dried out rock. What I'm plotting again here is liquid saturation from zero to 100%. Again, the blue curve is the initial saturation; red is net dry-out at 5,000 years; blue is net rewetting. So, you can see that this calculation is still accommodating for a large thermally perched region of wetted rock.

DR. DOMENICO: I've got to ask something about that again. This is a continuum model, correct?

DR. BUSCHECK: Correct, and I'll explain why it's relatively reasonable.

DR. DOMENICO: Okay. So, that is why you're getting that saturation above. If you had a fracture model, the water you're driving up would not necessarily go into the matrix--

DR. BUSCHECK: I think much of it would have shed through the repository, through the cold spots.

DR. DOMENICO: Yes, but still that high saturation there above is an artifact of the model?

DR. BUSCHECK: Correct, and it's--

DR. DOMENICO: Right, it's totally an artifact of a continuum model.

DR. BUSCHECK: It is and, as I stated, it's a conservative artifact. It's conservatively retarding the net dry-out and it's also conservatively over-predicting the rewetting of the repository horizon.

DR. DOMENICO: Well, I thought if it would say in the fractures instead of doing that once it dries out, then it would start to rain on the repository.

DR. BUSCHECK: No, it doesn't stay in the fractures. That was one of the many points, probably too many points, of June's talk is that flow in the fracture is imbibed in the matrix within a day and so it will not--unless the saturation in the matrix is near 100%, it will not--

DR. DOMENICO: It then will be rejecting it and it will be staying in the fractures. Somebody ought to calculate just what sort of volume of water you're going to move.

DR. BUSCHECK: We have calculated it. I was mentioning that.

DR. DOMENICO: Okay.

DR. BUSCHECK: And, the fact that John Nitao is busily, as I've talked, working on a new equivalent continuum model which accounts for nonequilibrium fracture matrix and I think we're going to have a very novel new scheme in the near future.

DR. DEERE: So, we shouldn't take notes on this one.

(Laughter.)

DR. BUSCHECK: No, you--well, the picture is going to become--we're going to find that the picture--the dry-out predictions are going to be much--again, I'm saying that when we account for those effects, we're going to probably find that the dry-out volume extends further and lasts longer in time.

Another point that I had made earlier when Max last heard my talk was that the Calico Hills, much of the Calico Hills, is well below initial saturation. This is going to impact transport through the Calico Hills. We've had boiling conditions along in the Calico Hills. So, if you're going to arbitrarily apply temperature limits in the Calico Hills, you may not be able to take advantage of this favorable--what I consider to be favorable flow performance. Drying out the Calico Hills will do a couple things. Even after boiling stops, it will continue to attenuate fracture flow because of increased matrix imbibition. Also, the more likely scenario where you have disconnected fracture networks what you have to do in order to get matrix or fracture flows to bridge through the matrix, you have to reach 100% saturation in the matrix to bridge between two discontinuous fractures. If you've driven the water down to near zero saturation, that's much less likely to occur. So, we've mitigated the impact of slightly discontinuous fractures from transporting radio-nuclides through the water table by virtue of that effect.

Another thing is again because we're grossly over-predicting the rewetting behavior, we're having water in the saturated zone as being pumped up by imbibition back to the dry-out zone. When we get hysteretic data--and, in fact, Alan Flint has some for the nonwelded tuffs which I intend to use in the very near future to implement in the model--we'll probably find this net dry-out will extend much further into the Calico Hills.

As far as the waste package environment, I think it's very worthwhile to point out that dry steam boiling conditions are going to persist for high APDs for thousands of years. Again, for the 100kW/acre and the inner two-thirds of the repository, dry steam boiling conditions persist for 4200 years, and for 80 they're persisting for about 3,000 years, and for 57 about 1800 years. For the low APDs, we get no persisting dry steam conditions.

You can also nearly double the length of these dry steam conditions by simply going from 30 to 60 year old fuel using the same initial APD and you almost double the duration of the dry-out period. And, that doubling of the dry out is obtained at very minimal impact on the waste package temperature. This is in your appendix. What I'm plotting here now is instead of the near-field rock temperature, I'm plotting the waste package temperature for drift emplacement. Here's for the 30 year old (indicating). It peaks at about 135°. This is the waste package temperature. For 60 year old fuel, it's peaking at around 150°. So, calculations that Marty Altenhofen and others have done at PNL for 10 year old fuel, 57kW/acre, which puts a lot less energy into the system, predict the package temperature around 300°. There is a big difference between borehole emplacement and drift emplacement with respect to package temperatures.

Okay. I'm sort of missing these bullets here. So, basically, what I've said here is the substantial boiling and dry-out benefits can occur for high APDs. The dry steam boiling conditions will persist for thousands of years. Rock dry-out benefits will continue to persist for even tens of thousands of years after boiling has stopped. And, for drift emplacement, we get substantial dry-out benefits with less impact than waste package temperatures. And, this point, I'm going to save for the uncertainty section.

Then, the next point is the impact of the ground surface. For 100kW/acre, we actually saw that the surface heat flux never actually exceeded 1.1kW/m^2. And, a heat transfer coefficient of the ground surface of 1W/m per ◦C, I think is very conservative. So, therefore, the rise in temperature would be approximately 1◦C with that type of flux arriving at the ground surface.

Okay. I really appreciated the comment that Nils Rydell from Sweden made yesterday about credibility of performance modeling. And, basically, I believe what he said was relying on basic thermodynamic principles is probably a more credible means of demonstrating, you know, the reasonableness of our calculations than simply looking at the inherent properties of the natural barrier system. And, this is what I'm trying to show here. As I stated several times, fracture-dominated flow and low matrix permeability tuffs may promote, if sufficient water is present, fracture-dominated flow to substantial depth. And, the reason why that occurs is that the capillary or the wetting diffusivity of the rock matrix is so small. What we find, for instance, during a two day episodic pulse in a fracture, we can get on the order of 100 meters of penetration, get only 1-1/2 centimeters of penetration into the rock mass. Therefore, the volume of the matrix attenuating fracture flow is limited to within a couple centimeters of the matrix. So, this is how much of the matrix is available to attenuate fracture flow. If this fracture pulse is entering a boiling environment, you still have the benefits of this imbibition, but now what you have is you have evaporative cooling here. As this pulse evaporates, that will disturb the temperature field here (indicating). And, to disturb the volume of matrix of the rock that is disturbed is now proportional to the square root of a thermal diffusivity. And, a thermal diffusivity for the host rock is nearly three orders of magnitude higher than the capillary or wetting diffusivity of the matrix. Therefore, we have a much greater volume of rock matrix available to attenuate fracture flow. This is one of the reasons why I think the equivalent continuum model is more reasonable under high thermal loads than it is under low because this particular phenomena averages out the impact of this refluxing phenomena over a much greater rock mass.

Another point to be made is that the thermal properties fall within a very narrow band, maybe a factor of 2, over the tuffs. And, they also due to thermal effects and even mechanical effects are not likely to change by very much by virtue of thermal effects. So, therefore, this performance is not subject to much spatial or temporal variability. Whereas in this case, this can change substantially if geochemical effects cause a permeability skin along the fracture. That could mitigate the ability of the matrix to attenuate flow and there can be very substantial changes in the performance of this fracture-dominated flow event in a below boiling environment situation. So, therefore, the basic variability of this system versus this system (indicating), this system under initial conditions and changed conditions is much less likely to be affected by uncertainty and is also not so great--not at all dependent on just the basic uncertainty of the hydrologic properties. It's dependent on the thermal properties.

DR. LANGMUIR: Tom?

DR. BUSCHECK: Yes?

DR. LANGMUIR: Your refluxtion, though, is going to bring fluids back down into the system with evaporation and salinity increases and there will be all kinds of changes occurring--

DR. BUSCHECK: Yes, there are--we have some--you know, I'm not saying--

DR. LANGMUIR: Even in that steam system, you'll have changes.

DR. BUSCHECK: Yes. In the steam system, we do indeed, I think, have some homework to do in terms of experiments, in situ experiments, lab experiments, and in geochemical modeling. But, as I had shown earlier, that the threshold matrix bulk permeability--not matrix. The threshold bulk permeability of the fracture rock mass appears to be many orders of magnitude greater than that which would begin to throttle the effective rate of dry-out. So, you can have a substantial variation about what we may find in situ in terms of fracture healing and still obtain substantial dry-out benefits. But, we definitely need to address that.

Okay. Impact thermal load on hydrologic uncertainties. I guess to go through these, again as I had stated, even high APDs result in the minimal temperature disturbance at the ground surface and boiling conditions in rock dry-out effects greatly enhance the ability of the matrix to attenuate fracture flow.

Okay. Now, we're going to talk about the uncertainties. We feel that--and, this is an example of 20kW/acre, 30 year old fuel. It's very hard to pick up, but there is indeed a discernible amount of condensate flow which at 1,000 years has made it to the water table. There is an impact. There is some finite amount of boiling occurring. There's a finite amount of condensate drainage. You would have to go substantially less than 20kW/acre, I feel. Also, you would have to probably break up and use less than three intact assemblies per package or age the fuel or do a variety of things to try to completely obviate this particular effect. But, within the range of calculations, we found that these effects, you know--that the alteration of flow and transport properties could be significant and must be still considered even under very low thermal loads, loads at which we derive no net dry-out benefits.

This has been a question, a typical question, regarding far-field disturbance and that is what is the impact on the Calico Hills temperature? This particular calculation (indicating), the Calico Hills is only 60 meters from the repository. So, it's rather conservative. In many places, it's further away, but what we find again is that the temperature rises linear and APD, but we also find that for portions of the rock that some of the rock, even under 20kW/acre, is going to remain above 40°C for between 2,000 and 3,000 years. Dave Bish will comment on that, but you cannot dismiss the possibility that that temperature may impact the properties of that unit. So, it's hard to get away from those considerations.

However, we feel that if we can keep a boiling repository environment and one that remains dry for many tens of thousands of years thereafter, that the impact of the temperature effects on the Calico Hills is possibly much less important than it would be under low APDs where you get no attenuation of fracture flow by virtue of heating effects.

Here (indicating), I've done a lot of calculations regarding the sensitivity of the persistence of the dry-out zone to a very wide range of hydrologic conditions and I'm not going to present many examples. I'm going to present one example where we considered what I considered to be almost an absurdly upward bound case on saturation distribution. What did I do with that plot? Anyway, well, if you can just recall that I had a plot of the saturation distribution at Yucca Mountain and at this point, 132mm/year, that we had a 96% repository saturation which is very darn close to flooded. Oh, here it is. And, I consider this example at 96% initial repository saturation to be way out just to show the sensitivity, though, of the persistence of dry-out.

Something that I didn't point out that I wanted to is that a zero moisture flux through the repository horizon doesn't mean a zero liquid flux. Ivan Tsang made this observation, and we have since we've done these calculations, that due to the natural geothermal gradient, you've got vertical buoyancy-driven flow of vapor and then downward flow of liquid water where you've got a steady state dynamic system. The downward flow of liquid water at zero moisture flux was .04mm/year. If you use the Richards' equation model, you would predict that a .045mm/year liquid flux would give you a repository saturation of 85%, not 69. So, it's important to consider two phase effects. And, Pat, we are considering two phase effects even when we don't have a high thermal load and they are very significant.

DR. LANGMUIR: Tom, another phase. How about Carbon-14 CO_2 release in all of this?

MR. BUSCHECK: Yes, I feel--well, as far as some hypothetical travel time, under the natural geothermal gradient it's about 100 years. So, if you add a thermal load, to me, the critical issue is not trying to minimize the hypothetical travel time. The critical issue is maintaining a favorable waste package environment in which the release of C-14 is minimized. So, therefore, I feel under the higher thermal loads we are much less likely to degrade the waste packages and release C-14. But, even though the hypothetical travel time is shorter, the overall transport to the ground surface is going to be much less.

Okay. Getting back to this extreme examples of initial saturation, this was a nominal case of a recharge flux, a moisture flux of zero. We got 4200 years of persistent dry-out. When we go to this case where the repository is nearly flooded, we get about 3800 years of persistent dry-out. So, under high thermal loads--and, I don't consider that a very significant variability relative to other uncertainties. If you were to predict the occurrence of episodic fracture flow through the repository for this versus this scenario (indicating), you would find that you would have a much greater likelihood of occurrence of fracture flow to waste package, over this range of initial saturation, but in this case, we've--the high thermal loads, there's a minimal impact on the duration of that dry steam /boiling conditions. And, just to how you that Eric's model is conservatively low, when we considered the heat conduction model, the heat conduction model predicted about 3200 years of dry-out--of, excuse me, dry steam boiling conditions.

Some of the coupling phenomena that we need to consider. Two of the most important considerations are what is--well, this is sort of somewhat contrary to a lot of what's been said in the past. But, the vitric tuffs which sit atop of the zeolitized tuffs have not zeolitized by virtue of hot, you know, saturated conditions which occurred in geologic time. However, the drainage of hot water from condensate flow, as well as heating that water, that the upper vitric tuff may indeed zeolitize and that could indeed change the transport properties of that unit. Also, because of persistent condensate flow which should be of very low ionic strength and slightly low pH, that flow through the zeolitized Calico Hills may possibly significantly alter the flow and transport properties of any preferential fracture pathways. So, these are two important considerations.

DR. LANGMUIR: Tom, it's going to be initially very aloof, but it's going to pick up salts, and as it recycles, it will get more and more saline.

DR. BUSCHECK: You know, I'm not a geochemist. So, you know--low ionic strength condensate water reaching those depths. But, it's probably going to be significantly out of equilibrium with the vedose water in the matrix.

As I hope is obvious, the impact of these effects could indeed be very significant if we're living with a fracture flow possibly occurring through the repository horizon not being mitigated by dry-out effects. But, if the dry-out effects do indeed mitigate the occurrence of that fracture-dominated flow, then these altered properties may impact very little, have very little impact, on transport because very little can indeed be transported if the packages remain under boiling conditions.

The impact, sort of geomechanical/hydrogeologic coupling, as Larry was saying, there's going to be both thermally, as well as fractures, induced by the mine openings themselves. And, basically, I call this macro-fracturing. By macro-fracturing, I mean fractures which have an aperture which is greater than the critical aperture for fracture dominated flow. Micro-fracturing or aperture fractures in which the fracture will not dominate flow, the matrix will dominate flow.

Thermally-induced macro-fracturing, I think, is fairly likely near openings. It may result in additional preferential pathways, but my feeling is that a few drift diameters relative to the scale of the mountain, you're not going to add any critical pathways that don't possibly already exist. But, I think of more importance is the fact that we may actually increase the liquid phase dispersion within fracture networks which would mean that if there was a preferential pathway that it has more tributary fractures to branch off into which could enhance the ability of the matrix to imbibe that flow.

Thermally-induced micro-fracturing is a possibility out to the boiling front which is well beyond a couple diameters of the mined openings for high APDs. I think it will very likely increase the matrix capillary diffusivity; thereby enhancing the impact of matrix imbibition on fracture flow attenuation. In both macro and micro-fracturing which result from thermal effects also may enhance the rock dry-out rate though my earlier calculations show this may not be a great effect. I want to point out when we account for non-equilibrium fracture matrix flow that the benefits of increased fracturing will be more apparent for rock dry-out when we can dynamically account for that coupling.

Now, getting to the questions that we need to address. The significance of the benefits, problems, and the associated uncertainties, first of all, vapor and liquid flow in fractures, we feel, is the key hydrogeologic consideration to be considered in all these questions. We feel that the repository performance, the near-field performance, and transport performance at higher APDs is less sensitive to potential variability and uncertainty in the hydrogeologic properties. We also found that adequate fracturing which current data indicates that the fractured tuff will promote rock dry-out by boiling and rapid condensate drainage. We also found that the rock dry-out volume above a certain threshold of fracturing which is well below what current data indicates is dominated by thermal load and thermal properties of the system. These thermal load and thermal properties are probably the things that we can most readily characterize and also probably the least variable. For higher APDs and also for a given APD, but going to an older age fuel with a given APD, boiling and rock dry-out benefits can persist or will persist for thousands of years which promotes more favorable waste package conditions which will also greatly enhance the ability of the matrix to attenuate fracture flow. We found that it's hard to get away from performance problems or considerations even at lower APDs; however, at these lower APDs, we see no rock dry-out benefits.

Now, specifically addressing the uncertainties, we feel for all the reasons I previously gave that performance modeling under high APDs is much less sensitive to hydro-geologic variability and uncertainty. I mean, the actual performances and then the prediction of that performance would also be less sensitive. However, the currently avail-able data on fracture network properties is obviously limited. We're limited to, you know, borehole traces and the like. We have to get underground to characterize that. Also, the in situ test data for hydrothermal model validation is limited to the various experiments that were conducted in G-Tunnel. And, so that is somewhat of a limitation. There-fore, to resolve uncertainties, we obviously need to get underground to characterize the site and conduct the ESF testing. I also think prototype testing still will be quite useful because if it can occur on an earlier time frame, it will allow us to update various possible design concepts, but also it very importantly will impact our ability to ade-quately test an ESF environment.

I want to point out that it's simply not counting the number of fractures which is going to determine the performance. We have to get in there and thermally perturb this fractured rock mass. I feel strongly that even under low thermal loads, since the alteration of the Calico Hills and the TSw3 vitrophyre is significant, that it will be very advantageous not only to thermally perturb or put heaters at the repository horizon, we should also heat underlying and possibly some overlying horizons to watch, you know, in real time the response of the rock mass due to thermal changes and that the importance of that work pertains to low thermal loads, as well as high thermal loads, and by going to different environments, it will allow us to more robustly validate our modeling approaches.

We feel that by going to boiling conditions, we have a better experimental basis for model validation than data that would pertain to ambient or low thermal load conditions. There is more to measure. The effects, the geochemical coupling, there are various effects that are accelerated at higher thermal loads and you can observe them in a real time basis.

I also feel or we also feel that we're more likely to adequately resolve uncertainties associated with high APDs for the previous reasons I've given than with low. That's not to say that we can't resolve them for low. It's just saying that I think that we'll more readily be able to resolve critical uncertainties for high APDs.

Thanks for letting me go over my time.

DR. NORTH: Given that we are way over on time, I think we want to hold questions to those that are viewed as crucially important by Board members and staff. And then, after that, we'll take a break.

Any crucially important questions not already asked?

(Laughter.)

DR. NORTH: Why don't we try to hold our break to 10 minutes. Back here at 10:48.

(Whereupon, a brief recess was taken.)

DR. NORTH: Let's resume our session.

MR. CLONINGER: Okay. May I have your attention, please? We need to get rolling here. Can I have your attention, please? Our next speaker will be Brian Viani from Lawrence Livermore National Laboratory. He will be discussing the geochemical aspects.

DR. NORTH: We're resuming with Brian Viani of Lawrence Livermore Laboratory.

DR. VIANI: Thank you. This should be a brief talk. Some of it will summarize what has been done at Livermore in the area of rock/water interactions and I will basically restrict my discussion to rock/water interactions. I will not be discussing interactions between other materials, waste form, et cetera, in the near-field. And, I hope to lead you or show you several things I think are important.

One is that the uncertainties in the fundamental geochemical processes that are likely to occur are not likely to be much different under low or high thermal loading. However, the uncertainties in coupled hydrologic and geochemical processes are likely to be greater than the uncertainties in the geochemistry alone and that, in fact, without a specific analysis of identified hydrologic/geochemical scenarios, one could be led to the wrong conclusions about the effect of temperature on geochemical processes. And, finally, the analysis of these specific scenarios has not been done at this point.

Clearly, we need to predict the variation in the composition of the groundwater in the repository over time and space and this is the common denominator that will control the solubility of radionuclides, that will control the corrosion that may occur in the waste package, and control the reactivity of the materials in the waste package. So, it's something we need to predict. Basically, its composition is going to be a function of temperature to some degree because of the interaction of water and rock and also on the hydrological scenarios that Tom Buscheck has told you about earlier. We also need to predict the ability of the rock matrix, fracture codings, and other components to sorb radionuclides. And, I bring this up in the near-field because I think it's something that we need to understand as a function of temperature and is not always addressed. And, finally, as Tom alluded to, are there geochemical reactions that can affect the hydrological situation in the repository and that's something we have to look at, as well.

In distinguishing something, I'm going from the mineral geochemical processes, such as dissolution and pre-cipitation, from what I'll discuss later in terms of coupled hydrologic/geochemical scenarios. Basically, we need to know these processes as a function of temperature clearly, but also as a function of the activity of water in the system, namely the relative humidity.

And, the basic concept is that if the rock is dry, no reactions will occur. My question is at what point is the rock dry enough so that reactions aren't occurring? If one looks at the characteristic properties of the Topopah Springs that relates activity of water to saturation in the rock, one can get an idea that probably below saturations of 20% the activity of water will be below .8; where clearly at 30 and 40 and 50% saturation, the activity of water will be near 1. And, basically, under those situations, we can model the reactions, the water/rock reactions using the codes that we have in hand. But, that's clearly an unknown.

Basically, we need to know how temperature affects the dissolution and precipitation of the big actors in the system, namely the zeolites and the fracture filling minerals. And, elevated temperature can have benefits in the sense that new minerals that are formed that have exchange capacity or can sorb radionuclides may also have detrimental effects in terms of dissolution effects. The benefits and detriments will not be able to be resolved without an analysis of specific scenarios, though. Similarly, in sorption --and, I use this word very generally to include at least several phenomena that can be modeled explicitly--such as cation exchange or surface complexation, the temperature can have a strong effect, but again, as I will show, that effect can be different depending on the scenario one chooses to look at.

Just to summarize some of the work that has been done at Livermore in modeling and in experiment and basically this is--you can consider this sort of generic geochemical modeling and experiment in the sense that no specific scenario was addressed, that the experiments were closed systems with a rather high water/rock ratio in comparison to the water/rock ratios existing in the matrix of the rock, probably low water/rock ratios in comparison to water/rock ratios that you may obtain in a fracture. And, to summarize, modeling and experiment are consistent with one another in that the activity of aqueous silica, concentration of aqueous silica or the activity of aqueous silica, is the dominant variable controlling the types of phases one sees in the system and that the activity of silica in experiment and in model is controlled by the solubility of the least stable silica polymorph. But, we find that where cristobalite and glass exist in an experiment or in a modeling run that the zeolites, such as clinoptilolite and mordenite, and clay, such as smectite, can be formed and these are phases really that have significant exchange capacities and, therefore, significant sorption capacities. In contrast, for situations in which the silica activity is controlled by quartz solubility and is therefore low, phases such as analcime and feldspars are favored relative to clinoptilolite and smectite.

Now, in a repository, we have cristobalite, we have glass, we have quartz and the evolution of the silica polymorphs from a more soluble phase such as glass to quartz is controlled by kinetics. And, therefore, one would expect that increasing the temperature is going to increase the rate at which one will go from a more highly soluble polymorph to quartz. That over the temperature range we're considering, the rates that have been proposed are on the orders of tens of thousands of years at 100°C for conversion of opal to cristobalite or opal-Ct and similar sorts of times from that opal-Ct to quartz. So, we're looking at a long period of time even at 100°C with the evolution of silica polymorphs.

Superimposed on that, one needs to know what is the rate of dissolution of the zeolites if, in fact, the evolution of silica polymorphs does go to quartz and the silica activity is lowered and at some point--

DR. LANGMUIR: Brian, don't all the polymorphs go to quartz at about 200° rather quickly when you get that hot?

DR. VIANI: When you get that high, we're looking at hundreds of years probably, at least from the extrapolating experiment in terms of the conversion to the second polymorphs, yes.

From the modeling though and experiments, the relatively short-term experiments, at temperatures near 100 and above 100, in short-terms at 250 for that matter--and, I'll show you an example of that--the phases that are formed are, in fact, the zeolitic and secondary phases that exist in the rock now. This is an experiment that Kevin Knauss had run and basically this just shows that even at 250°C over a period of time of on the order of a month or so, that a vitric tuff is converting to clinoptilolite basically quite extensively. Now, I will contrast this with observations that Kevin made at much lower temperatures of 150° and lower where he sees virtually no reaction over that same period of time. That's not to say that that reaction would not occur, but certainly interpretation of these experiments at lower temperatures are difficult because there are no results. At higher temperatures, we have results and we know we're making those phases, but we have a lot of silica in solution. The question is over a period of time when the glass is completely dissolved and altered to a more stable polymorph, at that point in time clinoptilolite would become unstable.

DR. LANGMUIR: Brian, are there any experiments which have produced kinetic data, rate data, for precipitation of zeolites which we could then extrapolate to longer times and lower temperatures?

DR. VIANI: We actually had some work going on at Yale and Penn State on that, but I don't believe that they have actually completed that work. I mean, it's not ongoing at this point. I don't know of any precipitation kinetics for zeolites--for clinoptilolites, anyway. I think there's even a lack of data for dissolution kinetics of that phase which is a lot easier to do.

We also looked at incorporating ion exchange models and attempting to match them or look at how they match with experiments. Basically, that for at least the alkalis and alkaline earths, we can predict relatively confidently the composition of the clinoptilolite and its sorptive capacity with regard to, say, cesium and strontium. We can predict compositions with clinoptilolite formed during hydrothermal experiments. There is really a lack of data though at elevated temperature and our modeling suggests using estimated data that ion exchange equilibria are very sensitive to temperature. However, to address the extent of the reaction and even the direction of the reaction requires again a specific analysis of a particular scenario.

This was taken from Tom's talk. If we look at two particular areas in this conceptualization of what might happen, one might look at the area of the condensation zone where you have refluxing. And, basically, one can look at an area where one has condensation and flow through a fracture out of the system. And, in one case, you're continuously condensing a dilute fluid in a rock which is then reacting with the rock, re-evaporating or boiling, and then in so doing becoming more concentrated and this is continuing around. In the other case, one has a fracture in which the fluid is condensing and moving through the fracture, a more open system in which one might envision perhaps a dissolution at that point.

Clearly, in order to understand the geochemistry that might go on, we need to be constrained by the rock/water mass in the simplest sense. We need hydrologists to say how much fluid is going to be moving in a fracture. We can model these things given those constraints and I'll show you an example of that, a very simple example. But, clearly, understanding the extent of fracture mineral dissolution requires an understanding of the amount of fluid passing that point and understanding the extent of alteration and permeability along the boiling isotherm due to precipitation of phases in that zone requires again some understanding of just how much fluid is recycling and refluxing at that position.

DR. LANGMUIR: Brian, is there any experimental work, at all, in this area that's been identified or anticipated?

DR. VIANI: There's a set of studies that have been done at VPI that address the refluxing situation. I won't discuss it, but I'll compare that to some of the results that Kevin got in a just a second.

Just to show you how sensitive our predictions might be depending on the hydrologic scenario that one can envision, basically what I have here--the details are listed in this little inset down at the bottom--is a prediction of the K_d for cesium and strontium on the right on a rock composed of approximately 50% clinoptilolite. The predictions were made using EQ-3 which is a geochemical modeling and using published ion exchange data. Basically, we know that the predictions fit the data of 25° because we've essentially looked at how this compares to the sorption experimental results that were done at Los Alamos. However, the data at other temperature are estimated. That's an important point to remember.

Well, what one sees is that if you look at what might happen where the water/rock ratio is small--0.19 conforms to basically the water/rock ratio in a matrix of approximately 30% porosity or 20 which corresponds to the water/rock ratio as used in absorption experiment at Los Alamos--basically, you see that for cesium, the K_d decreases with temperature, but that decrease is more precipitous when the water/rock ratio approaches infinity. Now, at some point in time, if you were looking at what is happening in a fracture versus what's happening in the matrix, the water/rock ratio is going be somewhere between here and here (indicating) and we need to know what that might be because in the case of strontium, the predictive effect of temperature is opposite, is in the opposite direction. Basically, there is equilibrium situations assumed in this model. So, that's one area where we need to--as geochemists, we need to explicitly include the output from the hydrologic models.

Now, going back to your questions about experiments regarding refluxing, et cetera. When we look at the results of Kevin's experiment with the vitric material, when he does these experiments at 150° or 100°, he doesn't see anything. Yet, some of the experiments that have been done at VPI where a crushed tuff material has been subjected to refluxing shows the growth of zeolites at 100°C. Now, on a closed bomb experiment, you don't see any reaction. Yet, changing the particular features of the experiment, even going to lower temperatures, you show a lot more reaction. So, clearly, both in an experimental sense and in a modeling sense, the specifics and areas are important.

The benefits and detriments related to elevated temperature, I think if we dealt with a--if we were given a specific system in which to analyze it, is it a closed system or a fracture system, I think inherently we could address the uncertainties. At this point, I think the benefits and/or the detriments await further analysis of these specific scenarios. The geochemical processes and I would say the coupled geochemical/hydrological processes are expected to be qualitatively similar over the thermal range that we're anticipating the repository. Perhaps, if it's very, very cold or very, very hot, that would not be the case. But, certainly, one would expect evaporation/condensation effects. The extent of those reactions, though, can be significantly different depending on the temperature.

Finally, the uncertainties I think associated with the fundamental geochemical processes are similar for hot and cold scenarios. I don't think there's any particular process that we need to address at either end of the thermal spectrum that we haven't addressed already.

And, finally, coupling, as I've said before, the processes--and I don't mean coupling codes. I mean actually analyzing these coupling processes is going to introduce greater uncertainties than attacking either of those separately. But, I don't think one can do the geochemistry without it.

Finally, to resolve the issues related to quantifying the benefits and/or detriments and reducing the uncertainties, I think under the existing scope or the scientific plans that we are addressing the critical scientific issues that will allow us to resolve the issues. However, I think that this integration of geochemistry and hydrology must take place given specific scenarios and that has to be the driving force, I think, at this point on in terms of some of the geochemical program. And, the elements that we require within this program, I feel, has to be driven by the scenario and in this instance for the geochemical for modeling applications to identify the experiments that may be relevant. Clearly, the results of the closed system experiments are quite different than refluxing experiments. And, appropriate experiments need to be designed to essentially model what may be happening in a hydrological situation. And, also to define the thermodynamic and kinetic data that might be required. At this point, I think we have to go away from sort of generic geochemistry and looking at all the thermo- dynamic data and zero in on data necessary for analyzing some of these scenarios. There may be some model development with regard to sorption at elevated temperatures that we have to address and certainly we need more kinetics data.

And, finally, I think clearly it has to involve some natural analog work where the same sorts of processes have been observed or inferred, at least, to see how well we can do in understanding those situations. Although the frame work exists for doing this within the plans, at this point in time, there will be no funding of geochemistry in the near- field program over the next year. This work as developed here will be deferred at least for this coming year.

DR. LANGMUIR: Brian, what do you see that the program should be doing in order to address the couple problems that relate to Tom's earlier talk on the transport of condensate and the properties of the matrix and just characterizing the system so you can better--could have perhaps some confidence in a high temperature system as being the appropriate one?

DR. VIANI: I think what needs to be done is that the hydrologists and the geochemists have to talk a little more on certain issues. And, I think that the geochemists have got to analyze those specific scenarios, at least to place some bounds on what is reasonable. Clearly, an infinite water/rock ratio is not reasonable, but I didn't know where to put that boundary. But, that analysis has to be done. I think critical points within the repository situation have to be addressed from the point of view of geochemistry as what may happen in the matrix, what may happen in the fracture, what may happen in the reflux zone, and that has to be addressed in three areas; one is the experiment, two is the modeling, and three is the natural analog. I think all of that is within the scope of the existing sort of framework that has been essentially put forward in the area of near-field geochemistry at this point in time. But, as I said, it is deferred at this point in time.

DR. LANGMUIR: A nasty financial question and a staffing question. That without any funding in the program, can you hope to have continuity? Can you start up again in 12 months and be able to jump immediately if you had the funding into a program which could address these concerns, the kinetics, the experimental work, the analogs, and so on?

DR. VIANI: I think it's going to be on a person by person basis. At least, at this point in time, geochemists who are off the program clearly are not waiting in the wings. They're on other programs. And, so it will have to be a person by person basis and might be difficult in certain instances, yes.

DR. LANGMUIR: Your modeling is similarly going to be done with EQ3/6 and it has the capability of doing kinetics.

DR. VIANI: Yes. Yes.

DR. LANGMUIR: The program as it stands, I gather, is not really validated. It hasn't been taken through QA. It's going to require some additional work, I presume, to be able to handle these problems and to be used within the program with confidence?

DR. VIANI: Yes, I haven't addressed those issues here. What I was mostly concerned with was geochemistry applications in the near-field. Clearly, some of those applications, in fact, most of them, will require use of this code. There will be some work done this year in terms of verification of the code and yet there is additional work. For instance, the ion exchange model that I presented now, the results of that, has not been put in the version of the code that is the most recent mainly because we have not the staff to do that at this point in time. Clearly, that has to be done and my assumption is that will be done in order for us to go on and address what I think are the nitty-gritty issues which are the actual geochemical effects in the near-field. But, that certainly is a necessary part of it.

DR. NORTH: I think we want to hold further questions and go on with David Bish on mineralogical uncertainties. Then, we can come back for the questions for both talks.

DR. BISH: What I'm going to talk to you about now are some of the things you've heard alluded to in the past couple of talks, essentially the mineralogical uncertainties related to whether we go with the high or low thermal loading. I've divided my presentations into four different areas. First, I'll talk to you about the mineralogy of the host rock right around the proposed repository level and a little bit about the mineralogy of Yucca Mountain as a whole; distribution of zeolites, for example. I'll touch on the effects of dehydration/rehydration reactions and the intimately associated effects of contraction and expansion, volumetric contraction and expansion of some of the hydrous phases. I'll just briefly talk about the effects of heating on the sorptive properties of the minerals at Yucca Mountain and finally discuss the long-term stability of a number of phases at Yucca Mountain.

Well, the rocks at Yucca Mountain consist of a variety of interesting minerals. We have a couple of what I would say in the temperature regime we're talking about are relatively stable phases including quartz and feldspar. We've got phases that may dehydrate or rehydrate depending on the hydration conditions including smectite, which many of you may know just as clay---it is the clay at Yucca Mountain at the depths we're worried about--clinoptilolite and mordenite which are both zeolites that are common and volcanic glass. We also have a variety of minerals that may transform to other phases or may dissolve including the silica phases, both crystalline and the non-crystalline, smectite which may transform to illite through the illite to smectite series, clinoptilolite again which may transform to analcime and also mordenite.

Now, when we worry about the types of reactions that may occur at the repository environment, it's important to keep in mind the vertical and lateral distribution of minerals at Yucca Mountain. I've got a couple of figures in your packet that describe that. This is a west to east cross-section showing the distribution of a variety of important features at Yucca Mountain. This is the repository horizon schematically depicted here in the Topopah Spring member which consists predominately from here up of quartz and feldspar and some cristobalite and a small amount of smectite. We have an interesting zone right above this vitrophyre which is black. The zone cuts across most of Yucca Mountain, the zone just above the vitrophyre, and contains large amounts of both zeolites and smectites and it's interesting from that point of view. The vitrophyre is essentially volcanic glass, but it has a relatively low permeability. It's densely welded. Underlying that, it depends where we are both east and west across Yucca Mountain and north and south, whether or not we have vitric Calico Hills tuff or zeolitized Calico Hills tuff. So, you can see right away it's important where we are across Yucca Mountain what types of mineral reactions we can expect. I've just shown here schematically the static water level.

You've heard a lot of talk about the importance of zeolites and these are primarily clinoptilolite and the potential for reaction. And, this slide really shows some- thing that's quite important and that is that going across the repository block the distance between the base of the repository horizon, namely right here (indicating), and the first significant occurrence of clinoptilolite--and, by that I mean a significant chunk of rock meaning not this piece here (indicating) that contains 20% clinoptilolite or more-- we see at the northern end of Yucca Mountain, we have almost 100 meters between the base of the repository and the top of significant zeolitic horizons and that deepens significantly as we go southward. So, that's an important thing to keep in mind. Tom Buscheck's plot showing the temperature in the Calico Hills unit was at 60 meters depth and that doesn't occur in this block.

The next figure in your packet, I won't talk about since Tom has spent a few seconds on something like that, but essentially it gives you an idea of the temperatures related to stratigraphy.

Now, moving on to dehydration/rehydration reactions which I mentioned were important with the phases such as clinoptilolite, mordenite, smectite, and volcanic glass, it's clear that the hydration state of these phases, particularly the zeolites and clays, will change whenever the partial pressure of water changes or the temperature changes. There's no single magic temperature or vapor pressure of water that will cause an abrupt change. It's a continuous function. The nice thing about these dehydration reactions or rehydration reactions in the temperature range below about 200°C is that most of them are reversible, at least in the time scale for which we have experience and data. In other words, minerals will rehydrate as temperatures decrease or as the partial pressure of water increases.

I say here that the uncertainty in these reactions is not strongly dependent on temperature and it's something that is relatively easy to measure. The important uncertainty is the vapor pressure of water in the repository environment and that's a recurring theme throughout my presentation that that's one of the most important unknowns we have. Thus, I say that I believe it's important to couple geochemical and hydrologic and mineralogic models because all of the processes that we're interested in are coupled. You can't assume that a dehydrating rock mass is passive because the minerals in the rock mass actually participate.

Now, associated with--

DR. DEERE: A question?

DR. BISH: Yes?

DR. DEERE: On your rehydration, you say most of them rehydrate below 200°. Would this also apply to the smectite?

DR. BISH: Yes. For short-term heatings, even including dry heatings, smectite rehydrates readily under all conditions for the compositions of zeolites or of smectites that we have at Yucca Mountain until temperatures in excess of 400° are reached. When you experiment with smectites having small interlayer cations which we don't have--for example, magnesium, if we had a magnesium saturated smectite which is relatively rare in nature, that magnesium migrates at elevated temperatures and rehydration is inhibited.

DR. DEERE: Are these mostly sodium or calcium?

DR. BISH: Yes. Sodium, calcium, potassium. Primarily, sodium and calcium.

DR. LANGMUIR: These conclusions are based upon experimental work in the lab or on your analog work when you're looking at--where you have more evidence through long periods of time which relate to our heating periods here.

DR. BISH: The results pertaining to dehydration and rehydration are based solely on laboratory measurements because it's difficult to extract that kind of information from natural analog studies because typically what we see in natural analog studies are general reactions that are not reversible, say, smectite to illite.

DR. LANGMUIR: Do we know enough to predict what would happen over hundreds of years--

DR. BISH: No, that's something I'll emphasize that the problem of kinetics is significant at these low temperatures.

Related to the dehydration/rehydration processes is something that goes along, the expansion and contraction, essentially the change in molar volume of these phases. Again, just as the dehydration and rehydration reactions are, these reactions are a function of the partial pressure of water and temperature. Both of these classes of minerals contract significantly on dehydration. The zeolites in the temperature range that we're looking at and the expected partial pressures of water, however, contract by only a maximum of several percent. Smectites can contract by a factor of several.

I have some data for clinoptilolite of a variety of exchangeable cation compositions and you can see that if it's a strong effect of exchangeable cation compositions for sodium, potassium, calcium, saturated clinoptilolite and natural clinoptilolite from drillhole G-4. Let me just briefly explain these data. On this axis here, I have the volume of the unit cell which is directly related to the molar volume. The axis at the bottom is partially temperature. RT-1 signifies data that were collected under room temperature conditions and room humidity conditions. VAC-1 signifies data collected under room temperature conditions but in a vacuum so that we were under low hydration conditions. Then, we go up 50 to 100 and so on up to 300°C in a vacuum for the red dots and in air that's saturated at room temperature for the pluses. VAC-2 for the red ones are data collected at room temperature, back down at room temperature in a vacuum. And, RT-2 is back in room conditions, room relative humidity, and 100% relative humidity for the pluses. And, we can see a number of interesting trends, but the important thing is--there are a couple of important things. Number one, the maximum volume decrease we see is with the sodium saturated clinoptilolite that occurs for the most part by 100°C and that's an 8% volume decrease. That's not applicable to a repository environment. That's in a vacuum. We see what happens when we do this experiment at elevated P_{H2O} or slightly, very slightly elevated actually, P_{H2O} conditions. The volume decreases by 100 or even 200°C in water saturated air. The volume reductions are very small. We can see that it is a sensitive function of composition. So, because of these data, that's why I say that the zeolites will contract by only a couple of a percent, but it's a measurable, it's a macroscopically measurable effect, as I'll show you in a minute.

Now, for the smectites, say, that they can expand and contract by a factor of two or more. These are data for sodium smectite taken from the literature. We're fortunate with smectites that there are a lot of literature data that we can draw on. We can see at high relative humidities we have what we call a basal spacing. It's the layer repeat distance for the layer structure of over 15 Angstroms and there's a significant decrease. There's about a 2-1/2 Angstrom decrease on changing relative humidity in this region. Based on much of Tom's calculations and some of the first information we have on the expected partial pressure water conditions, we think we'll probably be up in this region (indicating) so that this type of collapse will occur only in the low P_{H2O} area. I'll get into the implications of that in just a minute.

I think probably the most implications for smectite dehydration and rehydration and associated collapse is that smectites that are lining fractures--and these occur in the Topopah Spring member so they'll be in the region that will be significantly heated--can potentially collapse at the highest temperature, lowest P_{H2O} area. As I mentioned though at these temperatures of 200 and below, the reactions are reversible so that the pathways will probably return to their original state when the minerals rehydrate as temperature decreases and the partial pressure of water increases.

There have been a couple of questions about gas transport and the one area where we can conceive of that this may be more important is that when these fractures--when the materials lining the fractures are dehydrated, they're collapsed, that there may be enhanced pathways for gaseous transport through fractures that were otherwise in the original state essentially sealed by expanded smectites. Now, the data I've shown you for the zeolites, as I mentioned, have a macroscopic manifestation.

We've conducted a number of experiments looking at the axially confined hydration of one inch diameter pieces of core that were instrumented with collaboration with some of the geophysicists at Los Alamos. These are data showing axially confined hydration of cores that were vacuum dried for three days for Topopah Springs tuff, the densely welded portion of the tuff, and for the zeolitized Calico Hills. I should emphasize that vacuum drying for three days doesn't dry out a zeolitized tuff very much. There's a lot of water in the sample still at room temperature. You can see here when we re-immerse these cores in water and measure the stress generated, we get a small amount essentially of expansion of the devitrified tuff. It's difficult to say what this is due to, but we think that it's probably due to the minor amount, a couple of a percent, of smectite in the rock.

The zeolitized tuff, on the other hand, generates a stress approximately an order of magnitude greater than is observed in the divitrified tuff. I think about all I can say with that is that there's a potential effect on the rock strength. I think it will require some more experiments and perhaps some modeling to see what the large scale macroscopic effects will be on the rock strength. But, it's an interesting large scale manifestation of these data that we've seen for clinoptilolite.

Just touching briefly on sorptive properties of both smectite and zeolites, there are abundant literature data on the cation exchange data or cation exchange data for smectites and sorptive data and all of these data show very little or no effect on smectite cation exchange capacity as it's transformed to illite smectite. There's essentially a continuous linear decrease in the cation exchange capacity as the material goes to illite. As I'll tell you in a few minutes, I think this probably won't even be an important reaction at Yucca Mountain.

From limited experimental data that we've collected at Los Alamos, we believe that there's probably little or no effect on the sorptive properties of clinoptilolite even when materials have been severely degraded by temperature. These are data for three series of clinoptilolite samples, an unheated sample, a sample that was heated to 105°C for 385 days dry, and a sample that was heated to 200°C. The sample that was heated to 200°C for 385 days dry actually showed evidence of structural transformations resulting in collapse of the zeolite structure around the exchangeable cations. And, it was our initial assumption that this reaction that we observed would have a significant impact on the sorptive properties of the clinoptilolite. And, as you can see, these are R_D values for strontium, cesium, barium, and europium and there are no consistent or large trends, although we've decreased from 400,000 to 200,000. I don't think that's anything that we pay a whole lot of attention to, because it's still huge.

DR. LANGMUIR: Dave, what do we know about the effective temperature on the sorption by the smectites and zeolites? I don't mean just up and down, but the--are these exothermic or endothermic in terms of the sorption? Do they increase or decrease sorption properties with temperature below 100?

DR. BISH: I think I'd have to defer that to Brian.

DR. VIANI: If you're measuring sorption at temperature as opposed to measuring it at 23°, I can show you plots of how the cation exchange equilibria change with temperature. There's the log K of the reactions and clearly there are differences between the alkalis and alkaline--I have not looked at barium and europium. But, whether you predict an increase or a decrease in K_d, it depends very strongly on the water/rock ratio because the K_d is a function of the clinop-tilolite composition and as a function of the water, as well. In high water/rock ratios, the composition of the clinop-tilolite will be controlled by the water composition and a low will be controlled by the clinoptilolite and that will control the K_d and, therefore, the interplay of those things will make K_d go up or down in the case of strontium, basic-ally down in the case of cesium with temperature.

DR. BISH: Finally, to address something that I think is the most interesting mineralogic aspect of the heat pulse at Yucca Mountain. That is the long-term stability or the propensity for minerals at Yucca Mountain to change to other minerals over the long-term. There's been a lot of interest in whether or not clinoptilolite will react to other phases and certainly there's a potential for clinoptilolite theoretically to react to analcime or to mordenite. We have evidence in the northern most drillhole that we've looked at, G-2, that clinoptilolite as the temperature was increased reacted to mordenite and then to analcime at temperatures in excess of 100°C. I say here that it appears stable in saturated rock to about 100°C. There are a number of assumptions in that statement. Number one, that we don't have high water salinities, high salinities of water, because there are data suggesting strongly that clinoptilolite may react to analcime at considerably lower temperatures at elevated salinities. There are more and more data that we're obtaining that suggests that the reaction of clinoptilolite to analcime is a strong function of the silica activity and Brian alluded to this a bit. But, it appears that for the reaction of clinoptilolite to analcime to be important in Yucca Mountain, a tremendous amount of low stability silicic minerals would have to disappear and be transformed to quartz, essentially. The large amounts of clinoptilolite in the Calico Hills unit are intimately mixed with both opal-Ct and cristobalite in amounts of 10 to 40% and our data that we've obtained, so far, some of which are natural analog data, suggest that for that reaction to go, all of that material would have to transform to quartz. And, as Brian mentioned, the temperatures that we're dealing with, the reaction of opal-Ct or cristobalite to quartz would take tens of thousands of years at temperatures above 100°C. Comparable data suggests that mordenite is stable to at least 130°C, although the same caveats about water composition apply here.

The volcanic glass is an interesting phase at Yucca Mountain and I think it probably has the most potential for altering at the temperatures that we're dealing with and the time scale. I said here that it may alter at low temperatures in saturated rock to other silica phases, opal-Ct or cristobalite, to smectite or to zeolites. As Brian mentioned, experimental work at Livermore produced smectites and zeolites from the vitrophyre at the temperature of 250°C, whereas lower temperature experiments below 250°C did not produce these secondary phases. I think it's probable that the nonwelded vitric tuff that I outlined in one of my first slides on this side that in some places closely underlie the repository horizon will alter to clinoptilolite and/or smectite if some of the things that Tom was saying are true.

That if we have a pulse of hot condensate percolating through this very permeable material, I think it very likely that this material, considering its reactivity, will react to secondary phases readily.

Continuing in the long-term stability, just a few other interesting reactions. I mentioned that it appears that the cristobalite, tridymite, or opal-Ct must react to quartz before we can have the clinoptilolite to analcime reaction. This reaction can occur at low temperatures through a solution precipitation reaction, but it requires tens of thousands of years. In addition to affecting the other mineralogic reactions in the tuffs at Yucca Mountain, there's a significant change in volume when we go from cris-tobalite to quartz, a volume decrease of 12%. And, in the rocks around the repository horizon, there's generally between 10 and 20% cristobalite. I just mention this because it's been referred to in a couple of other talks, particu-larly Larry Costin's this morning that the alpha to beta cristobalite reaction which is reversible--shouldn't neces-sarily be under long-term stability--occurs at around 230° with a positive delta V.

Now, I've referred to what is some of my conclu-sions about smectite. It's well known from literature data and we have some data at Yucca Mountain also that show that smectite reacts to illite through the, by now well known, illite/smectite series with increasing temperature, I say here, under saturated conditions. And, there are more and more data now in the literature that show that saturated conditions and relatively high permeability is a prerequisite for this reaction to occur. There are several papers discussing the kinetics of the smectite to illite reaction in the literature. We haven't felt the need to reproduce them. They show that at temperatures around 100°C, it takes times in excess of 10^6 years with adequate water to form--to significantly illitize the smectite. Under the water/rock ratios that we expect at Yucca Mountain, which are much lower than, say, Gulf Coast sediments where this reaction has been studied a lot, this may not even be accurate. It may be low. So, I think the smectite to illite reaction is not going to be something that we need to be concerned with.

Again, back to something that was mentioned earlier, for all of these reactions, increasing the temperature or increasing the thermal load will improve our ability to predict what reactions occur because this will partially mitigate our kinetic problems. It's very difficult experimentally to determine what's going to happen in the 100° or below range because the reaction times are way beyond our lifetime.

DR. LANGMUIR: Dave, that's if you've got water.

DR. BISH: That's correct.

DR. LANGMUIR: But, if the increased temperature means the water goes away, the rates go down?

DR. BISH: That's right. That's right.

We do have some interesting data that Schon Levy at Los Alamos has been able to obtain using the alteration of the vitrophyre. I can probably put that slide back up real quickly. That underlies the repository horizon. That's that dark band. She's observed some very interesting alteration in this zone between the vitrophyre and the divitrified tuff. In other words, what we call the transition between TSw2 which is the devit tuff and the vitrophyre. And, we believe that it's a potential natural analog for repository-induced alteration, particularly of volcanic glass, both vitrophyre and vitric tuff. The alteration in this setting was dynamic and it was concentrated around fractures. So, we really don't know a whole lot about the state of saturation and the alteration varies all over the mountain. But, the natural alteration assemblage suggests that the vitrophyre altered to clinoptilolite, smectite, and silica phases. We know these phases are present, but our data suggests that the alteration occurred at relatively low temperatures which is quite interesting.

And, we've obtained some oxygen isotopic information for three samples from Yucca Mountain, and depending on whose fractionation curves you use, you get a range--we obtain a range of reaction temperatures from 40 to 100°C for the reaction of the volcanic glass to more stable silica phases. In this case, we looked at quartz. So, these data demonstrate, first of all, the reactivity of the glass phases at relative low temperatures and that's why I said that we feel we can probably safely say that the vitric tuffs and the vitrophyre will alter, at least partially, the secondary phases under the conditions we expect.

So, in summary, there's significant amounts of volcanic glass, zeolites, and smectites close to the repository horizon, primarily beneath in that interesting zone I discussed. The hydration state of the zeolite, both the zeolites and the clays, can change whenever the temperature or the P_{H2O} change. The volume decreases are largely reversible in the temperature range we're talking about and there's a potential for creation of fractures and differential stresses through these volume decreases and increases.

The sorptive properties appear to be little affected by hydration. An interesting conclusion I think we can make is that at the temperatures of around 100°C--you'll notice I'm not concentrating on the region right around the repository horizon which is predominated by quartz and feldspar and cristobalite. So, I'm down and worrying about temperatures around 100°C. It appears that very long times will be required to transform the clinoptilolite or the smectites to other less sorptive phases.

However, the volcanic glass which is much more reactive can transform to zeolites and/or smectite at temperatures as low as 40°C in the presence of water. That's an important caveat. Again, increasing temperature will improve our ability to predict these reactions because of the kinetic problems at lower temperatures. I don't think that the magnitude of the increase is going to improve our ability to predict by a lot because if we go from 80 to 100 or 120°C, we're still in the temperature regime where the kinetics are very slow. And, obviously, some of the thermal reactions, for example, glass to zeolite or smectite, may be beneficial, although they'll obviously, as Tom emphasized, cause a modification of flow paths and change of permeability if, for example, we react nonwelded vitric tuff to zeolite.

Now, addressing some of the things we talked about earlier, it's difficult to say definitively how all of these things tie together, but I think it's safe to say based on our analysis that benefits to lower thermal loading, namely a smaller rock volume affected and a lower intensity of alteration, the overall temperatures are lower, probably outweigh those of the higher thermal loading. There's a larger rock volume dried which is a benefit, but the temperatures are higher, we extend farther into zeolitic tuffs, and so on.

The potential mineralogical problems associated with the higher loading, namely that we may extend the higher temperature zone to the zeolitic tuff, are probably greater than those associated with lower thermal loading which might involve not drying the rock as much. There are a lot of uncertainties, as Brian emphasized. Whenever you're talking about geo, well, I don't have geo-mineral, but we have that in this. We really need to know a lot more about the time/temperature saturation curves that we expect in a repository environment and that's the crucial unknown in predicting many of these reactions because they're so tightly linked to both temperature and saturation conditions. And, in addition with all of these reactions that occur in the temperature range below 200°C, the kinetics of the reactions are a tremendous problem.

We can resolve many of these uncertainties at least sufficiently, I think, using experimental data and there are and have been in the past ongoing experiments evaluating the kinetics or attempting to evaluate the kinetics of these reactions. It's difficult. I think we've made an effective use already of natural analog and field data. I think that may be one of the areas where we can get some good information because we then have the long times required. We definitely need to consider these mineralogic reactions in geochemical and hydrologic models because all of the processes that we're worried about are coupled.

So, in a nutshell, I believe that changing the thermal load whether we go from 20kW/acre, say, to 100kW/acre will only modify the extent of the mineralogic reactions. Because of the distribution of phases susceptible to alteration at Yucca Mountain, I think we will not ever eliminate these reactions. I think that's a given. There are phases susceptible to alteration very close to the repository level. And, just to emphasize one more time that understanding the thermal effects relating to mineralogy will require some additional experimental data and coupled models.

DR. DOMENICO: David, both smectite and zeolites will probably undergo reversible dehydration above the boiling point of water and you say the smectite occurs in the fractures in the Topopah. Has anybody given any thought to just volume-wise how much water would be potentially generated with that reversible dehydration of those minerals? Do they constitute a significant portion of the rock?

DR. BISH: That's a good question, but you have to remember that as we--smectite doesn't dehydrate just like that as you reach the boiling point. There's an equilibrium between the water in the gas phase around the smectite and the water in the smectite.

DR. DOMENICO: But, it will break down over the time span we're talking about.

DR. BISH: Yeah, well, it will--you will evolve water, true.

DR. DOMENICO: Yes.

DR. BISH: But, the partial pressure of water, say, in the steam atmosphere above 97°C is going to be--it's not going to be zero. So that we need to be able to predict that. We need to know exactly what the partial pressure of water is at those temperatures and do some experiments. But, my suspicion is in the Topopah Spring member where the amounts of smectite are very low, on the order of a couple percent, that that amount of water would not be significant.

DR. DOMENICO: How about the zeolites? They're a bigger percentage of the rock. Do they occur in the fractures or do they occur disseminated through the matrix?

DR. BISH: They occur in minor amounts in fractures throughout Yucca Mountain, but very minor amounts. It's difficult to find very many zeolites above the massive zones. As many of Tom's calculations showed, the state of saturation will be high enough in the zeolitic tuffs that you will probably--this is what I meant by the importance of coupled models. You will change the state of saturation of a zeolite as you change the temperature, but they're in relatively highly saturated rocks, 80 or 90% saturated, where the activity of water is essentially 1. So, I don't think that because of their distance away from the heat source at the northern most point, almost 100 meters, I don't think the saturation will be low enough and the temperature will be high enough to make them an important contributor.

DR. LANGMUIR: Dave, you've done some work on the reversibility of heating and cooling of the sorption properties of the zeolites and smectite. But, realistically, if we're looking at repository temperatures that are elevated for hundreds of years, do we really know that the sorption properties are reversible for these solids?

DR. BISH: No, we don't. Just to give you an example. Previous to some work that we had done, it was assumed that clinoptilolite was insensitive to heating to about 550°C and, in fact, you essentially had to go to about 900°C to break down the structure. We've done some experiments dry now--and there's a big difference between dry and the conditions we expect at Yucca Mountain--at 200°C for two years and we've seen significant mineralogic reactions. This is that collapse that I referred to for the 200°C clinoptilolite sample. So, we're already getting into areas where we're finding new things that if you'd discussed them two years ago or five years ago people would have said nothing would happen. So, clearly, that's the big problem when you try to predict what happens at 100 or 200°C to minerals. We can't adequately address those questions in our lifetime. And, the kinetics people will say, well, let's go up to 400°C, but the mech- anisms are likely to change so much that the results may not be at all applicable.

DR. DEERE: Were you going to discuss the last--

DR. BISH: I left that last slide in there just as an example of some mineralogic data in case some questions came up, but that's just for your reference. I don't need to discuss that.

DR. DEERE: Was some of this obtained from the drill cores that--

DR. BISH: Those are data from drill hole G-2 with mineralogic data on the right of the figure and illite/ smectite data as a function of depth on the left.

DR. DEERE: I think you presented this to us in--

DR. BISH: I did in Reno.

DR. DEERE: And, I found it very interesting.

DR. BISH: Well, thanks.

DR. NORTH: Any other questions, comments?

(No response.)

DR. NORTH: Okay. Then, do we want to go on? We'll get in one more talk before lunch and we'll plan on breaking at 12:30.

MR. CLONINGER: --discuss waste form degradation and materials uncertainties. Those materials being waste package and other repository materials.

DR. GDOWSKI: Okay. Just a brief outline of the presentation. I have an introduction which for the purposes of this presentation will define three temperature regions. Also, I'll briefly discuss the functions of the various components of the waste package for the SCP conceptual design and we'll talk about thermal effects on container materials, specifically limiting the discussion to metallic alloys. Then, we'll talk about thermal effects on the waste form. We have two waste forms; the spent fuel which consists of the zircaloy cladding and the fuel pellets, and borosilicate glass is the other waste form which consists of a pour canister and the borosilicate glass. And, finally, to summarize.

Okay. For the purposes of this presentation, we have defined three temperature regions; high temperature region and the boiling region and the below boiling region. The high temperature region, the lower temperature limit is material dependent and is characterized by microstructural changes which can result in chemical and mechanical degradation of the materials. And, this region is also characterized by accelerated oxidation.

And, we have the above boiling region. This is dominated by gas phase phenomena. That is we don't have aqueous phase degradation occurring on the materials. The lower temperature limit definition is complicated by the presence of hygroscopic sales, pores, and crevices. And, this temperature limit will be above the unconstrained boiling point of water.

Then, we have the below boiling region which is dominated by aqueous phase phenomena. This could either be caused by high humidity or liquid water. Again, the temperature definition is complicated by the presence of salts, pores, and crevices.

DR. LANGMUIR: Greg, is the oxidation accelerated in the absence of water?

DR. GDOWSKI: Sure. Well, if you have oxygen there--

DR. LANGMUIR: But, you've got to have some--okay.

DR. GDOWSKI: Oxygen or water will oxidize materials.

DR. LANGMUIR: But, the continuation of the process may just armor it and stop, right?

DR. GDOWSKI: Excuse me?

DR. LANGMUIR: You might just armor the surface with an oxide and stop?

DR. GDOWSKI: You might, but at higher temperatures what happens is the film is a known protective and you can increase the oxidation rate after it becomes linear with time. This is at very high temperatures.

DR. REITER: High temperature, is it well above boiling?

DR. GDOWSKI: Well above boiling, yes.

DR. REITER: Can you give us an idea of what kind of temperatures--

DR. GDOWSKI: 350 to 500°C. It's very material dependent.

DR. VERINK: That range is well above what's anticipated then. Is that right?

DR. GDOWSKI: Yes. Yes. I'm just using this for a reference.

Okay. To briefly review the functions of the various components of the SCP conceptual design waste package for the spent fuel, initially the waste container functions to keep both air and water away from the spent fuel. Upon failure of the waste container, we would have Carbon-14 released from the fuel cladding. Initial studies indicate that the amount of Carbon-14 contained on the cladding is about 2% of the total inventory.

Then, the fuel cladding function is to keep air and water away from the fuel pellets. Upon failure of the cladding, we have release of the gap radionuclides and we also have degradation of the waste form.

For the borosilicate glass case, the waste container functions to keep air and water away from the glass. Upon failure of the waste container, the pour canister then functions to keep moisture away from the waste form. Upon failure of the pour canister, the borosilicate glass can then degrade.

Okay. Now, first, I'm going to talk about the temperature regions for the waste container's materials. And, what I've done here is I've just listed the possible degradation phenomena and concerns in the different temperature regions. This is the high temperature region, this is the above boiling region, and the below boiling region. First, I'm going to discuss the high temperature region.

In the high temperature region, this is characterized by phenomena which occur at elevated temperatures over typically short periods of times like years, minutes. And, the lower temperature limit in these cases are about 350 to 500°C. The concerns or considerations in this temperature range are the precipitation of carbides and intermetallics in such materials as stainless steels and nickel chrome--alloys.

You can have graphitization of cast iron. The internal oxidation of titanium occurs at temperatures of about 350°C. And, we also have accelerated oxidation in this region. Potential problems are all these considerations. Potential benefits of being in this region, I can see none.

Now, to discuss the above boiling region. The environment expected is a dry steam/air mixture with possible radiolysis products. Now, this will depend if we go to a self-shielded container or not. The considerations here are long-term aging, general corrosion, and episodic water contact. The reference water contains both carbonates and silica which will form scales when they come into contact with materials. We also have the problem that we may form halite salt deposits on these surfaces.

Potential problems are the microstructural changes which may be due to the long-term aging. That is these phenomena that we normally associate with the high temperature region may occur in this temperature region after long time aging. Another problem that could happen is the mineral deposition due to episodic water contact. That is we could form a porous scale on top of the metal which could be sites available for localized corrosion. Another concern that we have is the salt deposits on the surface. If water comes into contact at some later time and doesn't evaporate, that can also be sites for localized corrosion. And, finally, we can have enhanced corrosion because of radiolysis products.

Possible benefits of being in this region are the growth of protective oxide layers which would enhance the corrosion resistance of these materials when we go into this low temperature region for aqueous phase degradation. And, another benefit may be the annealing of residual stress in this temperature range. Another possible benefit may be modeling. If all we have is general oxidation, there are existing models which we could probably extend for our pur- poses.

In the below boiling region, the expected environ- ment is humid air/liquid water with possible radiolysis products. There are numerous considerations, general cor- rosion, localized corrosion which is pitting and crevice corrosion, halite-induced stress corrosion cracking, micro- biological corrosion, hydrogen effects, and mineral deposi- tion due to water contact.

Potential problems are all these considerations. Modeling is also a problem. Right now, we don't have very good models to predict phenomena such as localized corrosion for corrosion resistant materials and there's also the prob- lem with microbiological corrosion. Another problem may be the enhanced corrosion because of radiolysis products.

Potential benefits, we may have a favorable water/ material interaction. The water may remain benign and not corrode the materials.

DR. LANGMUIR: Greg, that's only going to be true with a metal which won't oxidize, right, because you're going to have oxidizing conditions in your water.

DR. GDOWSKI: I'm sorry.

DR. LANGMUIR: Presumably, with water in contact with the waste in the unsaturated zone, it's an oxidized system.

DR. GDOWSKI: Yes.

DR. LANGMUIR: So, to the extent that metals are going to oxidized, it's not favorable unless you've got a--

DR. GDOWSKI: Well, if you form--at these temperatures, the oxide layer probably won't grow very much under these conditions. By favorable, I mean we won't get halite ion concentration in the solution which would cause localized corrosion.

DR. LANGMUIR: But, if we go through a heating/cooling cycle and we cycle saline fluids back, we're going to have high chlorides in them when we get there.

DR. GDOWSKI: It is--yes, yes. Right, right. And, that's why I listed it under potential problems also. It could go either way.

Okay. Next, I'll discuss the temperature regions for the zircaloy cladding. Again, I've listed the possible degradation phenomena and concerns for the three temperature regions.

The high temperature region, we have a better definition of the lower temperature limit. That's 350°C. If there's no container failure, we have an inert atmosphere. If the container does fail, we have a dry steam/air mixture with possible radiolysis effects. The considerations in this temperature region are the creep/stress rupture of the cladding due to the high internal gas pressures in the cladding due to both the initial pressure in the cladding and also due to the gas pressure generated by the fission gases. Accelerated oxidation which would result in a porous non-protective oxide layer on this material occurs at temperatures beginning at about 540°C. And, you can also get internal oxidation of zirconium alloys at temperatures of 700°C.

The potential problems are the creep/stress rupture of the cladding and the loss of its function as a barrier to the oxidation of the fuel.

Potential benefits of being in this region, there are none.

In the above boiling region, container intact, we have an inert atmosphere; container failure, a dry steam/air mixture with possible radiolysis effects. The considerations in this temperature region are general corrosion, episodic water contact resulting in mineral deposition, long-term aging, and radiolysis effects. I also forgot to mention that hydrogen is also a concern in this region. It's hydrogen that's introduced into the cladding during the oxidation and the reactor. And, if this hydrogen precipitates, it could destroy the cladding.

Potential problems are all these considerations and, in addition, the Carbon-14 release.

Potential benefits is that in part of this temperature region we may be above the hydride precipitation temperature. Also, annealing in this temperature region may relieve some of the radiation hardening of the cladding. And, we can also get protective oxide layer growth and also a benefit may be modeling if all we have is the general oxidation of the zircaloy cladding.

The below boiling region, again if the container doesn't fail, the atmosphere is inert; if the container does fail, it would be humid air/liquid water with possible radiolysis effects. The considerations are the same as for any other metallic material; localized corrosion, general corrosion, halite-induced stress corrosion cracking, hydrogen effects, microbiological corrosion, and mineral deposition.

Potential problems are all these considerations.

Another problem is modeling of these processes in this low temperature region. Again, there are very few models that predict localized corrosion for these corrosion resistant materials.

A potential benefit may be a favorable water/ zircaloy interaction.

Okay. Fuel pellet degradation, I've combined both the above boiling and high temperature regions. If there is no container failure or cladding failure, we have an inert atmosphere. If both the container and cladding fail, it's a dry steam/air mixture with possible radiolysis effects. The considerations are the oxidation of the fuel pellets. At temperatures above 250°C, preliminary studies indicate that the fuel pellets are oxidized rapidly to U_3O_8/UO_3 and what happens is that this disintegrates the pellets into a powder form exposing all the grain boundaries. At temperatures below 250°C, the pellets are oxidized to $UO_{2.4}$ and fragments remain intact. I should explain that the pellets themselves are degraded in the reactor into fragments during the power cycle. So, these fragments that I'm referring to are those fragments that come out of the reactors.

The potential problems are the oxidation of the fuel pellets and the release of volatile radionuclides.

Potential benefits of being in this region are that we have no dissolution since there's no water and also no oxidation if there's no container failure or cladding failure.

In the below boiling region, the atmosphere is inert if there is no container or cladding failure. If the container and cladding fail, we have a humid air/liquid water with possible radiolysis effects. Considerations in this region are the oxidation response and the fuel dissolution.

Potential problems are fuel dissolution. If the fragments have not been oxidized and remain as UO_2, we have fragment dissolution. But, if the fragments have been oxidized to U_3O_8/UO_3, we then have to consider powder dissolution.

Potential benefits may be a favorable water/fuel pellet interaction. That is the fuel pellets are not readily dissolved. Benefits may be the low oxidation rates in this temperature region. The oxidation of the UO_2 has a very strong--dependence with an activation energy of about 27k cals/mol. Another benefit may be that there's no oxidation or dissolution if the container and the cladding don't fail.

And, finally, the borosilicate glass. Again, I've combined the above boiling and high temperature regions. If there's no container/canister failure, we have an inert atmosphere. If the container and the canister fail, there's a dry stream/air mixture with possible radiolysis effects. The considerations are devitrification of the glass above 500-600°C. This is not really a problem. Under none of the thermal scenarios does the glass ever reach these temperatures. Another consideration is hydration of the glass.

The potential problems are the hydration of the glass with the subsequent availability of the radionuclides for dissolution.

Potential benefits may be that the hydration rates are low in low relative humidity air. Under the unsaturated conditions, relative humidities are expected to be below 50% and temperatures above 120°C. Another potential benefit may be no dissolution since there's no liquid water. When a glass hydrates, it also may form secondary mineral precipitates on top of it which will slow the radionuclide release. And, finally, if the containers don't fail, we have no hydration.

Then, the below boiling region. If the container and canister don't fail, it's an inert atmosphere. However, if the container/ canister fail, it's a humid air/liquid water with possible radiolysis effects. The considerations are the hydration of the glass and glass dissolution.

The potential problems are radioactive release associated with glass dissolution and hydration of the glass.

Potential benefits of being in this region are that the glass very slowly hydrates in this temperature region and we also may have favorable water/glass interactions. Silica in solution is known to impede the dissolution of the glass.

And then, finally, just to briefly summarize. Based on previous experiments and preliminary results or testing, certain temperature regions appear to offer advantages over other temperature regions for the various waste package components when they are considered independently.

For the container materials and the zircaloy cladding, this region is the above boiling region. For the UO_2 fuel pellets, it's below boiling region just because of the very low oxidation rates that will occur there. And, for the borosilicate glass, the below boiling region is because of the very low hydration rates there. Now, this one could also be above boiling since hydration rates would be very slow in low relative humidity air.

And, finally, testing will be necessary to determine whether the degradation modes exist under repository relevant conditions, and if they exist, to determine their significance.

DR. NORTH: Questions? Ellis?

DR. VERINK: It would appear that your summary argues pretty strongly for a robust extended life container. Is that a correct presumption?

DR. GDOWSKI: Yes. If you want to protect the fuel from oxidizing or dissolving, yes.

DR. VERINK: And, you would like to do that, correct?

DR. GDOWSKI: Yes.

DR. VERINK: Does it make any additional warm, comfortable feelings to think in terms of a thicker rather than thinner container as a result?

DR. GDOWSKI: I would have to say that would depend a lot on the water/material interactions. I would think that you would probably want to eliminate some of the radiolysis effects which a thicker container would give you the added benefit of, yes.

MR. CLONINGER: Ellis, one thing that has come out of this as a suggestion is that for the thermal period we have a corrosion resistant shell to handle the highly oxidizing causations during the high temperature pulse and within that a thicker corrosion allowance material. It would also provide shielding--

DR. VERINK: I'm having an awful time hearing you.

MR. CLONINGER: Okay. I'll speak up a little bit here. This microphone isn't picking me up too well. I'll start over.

Some suggestions out of this work have indicated that for the high thermal pulse period, we have a corrosion resistant thin wall of material to account for the highly oxidizing conditions, steam/air mixture, during the early thermal period. And, within that, a thicker corrosional allowance material that will also provide shielding and whose aqueous corrosion products would also provide a sorption barrier and possibly even a filter for some of the partic- ulate materials that may form.

DR. NORTH: Further questions?

(No response.)

DR. NORTH: I have one announcement I'd like to make before we break for lunch. Attendees for this meeting need to sign in not just the first day, but every day. As I understand, we need to have this information not only for the Board, but also for the hotel. And, the consequences are that if you don't sign in every day, we run out of coffee cups and our chairman has to drink from a glass. So, please, sign in every day.

Okay. We're adjourned for lunch now for an hour and 10 minutes.

(Whereupon, a luncheon recess was taken.)

A F T E R N O O N   S E S S I O N

DR. NORTH: Let's reconvene to finish up the morning session. We have one remaining speaker, Dr. Kent Ostler. After we finish that, Dr. Dennis Price will open the afternoon session.

Dr. Ostler, would you like to go ahead, please?

DR. OSTLER: Yes.

I'd like to this afternoon, take some time and talk about the biological resource concerns that we have with thermal loading.

Just an outline of the presentation, first I would like to delineate the impact; talk about the significance of that impact, briefly mention what we know about the sig- nificance of that; talk about some of the uncertainties involved; then provide some resolutions, ways that can resol- ve some of those uncertainties; discuss briefly some of the residual uncertainties; and, then have a final conclusions.

The viewgraph shows the anticipated impacts. Again, these are taken from the model developed by Eric Ryder. The maximum temperatures are expected to be less than 6°C, again the most probable increase then would be closer to 1.0 - 1.5°C. The area impact on the surface is going to be over 2.3 - 3.0 square mile area. We are going to see a temperature increase that begins about a thousand years after initial emplacement that will reach peak temperatures some- where between 2,000 and 3,000 years and then will decline after that.

What is the significance then of that thermal loading on the biological resources? I think that is really going to depend upon the actual temperature increase at the surface. At less than 2°C, I think we are going to see minimal impact and I'll discuss reasons for that later on. Between 2 and 6°C, I think you may see moderate to larger impacts.

Areas that we anticipate being changed, being impacted by the thermal loading, these are the three main ones: altered water mass balance; altered timing of the biological processes; and, de-stabilization of the system. Certainly, there may be others, but these are the ones that I will cover today.

The altered water mass balance, we know with in- crease thermal loading, we are going to see increased evapor- ation at the surface. We are also going to see increased transpiration. As the plants try to cool themselves, they will be using additional water. What this does is take away then available water for other biological processes.

Since water is a key limiting factor in biological resources at Yucca Mountain, we will see probably a shifting of the growing season, or shortening of the growing season at the site.

Another impact again would be the altered timing of the biological processes. Many species use environmental cues to initiate certain phases in their life cycle. Many species use soil temperatures in order to activate particular activities. Example of this would be seeds. Many plant species out there, the seed germinates at a particular tempe- rature. If you alter that temperature artificially then you are going to create kind of an asynchrony. The seed will germinate before actual air temperatures may be suitable for survival of the seedlings.

A second example would be any of the species which hibernate, the desert tortoise and other small mammals on the site, use borough temperature as a mechanism to emerge from hibernation. So, if you alter that temperature artificially increase it 2°C, you are going to have them emerging from hibernation earlier than usual.

The limited data that we do have at Yucca Mountain shows that in the early spring months, that soil temperatures at 45 centimeters increase about 2 degrees every month. So, by increasing the thermal loading and increasing then the surface temperature by 2 degrees, you may see a shifting then of emergence for example of a small rodent by a month period.

That might not seem like much and may not be very significant for many species. But, for example, if the plant is dependent upon pollination from a migrating pollinator, and if it is a month off cycle, it may not be pollinated at all.

If you look also at increases of 4 to 6°C and a species that then emerges from hibernation, not in March or April, but is coming out in January, it may be a very significant impact on that species.

Also, along that line there may be insufficient time to complete life processes. Now, as you shorten the growing period, as you remove water from the system, you may reduce the amount of biomass that is able to be fixed. As you shift the growing season more towards the winter months where light levels are lower, there may be less fixing of nutrients, and thus there may be less biomass for animals as well who rely on those resources.

Finally, this may cause a de-stabilization of the system. Yucca Mountain is a very harsh environment. It is in the transition zone between the Mohave Desert and the Great Basin Desert. And many of the species that occur within this transition are very close to their threshold limits. As we artificially modify those environmental param- eters, we may very easily go over the threshold limit and go over the limiting factors that control distribution of those species.

Let me just give you one quick example. The last three years of drought that we have had at Yucca Mountain have essentially taken the dominant grass species out of the Larrea-Ambrosia association which is one of the very lowest ones out there and I will show you a little more about that later. Essentially, we have lost all of those two grass species from those two environments. What that will do on the total system is something that I will probably address in one of the uncertainties.

Also, we may see an enhancement of other detrimental processes to the system, such as an enhanced decomposition of organic matter. Organic matter is very important in these desert soils. It provides water holding capacity, and also creates an area of active nutrient exchange. Organic matter is generally very low in these soils, so it does play a very important role, and, as that is lost, then, it is of significant impact of the system.

We may also enhance soil pathogens or other soil pests if for example winter temperatures don't get low enough to kill these organisms in the soil.

What do we know about the significance of thermal loading on these biological resources? I think to answer that question, we need to look at the variability that exists within those systems. Look again at the current environment and from a regional basis, we can look at some of the season variability and how that compares with what is anticipated in the repository.

Fortunately, on a regional basis, there have been studies that have looked at soil temperatures and impacts on the system. There is an example of work done by O'Farrell up at Hanford. He's got data for soil temperature data, three depths for six different years. When you take a look at the variability within just any one year, we have a wide range of temperature there. Also, one of the important things from this is that in 1969 the low winter temperature there, and those temperatures caused a delay in the emergence of the pocket mice, peroganathus of four weeks. That was essentially a four centimeter decline in soil temperature.

We have some very limited site-specific data. I think that is one of the areas that we really need to get more information in. Over the past year, we have been gathering this information as part of our terrestrial ecosystems monitoring. This is to show you the vegetation associations that existed at Yucca Mountain, there are four of them: Larrea-Ambrosia, Larrea-Lyceum-Grayia, Coleogyne, Lyceum-Grayia. These are generally on almost a south to north gradient, and also an elevational gradient, too, with the higher elevations being over in the Coleogyne and Lyceum areas.

Three important things to get from this table is the variability that exists between the four different associations. I can see an increase of about 1.5°C between those, January and even more in the August temperatures. The second point, I think, is to look at the range of temperatures within these areas. Again, these represent 12 different study areas within each one of those vegetation associations. The range then, naturally, we see it to be between two to four percent in the case of the Lyceum-Grayia for January temperatures and about two to three degrees for the August temperatures.

Finally, we can compare difference between years; September of '90 versus September of '91 temperatures. You can see that the temperatures have dropped anywhere from one to two and a half centigrade in 1991. So, that gives you an idea.

The natural variability within the system then, is anywhere from one to two degrees. And you can get that two degrees change and really not be out of the same vegetation association. Those species will still remain present in there. Now, there may be different factors going on there, biomass may be different or total number of species may be different in those systems.

One of the other things that we can use as a comparison then to look at the impact, and that is to look at natural analogs or geothermal areas. Unfortunately, no one has done any extensive studies on those that occur in the desert area. I do have information on a geothermal area from Yellowstone National Park region, again reported by White in 1978. He looked at three different zones and those were based on the appearance of Lodgepole Pine in those three areas with normal Lodgepole Pine growth. He then went and sampled and got near surface heat flow within that range. Again, as Tom pointed out in an earlier presentation that we anticipate that we are going to be about 1.5, so even less than his lowest there.

In the mix zone where you did see some stunting mixed with normal trees, those were your heat levels. And then where all the trees was stunted was over 20.9.

He did make one important point though, and that is the actual upper limit of tolerance of the species is not so much set on the heat flow as it is by the seasonal, maximum soil temperature. At Yucca Mountain, we are already at very high soil temperatures during the season, so additional increases may be more important in a Yucca Mountain situation than in this case.

We also looked at the literature, ran a literature search for soil temperature, soil moisture, and then the interaction between those. There are really numerous studies out there that have addressed these topics. They are primar-ily in the agricultural/horticultural field, but I think they supply us with very good information on the biological proce-sses that we can expect to see change, although, we really don't have any site-specific information for the species at Yucca Mountain. Again, I think that is one of the major uncertainties in our state of analogy that we do have very limited site-specific information on the impacts of those changes.

The other uncertainties, I think, can be broken up in species processes and then system processes. I think the uncertainties lie in what the change of magnitude of the change in the phenology or the activity periods; what is going to be the magnitude in the biomass production or the available food resource that is there; and then, what impact will it have on the available water. We really can't answer those at this time. I think we know the direction that they are going, but we don't have accurate figures on what that change will be.

Then further looking at the ecosystem processes, we don't know what the loss of a species or several species from an ecosystem will be; what will be the interaction of the species that remain; and, then the impact on the trophic levels, as well.

So the resolution of those uncertainties then, I think that many of them can be resolved by measuring existing ecosystems along latitudinal/elevational gradients will supply us with some of those answers. We can also go in and measure local/regional geothermal areas. There are geothermal areas in the Mohave Desert. They just have not been studied extensively and I think we can go look at those natural analogs.

We may also need to conduct greenhouse studies or small field trials to look at some specific information for the species at Yucca Mountain. And then finally, we can develop models or improve existing models.

There will be, however, I think some residual uncertainties even after completing those studies. Most of those will deal with secondary impacts, the indirect impacts to other trophic levels in the system, and the problems with looking at effects on a large scale that are really not detectable from small scale studies.

Secondly, the evolutionary scale effects, you know we will have an area out there of approximately three square miles that may be isolated genetically from the surrounding areas as far as gene flow and what that will do to genetic drift, we won't be able to answer.

Then finally, climate change, we don't know what the climate will be when or if the repository is built or when we are actually anticipating these changes.

In the conclusions then, we believe that high thermal loading will have an impact on the biological resources. The significance of that impact obviously is going to be dependent on the actual level of increase of the soil temperature. At surface temperatures of 1 - 1.5 °C that are gradually increasing over a 1,000 year period, I think are going to create very minimal impacts. What I anticipate seeing would be a gradual shift or the greatest I guess would be a shift from a vegetation association that is more common in the Great Basin Desert such as a Coleogyne shifted towards a vegetation association that is more common in the Mohave Desert, such as the Larrea-Ambrosia.

With high thermal loading, I think we are going to see losses of some species from that immediate three square mile area. But, I think the biological system itself is going to have the ability to change and adapt to that system. So, as I mentioned before, we are going to see a change in what existed, what was there prior to the high thermal loading. But, I think it won't be an area devoid of vegetation or animal.

Certainly there are uncertainties that exist in the level of change and the impact on the civic/biological resources and many of those uncertainties can be resolved through a research program, but there will be some uncertainties that will still exist.

DR. NORTH: Any questions? John?

DR. CANTLON: John Cantlon.

I think it is important to emphasize that looking at the local effects on this three square mile area is a very different question than the biological vulnerability of any of the species populations or any of the vegetation for ecosystem types there. I think you made that point and I think that is one that is critical. However, when you get to the licensing question, it will be important that you not get caught up in saying just because a species population isn't at risk, you are not going to see change. And I think you didn't make that observation.

As one looks at the changes at the local site, one of the issues that you didn't discuss, I'd like to have you reflect on. Many of those species are rooted in fracture zones; many of them, the herbs and the shrubs both. The unique behavior of those fracture zones particularly the rhizalsphere, the mycorrhizal relationships, the bacterial relationships, must be looked at if you are going to really estimate what is going to happen.

DR. OSTLER: I agree. Many of the species, we are finding out more and more are really dependent upon those mycorrhizae. I think they do play a key role. There is some information in the literature on the effects of temperature on mycorrhizae.

DR. CANTLON: They are very sensitive.

DR. OSTLER: I think you are right. They are very sensitive.

DR. CANTLON: And the rhizalspheric relationship also is extremely temperature sensitive. The interesting thing about this is that in these generally basic soil environments, in those fissures where you get high organic matter and decom- position, you shift the soil medium to a more acid and a much more nutrient mobile situation, which is a very different nutrient medium than out in the desert floor where you don't have that kind of a unique environment.

The sensitivity of that little three square mile area is going to be keyed into some very, very tiny fraction of the total area of the system, and it would be interesting to look at what the thermal pulses are going to do in those fissures. Also, the Carbon 14 pulses that get released. It would be interesting to have some experimental data as to what the impacts of those things will have on that system.

But, again, I would stress you are not talking about risk to species populations or to vegetation types or ecosystems, you are simply trying to document in Court what is going to happen on top of Yucca Mountain, and you had better have that down in very solid facts I think as you get into the issue.

As far as analogs in looking at some of the Hot Spring action, the typical situation at Hot Springs is that you have a chemically altered soil, so it is a very atypical soil. It is shifted again in the direction of lower pH, and there is a fair amount of studies that Billings' has done a full series of studies in the Great Basin on those altered sites. So, be very careful that you are not attributing to thermal effects what in fact probably more likely is a shift in soil chemistry.

Would you give us a little thought about dealing with the fractures? Have you thought of rooting environment?

DR. OSTLER: I have really not thought about it all. I know that it exists, particularly the top of Yucca Mountain, the soil on the surface really is very limited and most of the roots go beyond on that zone into those fractures, so I think they are really keen to so many of the species up there.

DR. CANTLON: If our hydrologists and thermal response people have been accurate the last day or so that the impacts in these fissures high up run the movement of warm water vapor up through the system, if they are correct, then the impact on it is going to be much less than one might intuitively expect. But, it is very important that those figures get very careful scrutiny and so that you can look--if you are going to experimentally try to estimate what the impact is going to be on those fissure rooting zones, you want to have some kind of idea what sort of temperature variables to work with. So, I would recommend that you take a hard look at what those estimates are going to be and how solid they are on their prediction.

DR. OSTLER: Our hydrologist wanted to comment. Tom?

MR. BUSCHECK: Tom Buscheck.

The air coming out of the fractures will be in thermal equilibrium with the surface temperature. You weren't inferring that there would be any temperature difference were you?

DR. CANTLON: Well, you've got a moisture difference if you are going to drive moisture up that system.

MR. BUSCHECK: But as you saw earlier, hopefully the conduction in the upper 250 meters of the mountain, conduction was dominating, so it would be impossible for air or gas in the fractures to be out of equilibrium with the matrix temperatures.

DR. NORTH: Any further comments or questions?

MR. JOHNSON: Cady Johnson, M&O.

In reviewing the biotic surveys that have been done for the project, I was not able to find any type of a refere- nce or inventory of the non-vascular plants, specifically the mosses. And the reason I think that is interesting or it would be interesting to look at those, is because at Ash Meadows where you clearly do have air exhaust from the frac- tures, you can feel it blowing in your face, you've got two characteristics that you might use to recognize an outcrop that is affected by air exhaust. One is its thermal signa- ture and the other one is in association with moss. And during the winter months when fractures blow the moist air, there are growths of bright green moss that bloom in the orifices. The thermal contrast on those outcrops between fractures and matrix on the order of 10 degrees.

The same thing is true at Yucca Mountain and there is an association with moss on those same outcrops. But, there has yet been no clear demonstration that those outcrops are affected by air exhaust. Now if the environmental limits say on a certain type of moss were such that it would be restricted to some area where you would have supplemental moisture, it might be a very useful way to identify those types of environments for follow up study.

DR. OSTLER: Good point.

MR. JOHNSON: The question was, whether it is within your scope of work to look at the non-vascular plants?

DR. OSTLER: Certainly it is within the scope of work to look at those, yes. We have not done that to date. Most of the studies that we have been looking at so far to look at the impacts of site-characterization activities, not so much the impacts of a repository itself.

DR. NORTH: Any further questions or comments?

Then at this point we will bring the morning session to a close, a little bit more than an hour behind schedule and turn the meeting over to Dr. Price.

DR. PRICE: Welcome to the continuation of our meeting on thermal loading. I am Dennis Price, Chairman of the Board's Panel on Transportation and Systems. We seemed to have bucked a headwind this morning as far as schedule goes. And being an old pilot, I know that winds change in the afternoon and maybe we'll catch a tail wind and catch up and be on schedule. That is no comment on the expected headiness nor cerebral level of the presentations that are forthcoming.

Thus far we have heard some interesting discussion of uncertainties associated with alternative thermal loading concepts. It is clear from these presentations that some uncertainties relating to the effects to thermal loading on the repository host rock will likely persist. However, if the public and the technical community are to have confidence in the long-term safety and performance of a repository, it is essential that these uncertainties are addressed and reduced to the lowest possible level. Efforts to deal with these uncertainties might include cooling or aging of the waste; putting less spent fuel in the canisters; changing the spacing of the waste canisters within the repository. We'll discuss these issues in the remainder of the afternoon.

We also will hear how enhancements to the current repository and waste package designs such as a engineered barrier system might reduce uncertainties relating to the effects of thermal loading. Other considerations such as those associated with repository testing, near-field environ-ment testing, and waste form and materials testing, will be the subjects of several of this afternoon's presentations.

We also look forward to hearing a presentation on interim storage from the Canadian perspective. So, we have a great deal of ground to cover this afternoon and I am simply to going to turn the meeting over to Mike Cloninger who will introduce our first speakers.

Mike.

MR. CLONINGER: Thank you. I don't see the mobile mike handy, so I am going to have to shout. I have a few viewgraphs, but I think I can do it.

We are running way behind time. This afternoon's session is implications of higher and lower thermal loading. These are implications to the design and testing programs.

There are system-wide total waste management system implications to higher and lower thermal loading within the repository. In fact, the impacts go upstream through transportation, storage, MRS, even the operations of the reactor utility companies spent fuel storage pools. The focus today though is on the Mine Geologic Disposal System and the design and testing implications there.

Focus will be on reducing uncertainties, not necessarily on reducing thermal loading itself. As far as actual programmatic decisions by the Department of Energy, these will follow studies of the total system, including from reactor transportation storage, transportation to repository, emplacement, repository design system, system trade-off studies to be done in the near future. Those are yet to come. The decisions are yet to come. But, I have to remind you that the current focus of the program at Yucca Mountain is on site characterization, site suitability of the site as it exists today to see if there are any show stoppers.

Originally, we had a session on waste package engineered barrier design enhancements for this. It looked like it would take us about a day and a half to cover that subject. We did in June in conjunction with the Board have a workshop in Denver on this very subject. We will in the future cover this in more detail with the Board. Just let me summarize that for higher waste package temperatures, higher thermal loading which will occur almost without regard as to how we load a repository within the reasonable future, we need to look at redundant barriers during the thermal period to protect these waste forms from the steam air environment where the alteration rates of steam air oxidation is fairly significant, regardless of waste form, spent fuel or waste glass. Furthermore, that outer barrier for that period, will probably be a corrosion resistant barrier, and also, must be creep resistant at those higher temperatures. Soft materials that will creep, deform at high temperatures wouldn't be the ideal barrier during that period.

Lower waste package temperatures, that's in-evitable. We can't change the laws of physics. This place will eventually cool down whether it is Yucca Mountain or any other repository, it will cool down near ambient eventually, and we must design for the possibility of water. There again, you design for that eventuality, that assured even-tuality probably, again probably corrosion allowance materia-ls and absorbent packing materials. However, if we go to high thermal loadings, many of the absorbent packing materia-ls that we might consider putting in originally might not survive that thermal period. They may alter such that they no longer provide that function. What we would be looking for is something that corrodes to become an absorbent packing material at some point in time.

A very brief summary, we can go into much greater detail if you like on those aspects of waste packaging, EBS design, but like I say, we tried to design a talk for that and it looked like it would take a minimum of a day and a half.

So, given that, today's program, again repository, not waste package design enhancements will be presented by Dr. Tom Blejwas of Sandia. He will follow that immediately without break in other testing considerations that go along with that design process as well as the site-characterization process.

The near-field environment testing considerations will have to do with the waste package, engineered barrier system environment and its evolution with time, will be presented by Dr. Wunan Lin from the Lawrence Livermore National Laboratory. And again Greg Gdowski, from Livermore will present the testing aspects of the waste form and container, waste package, EBS, materials testing considerations.

That will be followed up later today by the invited presentations from the Board from the international community and others outside of the Yucca Mountain program.

Just briefly, tomorrow the DOE presentations will continue with the overall total system, total high level waste management system, comparative costs due to various thermal loading considerations by David Jones from Weston; regulatory and legislative considerations regarding thermal loading from Mike Lugo, formerly of Weston now with SAIC; and then an overall total system performance concept considera- tions from Mike Voegele from SAIC. I'll try to provide a comprehensive summary tomorrow morning as a lead-in to the round table discussions at that time.

Thank you very much. I would like to now introduce Dr. Tom Blejwas from Sandia.

DR. BLEJWAS: Thank you, Mike.

Well the title of this first talk for me this afternoon is Repository Design Enhancements, but I wanted to clarify what I mean by an enhancement, because, I think enhancement is one of those words that means a lot of dif- ferent things to different people. I have narrowed this talk to deal with enhancements as those kinds of things that we would do to a repository design that would change the temper- ature environment within the repository or within the geolog- ic repository surrounding the engineered barrier system. So I am not going to talk a lot about impacts. That's what I think we heard about this morning. Really what I am talking about are what kind of changes could we make. And the talk I gave yesterday actually covered a lot of this so you are not going to hear a lot of new material.

So, my outline looks like this: talk a little bit about design goals; design trade-offs, I'll give you a little bit of amplification on the plans I mentioned yesterday and then some conclusions. What we see as the goal, the design goal for the future is to design a repository system that needs performance objectives with an acceptable level of uncertainty. And some of those performance objectives deal with the thermal environment and some of the uncertainties deal with the thermal environment. I am going to concentrate my talk a little bit and try to focus in on those two areas.

I wanted to remind you some of the things that I said yesterday. In terms of uncertainties, we see the uncer- tainties coming from the mechanistic impacts of the thermal loading on the repository, and then those mechanistic effects in turn cause performance uncertainties. And we want to design against those uncertainties, but the approach that we have chosen and we plan to continue to use for the future, is to try to develop temperature goals or other goals, most of the goals would be temperatures however, that would minimize to an acceptable level or reduce to an acceptable level, the mechanistic uncertainties and then in turn the performance uncertainties. So the process and design is not to directly deal with those uncertainties, but rather to deal with them through temperature goals.

I put this next viewgraph up with a lot of trepidation because, yesterday is the one I got all my questions around. I am not going to discuss this viewgraph again in total. I really just wanted to point out that the area we are really concerned about is really design alternatives. What alternatives can we have that might change the thermal loading of the repository? I have amplified on this viewgraph though, what I mean by system assumptions. We are talking about waste characteristics, waste receipt, pre-packaging and etc. Those kinds of things that would come to a repository that the system would determine as opposed to things that we would look at for the design of the repository itself. And, the things I put on this viewgraph for design trade-offs, shouldn't come as a surprise to you. You are people that could easily recognize, particularly after a day and a half the kinds of things that will change the temperature environment in the repository. But, let me just go through some of them real quickly.

I wanted to emphasize the idea that when we deal with the design element of spacing, that is the amount of area we have, the volume of waste we are going to place and how much flexibility we need in our emplacement schedule, that generally we can get a hotter repository if we have a larger waste volume in a smaller area, and the inverse would be true. But I also wanted to point out that this is really the primary area that the waste package would have on the temperature for the repository. In other words, if you try to come up with enhancement schemes, different materials for a waste package, various packing materials, you are not going to change significantly the temperatures of the rock mass around the package. And that is part of the reason that is not included in this talk.

The real effects in terms of temperature come in how much waste you are going to put in the package and how far you space them apart. So that is the primary thing you would change. Also, very significant would be schedule, early emplacement, all these other things being equal will cause higher temperatures in general. Delayed emplacement would cause colder temperatures. There might be some confusion because in the past other speakers have talked about for example 60 year old waste and you see higher temperatures out in time, well that 60 year old waste but with the same areal power density, initial areal power density. So, what I am talking about here is just generally with these other things being the same, without keeping areal power density constant, what effects can we get from these changes.

We can also make some improvements on lay-out and you saw from Eric's videos yesterday, that as we vary the lay-out, you can get cold spots, hot spots, etc. So, for example commingling the waste can tend to smooth out the temperatures, where if we separate the spent fuel and the defense waste, we would end up with some hot spots and some colder spots. So, we can make changes that way.

Also, you saw that ventilation can have some impact on the overall temperatures, and I know you are going to get some discussions on that from others. But, clearly if we ventilate a lot, we are going to tend to reduce the temperature of the repository. Backfill is probably more of secondary consideration if we backfill early, obviously we don't have the option to ventilate and it might create some problems with respect to heat.

Now to give you an example of the kinds of things we have looked at a little bit in the past, we didn't specifically try to optimize the temperature of the repository, but we did consider the effect of options on the temperatures within the repository. So, for example, we looked at the effect of the orientation for the emplacement of the waste, whether horizontal or vertical on the temperatures that we would get in the boreholes. So, this was done during the conceptual design for the site characterization plan.

What we found is that if we went with a vertical emplacement, we end up with a curve that looks like this with a peak temperature that is almost 230 degrees, but it is indeed below what was at that time the design goal of 235 degrees. If we went with a horizontal emplacement and I should refresh your memory, that during the site characterization plan, horizontal was very long horizontal. We had a much smaller number of emplacement boreholes with a lot of packages in each one of those boreholes. With that scheme the boreholes are widely spaced, and hence we ended up with lower temperatures for peaks. So we did consider these kinds of things in the site characterization plan, but not aiming at optimizing temperature, just to look at the effects and insure that we made our goals.

And so I am going to repeat a little bit of what I said yesterday. This is what we see as our actions, and I hope that you agree after hearing over the last day or so some of the possible impacts of temperature on the repository that we will continue to perform mechanistic studies where they are appropriate, and we are looking for guidance from you to perhaps modify those plans, but we do have plans for looking at those. And then we'll update our temperature goals recognizing these factors that I mentioned yesterday. But, it is important to understand that we do plan to update those temperature goals and I expect the round table discussion will focus on that kind of an update. What should it look like? Again, we developed boundaries of design alternatives and performed the studies.

Another viewgraph that I used yesterday dealing with the overall approach to the design studies, establishing the baseline and then getting down to performing the study, revising the baseline, and I wanted to emphasize again that we expect to use expert judgment from a variety of groups and decision-aiding methodologies. A part of that expert judgment would come from an organization like yours.

This viewgraph, I will put up one last time. I got a little bit ahead of myself yesterday because of a question. What I wanted to point out at this point in my presentation, it doesn't appear in your package again, I didn't repeat it in the package, but it is essentially the same viewgraph, was the idea of what are we going to do if we don't meet our temperature goals? I mean, we always presume we will, but we know that we may end up with more tightly constrained temperature goals; temperatures that are lower than we had anticipated in the site characterization plan. That may result from the updating of our performance allocation process. So, what I anticipate us doing is updating this process, looking at the mechanistic impacts, the performance impacts, and based on a lot of expert judgment coming up with new temperature goals for the repository.

We will perform analyses based on these various inputs and we will end up with temperature profiles based on those analyses and hopefully the analyses will end up with us meeting those temperature goals. If they don't though, then we have to look at what things we can change. If we have already gone through all of our design alternative studies and we don't have anything else that we can consider for alternatives in the design, then we really have to go back to the assumptions. We have to find out do we have more area, for example, than we previously thought. That would be one way of changing the result. Or, we may have to look at the system assumptions which actually include the waste characteristics. And that would be the process that we would see ourselves going through, going through these studies if indeed we can't meet the temperature constraints, then we have to go back and change our system assumptions somehow.

And that generally is the approach that we would take. I will just quickly draw some conclusions on this part of my talk. We think that appropriate temperature constraints are necessary in the design process. I know there have been some people that have suggested that we should just immediately look at impacts, but temperature constraints really make the design process a lot easier to conduct, and it is almost an untenable process without those constraints.

We will contact design trade-offs that will include consideration of temperature changes, whether they be higher or lower temperature changes. And you have seen over the last day that there are some views that we should be looking at a higher temperature repository instead of a lower one. But, certainly, that would have to be something that would have to be worked out.

Finally, we anticipate doing these trade-offs during advance conceptual design. Now that is putting off some of these studies over the near-term, but I think it is important that we continue on with our exploratory study facility, based on the assumptions that we've made to this point in time, and as we get to an appropriate design stage to revise those.

I wanted to mention that part of the reason that I've given a relative general view of how these studies would be conducted is because the project is bringing on a new M&O contractor and logically the advance conceptual design would be planned in detail by that organization, and that planning hasn't taken place at this point.

Should I entertain questions in this part before I go to the next talk?

DR. PRICE: Why don't you just continue.

DR. BLEJWAS: See, we got a head wind already--or a tail wind.

The next talk, I had better not skip the title viewgraph is Repository Testing Considerations. And I wanted to emphasize the idea that this is repository testing. The interpretation here should be those tests that are designed to look at repository type design questions, as opposed to tests that would be looking at the waste package for example.

What I am going to cover very briefly is a look at those experiments that are potentially affected by a change in the thermal loading for the repository and I'll go through those very briefly for you. And then, what would be the effects of either lower or higher thermal loadings on those experiments. Would we want to change them considerably? Would we want to add new experiments? Expand experiments? Or conversely, might we want to eliminate experiments? Finally, I'll get to some conclusions.

From the perspective of experiments that are aimed predominately at the repository, there really aren't a lot of experiments that would be affected, because most of the experiments in the site characterization plan, or many, many of them are designed to look at what is out there presently. These experiments though are more designed to look at what would be the trade-offs with respect to design in the future of a repository. So, for example in the field, the kinds of experiments that we see being conducted in the exploratory study facility are heater experiments, heated block experiments, thermal stress measurements, a heated room experiment, and I'll discuss these very briefly in the viewgraphs that follow, and then a variety of laboratory experiments. They might impact what we did for thermal properties, thermal expansion. We have other properties that are temperature dependent and the ranges for those experiments would change. And then there are other laboratory experiments looking at conceptual models for example that would change also.

An example of the kind of fuel experiment I am talking about would be a heater experiment. And this one is aimed at looking at the affect of the heat on the rock mass around the emplacement hole, and this is just a cross-section of what that experiment might look like.

I wanted to point out a little bit about the analyses that we've done on some of these experiments to give you a feel for the temperature range that is to be included. This is a representation of the model that we did for looking at a vertical canister heater experiment. And when we did analyses using this model, these are the kinds of temperature profiles that we are predicting in situ for this experiment. These are three different curves that is three different radii from the center line of the heater. This furthest one .37 meters is actually the wall of that heater,. which is intended to simulate a waste package.

And as you can see, we are planning to be able to conduct that experiment up to a temperature of nearly 500°C; much higher than any of the temperatures we've heard over the last few days. And part of the reason for that is that we are trying to take the experiment up high enough so that we can actually induce some type of a failure, so we may never get to this point, but the intent is to be prepared to get that high.

You will also see that the temperatures in the rock mass, for example, a little over a meter away, where the temperature is nearly 300 degrees, where we are planning on limiting some of these temperatures to 200 degrees in the repository. At least those are our plans for the moment.

Even if we were looking at a repository that is quite a bit hotter than the one that we are presently planning, for, this experiment probably would still be adequate.

I did want to point out to you that we are basing these experiments on some experience we have from G-Tunnel and I know we have discussed some of this with part of the Board in Denver, so I am not going to spend a lot of time with it, but it does perhaps explain why I have confidence that some of our calculations will be fairly good in the future with respect to temperatures.

These are plots similar to the ones I just showed you, but except the solid and dashed line are calculated temperatures and then the various points are circles, triangles and squares are temperatures that were measured in the field from a G-Tunnel experiment. And you can see that in general, the trends were well predicted by the analyses.

Actually, the magnitudes are, but I will tell you that we did not adjust our thermal parameters in the model. We took the values from laboratory experiments and used them directly in the model and didn't adjust them, but I think that was just fortuitous. We happened to come up with very close correlation. There are problem some factors that would make it higher or lower, but still I think the most important things is the trends are very good.

Another experiment that would be very affected if we went to different thermal loading would be a heated block experiment, and I won't discuss that in any further detail. We have an experiment that we call a thermal stress test, and we were originally planning on doing a prototype test of this experiment in G-Tunnel, before G-Tunnel was closed. We still plan on conducting an experiment like this and an experiment design will probably go through some modifications with time. Generally the experiment consists of rows of heaters into the rock mass and then the variety of instruments that would affect the temperature in the rock mass and also measure the displacements in the rock mass.

Part of the reason that you see the heaters in the back or roof of the drift is because the idea was to heat this up high enough to see if we could induce a failure in the rock mass. And we would limit the amount of ground support that we would put into this facility.

DR. LANGMUIR: Tom, I bet that these studies are entirely being done by the rock mechanics people without discussions with the hydrologists or the geochemists. I suspect; I may be wrong. Historically, that's what it has been. And implications of this go to the hydrology and the geochemical implications of changes in the thermal regime. So these really ought to be Kermit's design and collaboration between these various people. What's going on here and what is the plan for this in the future?

DR. BLEJWAS: Well, I can tell you what went on when the experiments were being designed and there hasn't been much work since then. When the experiments were being designed, we had regular meetings of the people that were conducting the experiments in a variety of fields including the fields like hydrology. And those people would get together and talk about their experiments together. And indeed sometimes we would modify the experiments based on what other people would like to see in them. However, you are absolutely correct that these experiments were designed primarily to look at rock mechanics questions because they are oriented or based on design questions. What are we going to be able to do with the design of the repository. However, we do try to modify them to meet other needs.

Now in the future, I would expect as we get closer to being able to conduct experiments in the field that we will reinstitute that type of an organization where we regularly get together and discuss the experiments. I shouldn't say, the organization hasn't really been disbanded, there just hasn't been a lot of participating in the rock mechanics field, because, we are not doing any testing in that area.

We also have done analyses of this experiment, this thermal stress test. I wanted to point out here that we are aiming at heating the rock mass up to relatively high temperatures. The highest temperatures close to the heaters here are 400°C, but even as you get out to line D here, that is a temperature of 120°C, so the intent was to heat a relatively large volume of rock to a very high temperature to see if we could induce some failures.

A similar type of experiment, but was intended to be a much longer term experiment was a heated room test. And this is a conceptually arrangement for this test. Instead of putting heaters in a single drift, in this test what we have done is taken two drifts to the side of this experiment, as a matter of fact, this would probably be an excavation test experiment that then became a heated room experiment. We would put instrument in this drift, but also in these drifts, and then heaters, and the arrangements for the heaters; conceptually, this is not necessarily the final arrangement, we would modify those. But the intent all along was to obtain relatively high temperatures. So, for example, looking at a plot of temperatures 40 months after the initiation of this experiment, line D right here which goes completely around the drift and around the heaters is a line of 150°C isotherm. So, we are talking about high temperatures around a very large volume of rock, even with emplacement schemes that would lead to APD's on the order of 80 kilowatts per acre, this probably would still be a very good representation or upper bound on the kinds of temperatures we would need to look at.

DR. CANTLON: What is the figure within the D there?

DR. BLEJWAS: These?

DR. CANTLON: Yes.

DR. BLEJWAS: These are individual heaters, so they are heating this rock mass. Again, that arrangement might change. We might go to an arrangement where we actually had heaters in the floor, if we are indeed going to have vertical emplacement, it would take longer though with that scheme to get temperatures out here in the region we are most certain that that would be representative of the repository environment. That is the reason we considered that heater arrangement.

Also, Larry, this morning was talking a little bit about stresses. When we look at stresses at 40 months, what we see is that we get some relatively high horizontal stresses. Very locally, we get stresses as high as 120 MPA, but over relatively large regions we even have 60 MPA which is quite a bit higher than the kinds of stresses that Larry was talking about this morning. Again, trying to overdrive the system to see what kind of failures we could get.

I am not really going to talk much about the laboratory experiments, because I think some of the conclusions I have here are pretty obvious for the laboratory experiments. Instead, what I would like to do is just jump to what are the effects of lower thermal loading. I want to break this up into parts, what if it is a little lower, and I hope you won't ask me to define a little lower, because the conclusions are pretty broad here. A little lower, we probably are still going to have to do all our field experiments, because we are still going to have quite a bit uncertainty about the behavior of the drifts and the behavior of the boreholes.

We would however, probably be able to reduce the range of temperatures for our laboratory tests, and indeed, we would be able to reduce, probably the range of temperatures for some of our field tests. We would also have reduction in instrumentation problems, because, one of the biggest problems we have with field tests is trying to get instruments that will measure things like displacements accurately at very high temperatures. That problem would be reduced if we were looking at lower temperatures.

Also, the time required would be lower for some of the thermal mechanical tests if we reduced the temperature range at which the tests would be conducted. Now, what would be the effects of lowering the thermal loading if it was a lot lower? And here I am thinking more in terms of the kind of thermal loadings that Eric was talking about 20 to 30 kilowatts per acre where we could be relatively confident that we wouldn't get much boiling in the repository environment.

If it is a lot lower, I would recommend myself that we eliminate most or at least some of our thermal mechanical experiments, because, the questions of drift stability for example, would be reduced significantly and may not justify the kind of experimental program we presently have planned. We would clearly also have a reduction in our lab property tests.

Okay, now those are reductions if we go to lower, but what would we have to do if we have higher thermal loadings? From my perspective, since we have planned most of our tests for relatively higher thermal loadings than we really expect in repository, since we are trying to drive them to failure, I don't see us modifying significantly our field tests even if we wanted to build a repository that had higher thermal loading. I think it would slightly modify some of the experiments, but I don't expect the changes would be significant. We would, of course though, have to expand the range of some of our lab property tests, because we would have to be sure that we have properties throughout the range of interest.

And so, in conclusion, I think we can accommodate thermal loadings with possible changes to our testing program. We can accommodate most of the thermal loadings that have been discussed over the last few days if not all of them. And, I don't expect that the changes will necessarily cause any real perturbation to the present plans for the site characterization of Yucca Mountain. I expect those perturbations to be relatively small regardless of what thermal loading was chosen.

The present plans, and that is because the present plans can accommodate what I view as a wide range of temperature, so hence, the impact should be low.

Any questions?

DR. NORTH: I would like to reiterate a comment that I made yesterday on the need to tie these issues to performance assessment. You've given us a very general presentation here about how the thermal loading issues can be accommodated with, I'll call it a modest delta to the testing program. On the other hand, early in the presentation, you showed us a graph indicating a substantial difference in the borehole temperature between horizontal and vertical emplacement. Perhaps, this might be quite significant. Perhaps looking at horizontal drift emplacement, as opposed to borehole emplacement and it becomes even more significant. So, my hypothesis is there may be a lot of very important issues that must be dealt with later on down the line, where these tests could provide very significant information. And I think we will identify a lot of those when we get into the performance assessment and we will see more, what are the issues that are most critical in terms of overall repository performance. And, that seems to me very appropriate that we visit some of these questions a year or so from now when we have a DOE total system performance assessment complete, and ask, what are the implications now for the testing program and specifically what are the implications of the thermal loading issue for the testing program?

DR. BLEJWAS: I think that would be appropriate. My reaction though is, I think you are going to be disappointed in that showing thermal effects in a total system performance assessment is not only difficult but it is going to tend to get lost in the noise. So, I am not sure that you are going to see much even when we are able to include the thermal effects in the total system performance assessment, because, as you understand, it is a roll up of a lot of things into one single assessment.

Personally, I think these kinds of issues are better addressed with design specific analyses, performance analyses that are aimed at deciding whether something is better or worse without necessarily knowing whether that better or worse will exceed something like an EPA standard. And those kinds of analyses, indeed, I expect we will be able to look at better in a year, and we can show you some of those. But, the total system, the little that thermal will be included in it will not give you the kind of definitive answer you are looking for.

DR. NORTH: Yes. I think your point is well taken. We may have to look at the whole pyramid, not just the top part of it.

DR. BLEJWAS: Right.

DR. NORTH: We may also wish to consider in much more detail what you have called systems wide implications. Looking at MRS transportation, etc., and looking at issues such as what kind of a cask do we want to use. Do we want to use a multi-use cask for emplacement and consider what cost savings, and what savings in radiation exposure, etc., some of these considerations may imply.

DR. BLEJWAS: I agree.

I would like to say though in response to the early part of your comment that I think that the intent of my presentation was to try to give you confidence that we didn't need to stop now and redesign a testing program to ensure that it could accommodate whatever thermal loading that we are likely to come up with for a repository, but rather we think that that can be adjusted as we go on in the future. And that was the aim of my presentation; I hope that came across.

DR. DEERE: Tom, I can see in one configuration, where if there were in-drift emplacement, it would change the layout of some of your tests.

DR. BLEJWAS: That is correct.

DR. DEERE: It probably would be easy to accommodate it, but it might make it simpler in some cases; it might make it more difficult.

DR. BLEJWAS: Yes. And there are likely to be a lot of things like that considered that would cause some changes to the test program. I didn't mean to imply we wouldn't have changes, but I agree with you; that would be a significant change.

DR. DEERE: And by the same token, if we were looking seriously at TBM excavated circular emplacement tunnels, would also change considerably the results of the studies that had been presented.

DR. BLEJWAS: Right.

Of course, we are going to have to adjust the design of our experiments to whatever the concepts are for the repository. So, as we go to more reliance on mechanical excavations such as TBMs, whatever shape openings we are going to deal with in the repository are experiments we also intended would be adjusted to try to represent the repository as best we could. Good point.

DR. PRICE: Any other comment or question?

If not, we will proceed to the next speaker who is Wunan Lin from Lawrence Livermore. His topic is Near-Field Environment Testing Considerations.

DR. LIN: My talk also was designed to give you my view of what high or low thermal loading would have impact on the uncertainty of testing. I am not supposed to tell you either high or low thermal loading is good or bad.

DR. PRICE: If you have an opinion, can we catch you in the corner other time?

DR. LIN: If it is off of record, yes.

I would like to say that engineered barrier system should include not only waste package but also the near-field environment. Dr. Larry Ramspott said they mentioned that near-field environment is also a part of the repository.

The main concern here is the quality of water in this environment. And by quality I mean the chemical property of the water.

So I'd like to cover in this talk about the tests that are required to understand the moisture movement or moisture content in the near-field environment. Now, I am not supposed to bore you with the test results, I've been told not to, but I cannot resist the temptation of showing you some of the results just to show you that those tests can be done and that we are doing it.

You have heard Tom Buscheck's very detailed analysis of the hydrologic property in the repository including the near-field environment. This is my layman summary of conceptual model. You can see that in either high or low thermal loading cases, we are going to deal with temperature. Of course, on this side the temperature may be higher than this side. We are going to deal with thermal cracking maybe more on the high thermal loading case than the low thermal loading cases, but some of the rock may start some thermal cracking at a temperature as low as about 50°C, simply due to the difference of thermal expansion of different minerals.

The dehydration may be more in the high case than in the low case. Going to have drying in the high thermal loading case, maybe not much in the low thermal loading cases. It may take a very long period of time before we can observe some of the moisture movement in the low thermal loading case.

In the rehydration and infiltration, when water comes back it may imbibe into the matrix more in the high case than in the low case. And, of course, geochemistry you have to study both cases except that in the high case you may have mineral left behind the dehydration zone. Flows, you have to study both fracture flow, matrix flow and also in the both cases.

Borehole stability may be more in high case than in the low case. So, my conclusion, before I reach the conclusion part, is that similar tests are required for both cases; and, you may see that at the end of the talk too.

I am going to cover both laboratory tests and in situ tests. In the laboratory testing, I'll cover fracture healing, model validation experiments, matrix property measurements, hydrology and nuclide adsorption experiments; this is particular design for the performance analysis.

In the fracture healing experiments, we try to study the fracture healing at different pressures and temperatures. Our experimental results so far indicated that fractures begin to heal, and when I say heal I mean decrease of permeability, at a confining pressure of 50 bars; when temperature is above 90°C, it doesn't have to be boiling; when you have water flow all steam, I think probably is wet steam, flowing through the fracture. Now, in the parentheses, I indicate that this test has to be done for both high and low thermal loading cases.

In the model validation experiments, we study the effect of temperature on the flow of water and vapor. And we do this in a very close collaboration with Tom Buscheck and John Nitao. We start imbibition and drying of rock samples; we started condensation around fractures; and, this probably only needs to be done for the high thermal loading cases because boiling is going on.

We start a fracture flow versus matrix flow, and this has to be done in both high and low thermal loading cases, maybe more on the high thermal loading cases. We are then going to put together a laboratory scale heated block so we can have an integrated study and of course that needs to be done for both high and low thermal loading cases.

To show you some of the results that we have got so far, this is impedance imaging of a rock sample with a longitudinal fracture at about that location. And we start the sample with total saturation, gradually dry out the sample and you can see that as Tom mentioned to you this morning, that drying started at fracture zones gradually expends into the matrix area then of course the sample eventually becomes totally dry. Then we rehydrate the sample putting water back into one end of the sample and you can see that unfortunately the rehydration is not a reverse process of dehydration. This kind of thing needs to be taken care of in the model analysis.

Another experiment we did and this is prototype testing in the laboratory, therefore we did not use Topopah Spring Tuff, instead we used plaster of paris, that is easier to do. You can see the fracture flow, we put blue dye water pounding on top of the fracture which was about 25 micron aperture. The fracture flow through the sample, you also see matrix flow into the matrix part or the sample.

In the matrix property, we measured suction potentials to a temperature of 160°C; we measured thermal cracking, this probably needs to be done especially for high thermal loading cases; we measured permeability, both water permeability and gas permeability, and of course this has to be done on both cases; we measured Klinkenberg coefficients and this was for Tom Buscheck to calculate the vapor diffusion into the rock samples.

Some of the results that we obtained in laboratory, the suction potentials was the saturation level in Topopah Spring Tuff at 20°C and 70°C. When the sample is wetting, imbibing the water, you can see that it is at higher temperature because a smaller surface tension of water, you have got smaller saturation level at the same suction potential. This is when the sample was imbibing water.

When the sample was drying out, the same kind of situation except that the difference between the room temperature and 70°C is much significant in this case, when the sample was drying. So this kind of thing may have to be taken care of in the model calculations.

Hydrology and adsorption of nuclides, we have just designed a system that can be operated at the temperature of 150°C, and can be used for various confining pressures and pore pressures, and this is to study the effect of adsorping the nuclides to the hydrology. This would be studying fracture flow, along with nuclide adsorption in the sample.

Turning to the in situ testing, the in situ testing is an extension of laboratory testing and also intended to use it to validate Tom Buscheck's model. In that case we want to study hydrologic, geochemical, geomechanical responses of rock mass to thermal loading. We can use various power outputs of heater, so that we can overdrive a bigger rock mass, therefore, Tom can test his model at greater range of conditions. And this kind of test needs to be done in both high and low thermal loading cases and we have completed our first prototype testing in G-Tunnel. We would really like to emphasize that prototype testing is really necessary for this kind of test because we learn a lot and learn some surprises from geothermal tests.

DR. LANGMUIR: Wunan, are you doing these tests in collaboration with researchers at the other labs, because for example, Los Alamos was doing adsorption work prior to this time. Is there interplay between the laboratory researchers in these areas?

DR. LIN: We would like to. So far in these geothermal tests, we have not done that yet, but they are being considered in the future tests.

In situ testing, we measure those things including temperature as functional space and time; moisture content as functional, temperature space and time; gas pressure; borehole stability; air permeability before and after heating; infiltration tests; and we sample rock sample and water and gas samples.

In G-Tunnel test we didn't do all this, we didn't have infiltration tests, we didn't include the borehole stability, and we sample some water samples but not gas samples.

In situ testing, the method that we are going to do obviously, is use thermocouple to measure temperature and that can be used to measure very high temperature. We use neutron and density logs to measure the moisture content and we have learned how to use that in the high temperature region. We use high frequency, a tomograph to get semi qualitative or qualitative study of movement of moisture in the rock and that can be done for both high and low thermal loading cases.

Microwave resonator that was designed in Livermore to measure the moisture content in the rock and that is, I think it would be a high temperature so that can be used for both cases.

Thermocouple psychrometer was also used to measure the moisture content or relative humidity, but that would be better just used for low temperatures because of the calibration problems. Geotechnical instrument that would behave better at lower temperature as Mr. Blejwas mentioned to you a moment ago.

Some of the results that we got from G-Tunnel test, this temperature measurements, I'll just show you from four different thermocouples, 86, 87 were just almost right below up in the heater area; 88 and 89 are to the site, but below the heater horizon to the site. You can see that boiling was prolonged in 88 and 89 region; we didn't observe that in the thermocouple right below the heater.

Another G-Tunnel result, this was measurement of change in moisture content from neutron logging, from area below the heater and at side to the heater and above the heater. This is just to show you that we were able to measure the change, the dry out or the region or change of the moisture; also, rehydration region after the heater being turned off, totally being turned off at this point. And you remember Tom mentioned to you this morning that rehydration was much slower than dehydration.

DR. DEERE: Were these measurements that were made in boreholes that were above and below?

DR. LIN: Yes. And, by the way it is about .8 meter from the heater.

DR. DEERE: It looks like it is quite sensitive in picking up.

DR. LIN: Right. Actually, this is indicating that the rock in this region by this time is almost dry, totally dry.

In conclusion, as I mentioned at the beginning, both high and low thermal loadings require similar testing. Of course, if we go like to Dr. Blejwas, very low thermal loading, you probably don't need to those testing.

Technologies exists for both tests. We don't have to invent new wheels in this case. Some instruments are more reliable at low temperature, however, some parameters for instance to define the dry zone and saturated zone will be more detectable in the high loading zone, high thermal loading cases.

Thank you.

DR. PRICE: Questions or comments from the Board?

If not, I think perhaps what we ought to do is take a ten minute break at this time and then we'll finish up. I think there is a basic physiology. I don't think it is related to head wind or tail wind, but a basic physiological need. We'll take a break.

(Whereupon, a recess was had off the record.)

DR. PRICE: The next presentation on Waste Form and

Materials Testing Considerations will be by Dr. Gregory Gdowski from Lawrence Livermore. Go right ahead.

DR. GDOWSKI: This talk is about the effective thermal loading on waste form and materials testing considerations.

The outline of the presentation, just a brief introduction defining the low and high thermal loading considerations; discussing the testing considerations for both the low and the high thermal loading; some other testing considerations on materials and finally just to summarize.

For the purposes of this presentation, define two thermal loading scenarios; a low thermal loading scenario where the temperature always remains boiling, that is we are primarily only considering aqueous phase degradation of materials; then we have a high thermal loading scenario where initially the temperature will be above boiling but will eventually go below boiling.

First I'll consider the low thermal loading testing considerations. The bulk of the testing would be done at low temperatures. We would be interested in the degradation of the container materials and Zircaloy cladding. This is basically all the phenomenon which I mentioned this morning, such as localized corrosion, pitting and crevice corrosion, general corrosion, hydrogen effects, biological effects.

The type of testing that will be emphasized will depend on the waste package design, whether it calls for a corrosion allowance material or a corrosion resistant material. Other testing for this scenario would be the hydride precipitation and the reorientation in Zircaloy cladding. The oxidation and dissolution of the UO_2 pellets and the hydration and dissolution of borosilicate glass.

Also, some high temperature testing would be required even for the low thermal loading scenario, such that we could do accelerated testing in order to characterize and model some of the very slow occurring phenomena at the lower temperatures. However, we must use caution in doing these high temperature testing, because, we must ensure that the mechanisms of degradations do not change with temperature.

Now onto the high thermal loading testing considerations. A number of tests can be required to be done at high temperatures, such as aging and oxidation of the container materials. We need to know what the effects on the micro structure will be from long-term aging at high temperatures. We also need to consider other degradation modes of the container materials, such as what are radiolysis effects on the container materials.

Other high temperature concerns include the creep/stress rupture of Zircaloy cladding; hydrogen effects in the Zircaloy cladding; the oxidation of the UO_2 fuel pellets; hydration of the borosilicate glass. And also as before, we will need to do some accelerated testing of the low temperature phenomena which will occur when the waste starts to cool down.

The lower testing considerations for the high thermal loading scenario will include all those degradation phenomena which I mentioned previously for the low thermal loading scenario. But, the tests will have to be modified in order to reflect the changes that have occurred in the materials by the high temperature processes, such as, what is the effect on the dissolution of the fuel pellets which have been oxidized to U_3O_8/UO_3; what is the effect of the dissolution on highly hydrated borosilicate glass; and what is the degradation resistance of aged and oxidized container materials?

Some other testing considerations for the waste package; we need to consider what is the effect of the backfill on the container materials, whether it is bentonite or crushed tuff, how do they effect the corrosion resistance of the oxide layers that form on these container materials? We also need to consider waste package component interactions, the effect of the corrosion products from one material on the other also need to be considered. And, also, we need to consider the final closure usually the most susceptible part of a structure to corrosion processes.

In summary, we have identified the degradation phenomena and concerns for both high and low thermal scenarios; testing will be required to characterize and model and determine the significance of the degradation loads of the materials and waste forms; and, testing should also proceed simultaneously with engineered barrier system design.

That is it.

DR. VERINK: Greg, I'd like to ask you a couple of questions. You have not listed what container materials you are going to be testing.

DR. GDOWSKI: Well, I think that depends on what we decide or the engineered barrier system design.

DR. VERINK: You have no ideas about that?

DR. GDOWSKI: Well, we have ideas, yes. And, if we are going with the conceptual design, we have decided that we would look at materials such as titanium based materials, nickel chrome molybdenum materials, and LIA-25. Now if we go to a thick walled container material, then we would have different considerations there.

DR. VERINK: You said, if, have you already decided that there is nothing but the thin wall?

DR. GDOWSKI: I don't think the decision has been made on the design of the waste package, no.

DR. VERINK: Then what would you consider for the others?

DR. GDOWSKI: For? I'm sorry?

DR. VERINK: What would you consider for the other than thin wall?

DR. GDOWSKI: Materials such as carbon steel, if you were going to a thick walled waste package in order for radiation shielding; we would consider that.

DR. VERINK: Is that all?

DR. GDOWSKI: If we go back to the thick-walled self-shielded container, we would also consider copper alloys, because radiation and corrosion of copper materials is a problem if you don't use self-shielded.

DR. VERINK: You didn't make any mention in your exposition here about filler materials, is that--

DR. GDOWSKI: Oh, that would be a definite concern. I guess I just forgot to mention that when I was talking about waste package component interactions. We would have to consider what the effect of filler materials are on the other components.

DR. VERINK: What would be some candidates for that?

DR. GDOWSKI: For the filler material? I guess we could consider--

DR. VERINK: What is your plan?

DR. GDOWSKI: What is planned for that?

DR. VERINK: Yeah.

DR. GDOWSKI: At the moment there are no plans for that. It is in the developmental stage. There is no testing of filler materials.

DR. VERINK: So you say lead is one of the ones that you might then consider?

DR. GDOWSKI: Yes.

DR. VERINK: What else would you consider?

DR. GDOWSKI: As a filler? I would think if we were going to go with the filler, we would also need to consider a thick walled container for shielding purposes. Other than lead, I am not really aware of what else we would use.

DR. VERINK: The comment having to do with degradation of container materials and Zircaloy cladding, I gather you are not implying that that is a galvanic corrosion problem, just putting them both in mind.

DR. GDOWSKI: No. I just put them both together.

DR. PRICE: Are there other questions?

Now we will shift gears just a little bit and have speakers who are outside of the labs at DOE and at this point it is my pleasure to introduce to you Mr. Peter Stevens-Guille who is head of Radioactive Materials Management for Ontario Hydro in Toronto, Canada. Mr. Stevens-Guille leads a group 30 engineers who are responsible for nuclear materials management. Today he will speak on a candidate interim storage concept that may be used for disposal.

Mr. Peter Stevens-Guille, thank you for being with us today and we look forward to your presentation.

MR. STEVENS-GUILLE: Mr. Chairman, Dr. Deere, I would like to thank you and the members of the Board very much for inviting me here today. I am very grateful to be here.

Ladies and gentlemen, I would just like to tell you a very brief tale. This is my first trip to Las Vegas, so where did it find me last night at about 9:00 but on what you call the strip with a nob in my hand and quarters in my pocket. And what did I do? I lost the lot, but before I did I had one win. The one win, the numbers came up and I am new to this business, never done it before, and a little device came on the screen or whatever it is, and I said to my colleague, what is this? Oh, she said, it was a she, a wild card. So, I made one win, but I lost it all. But anyway, tonight's another night. But I realize though that the wild card was very characteristic because I had been searching around for an introduction to this talk and I discovered what it is; I am your wild card. And in fact this talk is a wild card, because it is something rather different from what you've heard. I am a different person; I am not a researcher; I am not a scientist. I come from utility; I don't think there are many of us here on the ground today. So you will bear with me; it is quite a different talk. It is coming othogonally, coming at right angles to everything you have heard today. Just bear with that, if you will.

The first part that is a bit othogonal is the title, because this was the title I agreed upon to talk and of course it is a slightly different one than in your program. But, if you will stretch your imagination quite considerably and think about interim storage as being a way of devising an engineered barrier, certainly which extends in time and space, I guess as well, the period of aging fuel, then there is this very tenuous connection to the title of this talk, which I am now going to give you to the one in your program. So, you'll just have to bear with me.

I guess the other thing that I should mention, I hope I am going to surprise you twice, this afternoon, and I don't think I'll make you fall asleep because it is going to be short. The first surprise is Ontario Hydro up in Canada, Ontario, is one of the largest provinces in Canada. This is a publicly owned utility which has the largest nuclear program in the continent. In fact, it has the second largest nuclear program in the whole world, Electricity du France, is of course as you know the largest one.

Now, just in a word about so-called nuclear Canada, amd what we've got. We have 22 reactors in operations, at least most of the time; one in Quebec; one in New Brunswick; 18 in Ontario which I am sort of representing here today; and, then we have a further two under construction. And if we didn't have a new change of government in the last year which has socialist tendencies, we wouldn't have a moratorium on building new plants in our province, anyway.

So, 15 percent of Canada's electricity is nuclear. Canada traditionally has a very high generation of kilowatt hours and kilowatt per capita probably as high if not higher than the U.S., at least our anti-nuclear people are always talking about us as energy pigs, that is the expression they use, so there must be something and you can turn it around and say it is a good thing to an audience like this.

Just a word about Ontario Hydro then, five nuclear stations; we group our reactors; we tend to build stations with four units at a time. I'll show you a picture of one of those. We have got 13,200 megawatts in service and 50 percent approximately of the electricity that is generated in the provinces nuclear, and therefore would probably rise to about 60 percent.

Here is what I hope to be the other surprise and you may note a little bit from yesterday, this is the amount of used fuel in North America in your whole country and this is a very reliable number, I've taken it from one of your publications, this is not quite such a reliable number; I've taken it from one of our own. So, I want to show that right now, we have 21,000 MTUs. I think there are some awful units around. Kilowatts per acre is just ugh. But, anyway MTUs, it should be megagrams if you are an SI purist like myself. , but, anyway in the year 2,000 you will arise to about 40,000 tons and we will be up there at 32,000 tons. So, I just wanted to show that one owner has a very high stake. Our fuel of course is not the same as yours and I had better explain just what it is.

As you know you saw a picture of a CANDU fuel bundle yesterday; it's natural uranium, and it has got a very loud burnup. We pass it through very quickly. The advantage is that it is cheap. There is no enrichment other than what is naturally there. And so that is why there are these very high fuel arisings. It is just the nature of the fuel; it is a physical phenomenon.

Just another word about Ontario Hydro, this is not blowing trumpets or anything, but explaining the different institutional responsibilities between Canada and the U.S., because they are germane to this talk and they may be germane to other things as well. We've got a responsibility for safe management of all radioactive materials, and particularly used fuel that we are discussing today. And the responsibility goes right from the production, sort of from the cradle to the grave; production, storage, transportation, immobilization. That is a responsibility. It doesn't mean we do all the work. You have heard from Gary Simmon that should be here listening to my talk, that AECL a federal government lab is carrying out to work on the disposal technology, jointly funded by ourselves, I might add.

But this is a responsibility that the federal government lays on all waste producers. And that is an interesting thing because it leads to a focusing of the mind, so that unlike utilities in your company, it doesn't stop at the plant gates. The utilities here, they use fuel, they literally cannot ship it out of the plant gate because that is a responsibility of your Department of Energy, so this is a fundamental institutional difference. It is neither good nor bad; it is just a phenomenon.

Now, recently we have developed in our company, we have published and it is available if anybody wants it, just leave a business card, we have published a long and complicated plan. I am showing you that because you can't read it, not the details anyway, but I am putting up a very simplified version. It is a very straight forward plan and what the good part of it is is that it is a public document; it is a commitment by utility which has got 36,000 employees to what it is going to do. And it consists of a storage component, a transportation and a disposal. And unlike in the U.S., we are taking a more leisurely time for disposal.

Our in service dates for a repository is 2025, and this is being worked out from what we considered to be reasonable times for both the technology to develop and for the public hearings of which will be extensive to be done and undertaken. We have layers of federal and provincial public responsibilities to meet. We are involved in some of them.

The so-called Canadian disposal concept, not a design, a concept, is now undergoing review in Canada by the federal government. And before, if that concept successfully meets the requirements of the federal government, it doesn't mean we can go ahead and build it, because each stage, site selection, and the rest of it, even transportation, is almost inevitably going to have to be subject to further public review, as indeed it should be, I think.

But, what I am going to talk about today is really interim storage. It is shown here, it is not quite correct in this diagram because it is shown here as coming in--well, I guess it is correct. The interim storage could be for a period of up to 50 years; we usually would extend its storage for longer periods of that. If for instance the public does not agree with our plans for disposal, as indeed they might, then we would have to do something. And what would we have to do? We would have to continue storing fuel either in a central location or at our station sites for a very long time. So this is the subject of the talk today.

I just wanted to mention a point that hasn't been raised. In Canada we spend a lot of time on what we call spent fuel integrity program. We use the word spent fuel, and used fuel somewhat interchangeably. I think the more international use is just spent fuel. We try to use a more simplified word, used fuel for the public because we think it is easier to understand.

We've got some fuel bundles which were first generated in 1962, I think they were, so they are almost 30 years old. And we have certainly done some comprehensive examination of 27 year old fuel. And we believe from a long series of tests and so on which have been reported internationally, it will be at the upcoming ANS Conference I believe again, which is going to be held in San Francisco in November, that the fuel should be able to retain its integrity under water for at least 50 years, probably longer, but we would want to make a claim of 50; whereas, fuel in dry air, if we can keep it at less than 100°C for probably 100 years. If we had it in another gas medium such as nitrogen or helium, it would be longer, or else the temperature could be higher, because 100°C is a pretty mild temperature, particularly after what we have been hearing.

This is not new of course, but I just wanted to discuss some of the, well, let's just step back from that for a minute. As with all utilities and all utility reactors in the world, we discharge our fuel underwater. That is an integral design of our utility stations. And we will continue to do that. And at the moment, we have storage in Canada for 33,000 megagrams, 33,000 metric tons. But, in certain areas that space is kind of being used up. So, we are not considering very seriously going into dry storage in a very large scale way, and I will explain the magnitude of this in just a minute.

But, first of all, just a background, dry storage is being used in Canada since '74. We've got over 600 megagrams or 600 metric tons in dry storage right now and it is behaving perfectly. We have demonstrated in Ontario Hydro dry storage since 1988. When I say demonstrated, that means we have a licensed facility, licensed by our equivalent of your NRC, and also incidently as we have, I would like you to satisfy the regulations of the IAEA with regard to international safeguards on the non-proliferation, etc., etc.; this demonstration has been under their control.

We use containers of about 60 tons mass and they are built of steel and concrete, heavy concrete, illminite concrete for low cost. The interesting trick is we are designing these containers for transportation and for those of you in the business, the B(u) container is the one that has got to go through, you drop from dizzy heights and is subject to impacts and puncture tests, fires and being stuck under water for long periods of time, which I will discuss very briefly in a minute.

The container that we have developed, looks a bit like this. It is rectangular in planned form; it is quite large. I should put a picture of a human being here. A human being would be about that high, relative to the top being the head. This is a CANDU fuel bundle then which is about as big as from here to here (indicating), and weighs 22 kilograms and is about 100 millimeter to 500 millimeters long. It fits into a cage that we use, a module we call it, and four of these things fit into this container.

In principal, it is a very simple device. It is a double steel container filled with heavy concrete, with some reinforcing bar in the middle; it's a good civil design. It has got a lid in the same way, it is welded on. The inside surface is done to ASME standards for reactor containment standards, it is helium leak tested and all that sort of thing. It doesn't have any elastomeric seals; it has got a great bit weld up here again designed to ASME standards.

On the 21st of October, we're going to build a ten ton model of one of these and we are going to be drop testing it. This is a final test in a long series. We have been working up from little tiny models of 1/8th scale right up now to a half scale model, which is going to be quite an impressive test.

Just to give you a feel for one of our stations, this station has got the somewhat dubious distinction of probably being the largest station in the world, which is very close to a large city. The city is Toronto and it actually resides in the City of Pickering, which you can see in the background. There are eight units here and each is roughly 500 megawatts, so that is about 4 gigawatts, which is quite a large concentration of nuclear power. And there you can't really see them here and it is hardly worth me just pointing to just odd points of the diagram here, but there are swimming pools, some people call them ponds, the British call them, we call them bays for some obscure reason, where the fuel is kept or stored under water. And they are buried in the structure of these ponds so you can't see that.

Over here in this space, which I am going to show you if you would just keep that little space in your mind, this is what we propose to build, and we expect to get approval for formal company functions by the end of the year so these are the last two units. In this building here, which I will show you in greater detail, we hope to condition, weld the lid on and store these containers for a long period of time. Our company policies will keep the fuel on site; there is no reason to move it to some central place. We would subject the public to unnecessary risks, albeit it very low risks I believe in transportation, but nevertheless, unnecessary, so our policy is to keep it on-site. And we in theory would keep it here until 2025 when it would be moved off.

Incidentally, on the same site, we have had some difficulty with some of our reactors as many utilities have. And I would just might mention we have had to do what is called re-tubed them, and without going into the details of how our CANDU reactors are designed, we have had to essentially take out the pressure vessel of the reactor, and the pressure vessel, there are actually tubes in the Canadian reactor, they are stored in a large number of concrete containers. So, we've got quite a lot of experience; these are 200 ton containers I've just shown here. We have got quite a lot of experience building concrete for storage purposes.

Just a further picture of this building shown in detail; there is a workshop here where they would be leak tested, decontaminated as necessary, because, they will be loaded under water, and where they would be then moved to a storage area here, and there are 700 in this particular first phase of our storage facility.

Eventually this first phase would hold about 5.8 thousand metric tons, and eventually on this site, we would have about 10,000 tons of fuel, which from the document the Department of Energy kindly sent me just the other day, is the size of the MRS that you propose to build. So it is exactly the same size. Your MRS covers 400 acres; it is well known that you've got a lot of space in the United States; in our area in Canada, we are only putting it on about 3 acres, it is smaller, I guess.

I thought I would put in this slide, this is our latest calculations of temperatures and by what we have been hearing for the last two days it is awfully dull stuff; but that's intended to be. The surface temperature, this particular run was run in this building on the hottest day in Toronto and it can get hot there too, even though it is far north; it could get to 38°C for relatively long periods of time like several days. And with a cavity fluid of air and a decay heat of about 2.8 kilowatts per container, we expect the surface temperature to be 48, the inner wall to be 78 and the fuel to be about 137; pressure would be about 1.5 bar or 1.35 kilopascals, nothing at all.

Just a few points in here in anticipating questions, which I am sure the Board will ask, what are the affects of temperature on concrete? It is quite difficult to find out actually. It is certainly true to say that 200°F which is 93°C is kind of enshrined in American codes as being a very, very safe temperature for concrete for sort of indefinite periods. But, if you read carefully, you can take concrete up to 500°F for quite long periods; it depends a lot on the aggregate; it depends a lot on how the water, which is bonded into the chemical matrix of the concrete comes out and so on. But anyway, this particular one is less than the magic 200°F or the 93°C, so we feel quite confident. And most of all, we feel very confident because this is a result of a modeling, and they are notoriously inaccurate as I am sure you all know, and practice has shown that the temperatures would be much more modest than this.

I guess just by way of conclusion then, I said it wasn't going to be very long, I didn't write a conclusion or make a slide because I wasn't sure how one would conclude this talk to this audience, because, I didn't know what you were really after, and now having been here for a day and a half, I am definitely sure I don't know what you are after. But, I suppose I did want to point out that up in Canada we've got some plans and our plans if they come about as I hope they will, on a world scale, I mean we are building an MRS in the city; I am not saying it is good or bad, that's a whole separate thing we will have a session on public awareness and so on and we will discuss that, for the size of the MRS you anticipate for your whole country; that's a matter of fact. Whether it is good or bad is another point.

Now we do believe the transportable containers have got some advantage. I am sure that society in the future, we've been looking 2,000 years and more in some of these viewgraphs, will be very much more conscious, in fact getting we are getting more and more conscious of what dose does to human beings, or what we think it does, because nobody quite knows at these low rates. But, I am sure there will be a feeling that we don't want to dose workers or the public for that matter, unnecessarily. So, rehandling of fuel from the time it is discharged from the reactor, I am sure will be frowned upon if it leads to dose. So, if you do it once and you put it into a container and you don't have to do it again until you get it to repository, I am sure that will be seen as an advantage.

It is always a hard thing to put dollars in those sort of things. There are people who put dollars on man-rem; it is fraught with danger, but nevertheless as low as possible is obviously a desirable target.

I think there is another matter too, and that is that we feel that interim storage, if you know you've got fuel in a very safe environment, and our public tells us that they quite like to see it on the surface. Our anti-nuclear critics say put it out there, put it on the highways they say, so it will be a monument to the folly of the nuclear age. That is the way they speak, some of these people; and others are more thoughtful but still perhaps not pro-nuclear say essentially the same thing. They say, we want to see it. We want to have it in sight. They obviously want shielding there, but they want to see it; they are not sure that they want it buried underground.

We have recently conducted a five province--we are dealing with the public with this hearing in mind, we are going to hold a public hearing on what to do with our nuclear fuel, or this concept of disposal. One message that came through which was surprising to us, but came through loud and clear is that the public wants to decide, whatever that means, the public wants to decide when they will allow the repository to be finally closed. Closure is an important thing in their minds. They say, what if? What if they leak? This is the way the public speaks. You have to address them at that level.

Well, what if? If things are all sealed up, that obviously is not impossible but very expensive, very difficult to retrieve. But one thing, if you had these kind of containers and if they were in a repository for instance, they would be clearly retrievable, particularly if it was a ventilated repository, as we have here. I would point out and this is speculative now, so please don't ask me hard questions about this, but it may be possible to make a design like this in the future for disposal. Now, if you are an electro chemist in the audience you will groan at the mention of cementacious materials being used for such a thing. Some people will groan at the thought of carbon steel being used in a repository because it generates hydrogen. But, there are several years of research and development ahead of us and we may strike it lucky.

So that is something we are trying to keep in mind. Then it would be a true triple purpose container; storage; transporation; disposal. That would be a very interesting objective to aim towards.

Thank you.

DR. PRICE: Thank you. Questions?

DR. NORTH: It might be interesting to hear a little bit more about the other aspects of the triple purpose, the storage--rather the emplacement and the transportation. 100 ton container would seem relatively difficult to transport.

MR. STEVENS-GUILLE: No, 58 tons.

DR. NORTH: 58 tons?

MR. STEVENS-GUILLE: That would be transportable by barge for instance, or boat of some kind or by rail. It is pretty heavy, I know in the U.S. you have got this legal weight truck concept, or not a concept it is a licensing requirement. We have something similar so road transportation on highways is probably not very likely. But, almost any scenario of transportation would involve some kind of road transport. But, we don't see that--if it was a private road to repository from a rail-head for argument sake, we don't see that as being a particularly difficult thing. Yes, you are quite right, handling a container of these dimensions underground would represent some challenge; there is no question about that. And that isn't Canadian concept. I understood the whole purpose of your Board meeting was to try and look at some sort of lateral thinking as one might call it or some other schemes; here is one.

Fifty-eight tons is on the high side, but it is not un-doable; not non-doable, I would think.

Now with regard to the research and development, we have in place a program which was started in a very modest way one year ago and will continue between ourselves and our colleagues in AECL, with regard to the real technical issues of whether a concrete container even gets into the running as a disposal container. So, that work is very, very tentative and I don't want to speculate on the outcome of that.

DR. DEERE: Probably the transportation of some of your transformers approaches that in size.

MR. STEVENS-GUILLE: Some of our transporters are 200 tons; this is very small compared to that.

DR. DEERE: And you take those across country truck.

MR. STEVENS-GUILLE: Yes, but you have to start to bring down telephone poles and wires.

DR. DEERE: And reinforce bridges.

MR. STEVENS-GUILLE: They are special arrangements; that is a legal term also for nuclear transportation. But, no we have done transportation studies; we have had them reviewed by British Nuclear Fuels, which as you know is foremost in transportation, and we don't believe that is an insurmountable problem. After all, we are not thinking of doing it for nearly 30 years in the future, so there is some time to iron out the bugs.

MR. MCFARLAND: Do you see any problem with convincing your review authorities that the concrete canisters can pass the drop tests?

MR. STEVENS-GUILLE: Yes, we do. Sure. Yes. Absolutely. That is why we are inviting them down to come and witness our drop test and they have been part and have witnessing various aspects of our program. I mean this program didn't start yesterday, it started ten years ago and we've brought them along--that is not the right phrase, but we have physically brought them along to each test that will be conducted and we have given them our calculations and told them what we predict and then shown them what we've got.

You must understand that when we talk about dropping 58 tons of concrete, we are not dropping it bare, although we have dropped models bare, we cover them with impact limiters. It is a fairly standard thing in the industry; we use high density polyurethane foam. You can design impact limiters to restrict the G forces quite considerably. But no, I anticipate that licensing a concrete container with conservative Canadian licensing body will be a protractive job. But I am looking forward to finishing my career in the years to come in such an endeavor.

MR. MCFARLAND: It might be an interesting effort to document to help our DOE perhaps convince the NRC that ductile cast iron --

MR. STEVENS-GUILLE: When you really look at concrete, it is not quite the material you feel it is. The popular conception of a big concrete cylinder is what we see in Canada and I'm sure you can see it here in Nevada, a big culvert section going down the road, T-juction or something like that. It looks awful; it has got rebar sticking all over it and it has got a great big hole cut in the side and it is a big shabby and shoddy because it is going to be buried underground. It probably has no strength at all if you dropped it off the back of the truck. I think most people would think it would crack in such a way that it would have no integrity for through leakage. I am sure that is true. But that is not really an engineered product for a nuclear waste transportation device. It is the same materials, but you can engineer them in different ways.

DR. PRICE: Thank you for that presentation; we are glad you have got something concrete to work on.

Our next presenter is Dr. George Danko from the University of Nevada. He is an associate professor in the mining engineering department, Mackay School of Mines. He has been working in thermal simulations, heat transfer studies, ventilation and climate simulations for the mining industry. He will present concepts on thermal enhancement for a high level repository.

DR. DANKO: Thank you, very much.

I'd like to thank the Chairman and the Board for inviting me and giving me the chance to present some of the ideas we have developed at the mining department about cooling enhancement for nuclear waste repositories.

I would like to define thermal enhancement; I would like to describe a few techniques which can be used for thermal enhancement; I'll show a few application examples relative to waste emplacement using enhancement; I'll discuss a few impacts concerning temperatures and drying enhancement and then I'll draw some conclusions.

I am sure that influence in temperatures in our high level waste repository was a complete puzzle for many of us yesterday morning, but we have heard about many ways of influencing temperatures and by today, this afternoon, this puzzle has been solved. So, I will put this into a jigsaw puzzle form and certainly I have put it together.

There is one element in this puzzle which I am going to talk today and that is engineered thermal enhancement; I fit it into the middle among the other elements influencing temperatures and heat flows. Those are site thermal physical properties of every power load or initial area heat load of the waste. The third element is waste heat decay law; that is the age of the waste. And certainly, an important element is the waste emplacement layout geometry, especially the exposed rock surface area where heat is injected into the rock. And the last element is engineered thermal enhancements.

That will make it easier to define thermal enhancement. That is promotion of heat rejection into the geological rock mass and/or the environment of the repository by engineered heat transport techniques and/or devices.

I listed four different techniques to realize the thermal enhancement. Three of them will include air ventilation and convection to remove the heat from the emplacement area into the rock at farther distances from the emplacement area or into the environment; into the atmosphere.

The first one is open-loop repository air cooling by ventilation. This method has been considered to cool the waste in situ and bring down temperatures of the emplacement borehole in the area.

The second one is a slight modification of an open-loop air cooling using closed-loop controlled air recirculation.

The third one is a closed-loop natural air convection.

The fourth element, the fourth technique is a unique one when heat transfer is promoted within the rock and it does not include any kind of a visible cooling loop.

I'll show a few pictorial diagrams to illustrate these techniques one-by-one. The first one is open-loop repository air cooling by ventilation. The underground repository facility is shown in the middle; waste containers are in place either in cavities or in the drift. We have an access ramp and a shaft and we can maintain a cross-flow of air to bring some heat from the emplacement area into the atmosphere. The enhancement direction is container to air and it is probably more effective to apply for drift emplacement than for cavity emplacement. This technique has been considered to cool down the emplacement area for possible maintenance or waste retrieval. That technique needs power, needs a fan, needs many things including monitoring of the radionuclide carried away by the air and by ventilation. So, it does have some definite disadvantages.

This modified version, which has been suggested by others and there are papers on closed-loop controlled recirculation, does not communicate with the atmosphere. The thermal enhancement includes two steps: one is container to air, and the second step is from air to rock and that is an enhancement from one part of the repository to another part of the repository. That can be ventilation between two panels, one being in place and the other is under construction, in order to bring some heat into another area to enhance drying in the other area while keeping cool the active repository emplacement part.

Number three, still working with air cooling, is a closed-loop natural air convection. There is no cross-flow involved in this technique. We can see an air recirculation; we see assume this air recirculation here driven by natural buoyancy, difference between the hot and the cold air; that can be enhanced and the temperature difference can be broke down between the container and the air and between the air and the rock surface.

Technique number four is the unique one. When promotion of heat transfer is established using some engineered heat transferring device installed into the rock mass. This is the direction of heat flow, thermal trajectory and I'll elaborate on the techniques which we can apply. But, first, I would like to emphasize why we need this rock-to-rock thermal enhancement. I listed two main reasons.

The first one is to remove heat from the emplacement cavity towards the drift surface. If you want to perform an in situ cooling re-aging of the waste, this technique can help remove heat from the emplacement cavity towards the surface of the drift in order to remove it by ventilation. So that can be incorporated in the ventilation.

The other reason for using rock-to-rock thermal enhancement in cavity emplacement is to bring down temperatures, high temperatures around the emplacement borehole. And then I want to emphasize that enhancement is established between point A and B where both points are within the rock mass.

The other main reason to consider rock-to-rock enhancement is shown in this other sketch. When heat is rejected towards the pillar area from the emplacement area in order to keep the temperature difference lower, to heat up the rock at farther distances in order to enhance drying of the rock.

I listed four feasible techniques to realize thermal enhancement within the rock: heat pipes; thermal syphons; heat-superconductor rods and, active or passive heat pumps. I would like to elaborate on heat pipe and thermal syphons.

I am sure we all have a fairly good idea of what a heat pipe includes. A heat pipe is very simple in appearance; it looks like a rod, a solid rod. But, inside the heat pipe there is a small amount of liquid that recirculates within the heat pipe and transfers heat from the hot end to the cold end. So, this is the heat pipe (indicating). This heat pipe is applied to a horizontal short borehole emplacement. The heat pipe is running close to the container; doesn't touch the container and if a borehole lining is used, then it can touch the borehole lining, but it is not necessary to have a borehole lining and an attachment.

The heat pipe itself contains a small amount of liquid that evaporates and vapor transfers heat along the length of the heat pipe and during this travel, it condenses and a condensate will flow back within the weak structure of the heat pipe interior wall.

If I say that this figure is not to scale, that is a serious understatement, because heat pipe is relatively small in diameter and this is an attempt to show some more realistic proportion between the diameter of the container and the heat pipe.

The heat pipe is emplaced in a borehole and after emplacement it is backfilled with a relatively high conducting material to facilitate a good thermal contact between the heat pipe wall and the rock, all along its length.

Thermal syphon is similar in terms of emplacement and the installation. It is different though because there is no evaporation in a thermal syphon. It is a fluid loop that carries heat from the hot end towards the cooling section, and the driving force is just density difference and buoyancy.

I would like to show a few applications of these elements. I made one mention superconducting rods, which is so simple that I didn't even prepare an overhead transparency for that, that is a physical way of transferring heat from the hot spot towards another point of lower temperature within the rock.

Here are some application examples; I prepared four. Number one will show a short vertical emplacement with a container-to-air and rock-to-rock enhancement. The second example will include a short horizontal emplacement with rock-to-rock enhancement. Number three, will be an example on drift emplacement with rock-to-rock, container-to-air and air-to-rock enhancement. And the fourth example will be a high density vertical emplacement with container-to-air and rock-to-rock enhancement. All these elements I discussed in these conceptual sketches will be applied upon a high level waste repository conceptual example.

Short vertical emplacement with container-to-air and rock-to-rock enhancement. This part of the figure is a planned view of a section of an emplacement panel. A cross-section, along these containers and devices as shown here, and these thicker lines are the cooling or thermal enhancement devices, heat pipes, if you wish, that carry heat from the container area into the pillar area. These heat pipes are stretching up at 45 degrees in order to bring heat out of the horizon of the emplacement and that provides a better thermal performance, and on the other had to provide some gravity assistance for this circulation going on within the heat pipe.

The connection of the heat pipe to the waste container is shown in this figure. As you can see, the heat pipe is installed in a borehole, which doesn't have a direct communication with the container emplacement borehole, and just transports it out, picks up the heat by conduction within the rock mass. The heat pipe is crossing the emplacement drift, providing a very important service, we will bring out heat from this hot area into the drift and will allow us to remove heat by ventilation more intensively. Then the cooling section of the heat pipe is installed here; it is backfilled so it becomes an integral part of the site. And if you look at the connection between the hot end and the cold end, we realize that this is a rock-to-rock enhancement, however this lower section will realize a container-to-air enhancement, so it is a combined enhancement, and this configuration was analyzed numerically. I would like to quickly jump to some results to show you some immediate benefit of this cooling enhancement.

I used 3.3 kilowatt initial heat load and a relatively young waste in this simulation that was from two years ago, it needs a serious update which we hope we will be able to perform. Now, here is the reference case when there is no heat back and no ventilation. And maximum temperature is around 230° celsius on the borehole wall in this cavity, somewhere around here, with shut down heat pipes. If you turn on the heat pipe, but we do not perform ventilation, temperature will drop down to 136° celsius on the same spot which is considerably lower. If we add ventilation and remove some more heat on these sections of the heat pipe, that will bring temperature down to around 100°celsius if we remove two kilowatts per container section, which is 15 feet long section in this simulation.

I would like to emphasize the efficiency of the heat pipe cooling enhancement. I would like to also show that ventilation, cooling by ventilation is relatively inefficient as compared to heat pipe, and if we consider the range here Eric Ryder showed us cooling by ventilation, his range was somewhere around here, so we could expect about that much (indicating) of a cooling by ventilation in this.

Going further on the line and showing you another example, this is short horizontal emplacement with rock-to-rock enhancement. Again, containers are cooled by thermal enhancement devices. One container per one heat pipe was assumed in this configuration.

The further application example I would like to show you is a drift emplacement with rock-to-rock, container-to-air and air-to-rock enhancement. A section of a drift is shown here with two containers emplaced on the floor and these funny looking necks stretching out are the rock bolts. We know we are expecting to see rock bolts around the emplacement drift, of 15 square feet, so that will provide a regular array, of semi-regular array of rock bolts, and we envisioned that rock bolts would be combined with cooling enhancement devices. There will be holes there so it can be just used for two different reasons. One is for supporting the roof and the second to inject more heat into the rock area.

Now we can see that with this rock-to-rock enhancement we are realizing two other enhancements at the same time. So it is really a trifling advantage. Again, say heat pipes or other thermal enhancement device, devices will remove heat from this base plate or mat into the rock. This whole area here will de-stabilize the thermal boundary layer within the air and makes this recirculation more intense and faster. Therefore, even on this rock wall we will see an air-to-rock enhancement due to these cold plates. And once we accelerate the recirculation, there will be a container-to-air enhancement on the surface of the containers. So that is a three step enhancement with one technique.

The last example shows a high-density vertical emplacement with container-to-air and rock-to-rock enhancement. That was really the first example we published and we came up when we started envisioning thermal enhancement. It again needs a serious update, but the principle was that left out drifts two out of three, so these lines stand for drifts which were left out and we removed the containers from this drifts into these drifts which we kept, so that is a reduction of 66 percent in the underground construction work. It is a dramatic change and a reduction in the emplacement drift length.

Instead of emplacing containers into these drifts, we sent out heat using heat pipes. So we realized that we don't have to spread the containers all over the mountain. We can send out heat without sending out containers and keep the containers in a smaller area. That seemed to be a reasonable idea in order to make the site more retrievable with less ventilation problems and other benefits. The temperature for about the same level as was for the reference emplacement layout.

These are the impacts I listed, which we came up with using thermal simulations; decrease in hot-spot rock, and container surface temperatures. As a direct consequence of number one, we certainly had the lower thermal gradients around the emplacement area in the drifts. We saw the promotion of rock drying because of the elevated temperature in the pillar area, and as a consequence of these three, we expected a redistribution of in situ and thermal stresses around the emplacement area and farther in the drift.

That temperature curve is going to be used one more time. I have already discussed temperature distribution with time, using thermal enhancement for this layout, and I would like to show two other interesting temperature fields for the same layout and for the same enhancement. This is again an example, if you read the bottom curve here, this shows us a cool concept. This is around 100°celsius, so with the original heat load which was a high heat load density, of 3.3 kilowatts, but a young waste, we are still able to modify the referenced temperature, that is a hot concept into a cold concept just by using thermal enhancement with no other measures.

We can certainly increase the waste mass and bring up the temperature to the original level and this is what is shown in this figure. If we go with the heat load in 25 percent steps, then this is the first step, it is already 25 percent increase in heat load and temperature is still favorable. Fifty percent increase, still acceptable for the present considered temperature field or maybe even 75 percent. 100 percent increase, which means a double waste mass would still give us a slightly lower temperature than that of the referenced case.

Now another interesting thing which can be observed by looking at this figure that this temperature history is being transformed and it shows it is more similar to an older waste. So this really performs as an in situ aging if we consider it that way.

Another exciting figure is this showing temperature history within the pillar area, 30 meter distance from the container and that is really somewhere around here (indicating). This is the reference case, and with an increased heat load, that reference case is running below 100°celsius so that is not going to be dried out for this kind of a young waste. But, with elevated heat load, average heat load, it would give us other solutions for hot concept, with dried out area within the pillar.

With these results in our mind, I am in the position of drawing some conclusions. Number one, thermal enhancement can significantly improve temperature distribution both in the emplacement and the pillar area.

Number two, a variety of conventional technology can be used, especially ventilation, heat pipes, and the combination of the two. And that is an especially a favorable combination when rock-to-rock thermal enhancement can improve ventilation, cooling by ventilation.

Number three, thermal enhancement can be applied to either cavity, or drift emplacement.

Number four, either hot, or cool concept can be supported by thermal enhancement. It is not specific to either emplacement method or to concepts relative to temperature level.

And number five, additional advantages can be achieved, such as increased drying, a favorable stress redistribution around the emplacement drift, and reduced emplacement areas, or increased waste mass.

Thank you, very much.

DR. LANGMUIR: Are you producing the same amount heat in the system overall regardless of what technology you use for ventilation? So, what you might in fact do, is you might evaporate more moisture for a larger volume of rock if the average temperature stayed over 95 degrees. So, we would have to consider that consequence, perhaps if we dissipate the heat further out and maintain a lower temperature near the waste. But, then you have options where you can get below 95 too, presumably as well.

DR. DANKO: Right. It is a valid comment, but the simulations, Eric Ryder's simulation did not include the removal of heat by evaporation, so those considerations about enhanced drying are really speculative and are relying upon temperature levels. So if we see a temperature running about 100° or close to this boiling point temperature, then we assume that there will be drying due to this high temperature. However, the model was a three dimensional model using poor heat conduction for the rock mass, and using convection along those heat pipes. So the model really was capable of handling heat pipe effect all over the site and that was included in the thermal model. But, it was not assumed that due to evaporation, more heat was removed from that area. However, that would modify the temperature results to a certain extent.

DR. CANTLON: As I read your figures, the beneficial impact on cooling attenuates around 200 to 300 years. Is that correct? It becomes almost asyntotic.

DR. DANKO: Right. It is interesting to see that. If we have a look at the heat flux removed by those devices, it will be the majority of heat removed will be within the first 70 to 100 year period and then after that the efficiency of heat removal by these enhancement devices will be very low. So, if you relate this to the title which says pre-closure thermal enhancement, then it is really considered to be pre-closure enhancement, because the job is going to be done within the first few decades.

DR. CANTLON: That would mean then that the engineering of the heat transfer mechanism, whichever you use, doesn't have to have a very long life?

DR. DANKO: That's correct. That's a correct observation.

DR. CANTLON: Then the question would be, what sort of complications do you intrude into the repository in terms of chemistry, different corrosion environments and that sort of thing?

DR. DANKO: Well, it is going to touch the same issues as we have got to address when we talk about container corrosion and emplacing containers. Very similar or the same material can be used to cover them and the cooling enhancement devices, the heat pipes. The difference is that these pipes are 0.05 meter in diameter, two inches, and it seems to be easier to handle that small disturbed area. There will be no stability problems along these lines. There could be some corrosion questions addressed along these lines, but as you said at the time, the lifetime expectancy doesn't have to be more than say 100 years and if these emplacement boreholes are correctly designed, they can be effectively used for dividing the repository and engineering the hydrology around this disturbed zone, that it will not invite water into the emplacement area. So even if we assume that the ceiling of these boreholes become defective within a relatively short period of time, it can be engineered in a way that it is not going to be deterious to the waste insulation.

DR. CANTLON: What about the coolant fluids in the heat pipe, that is a complicated chemistry problem, that would seem to me.

DR. DANKO: It is going to be, according to our region a very simple material being water.

DR. CANTLON: Water?

DR. DANKO: Water. That is the best you can choose for heat pipes at this temperature range. And that is just by sheer chance that if you look into the heat pipe literature, but within this range of temperature, they will list that the superior material that you can select is water.

DR. CANTLON: So you have ensured water being present near the container?

DR. DANKO: Well, thank you, very much. It seems to me that we are using say half a gallon of water in each heat pipe and you might be able to dry out a few cubic meters of water during this 100 years with a gallon of water which will shed finally and will disappear by itself.

DR. DEERE: In the in drift emplacement, when would you be able to do the backfilling?

DR. DANKO: In drift emplacement if you talk about rock-to-rock enhancement with thermal enhancement built into the rock bolts, then it is immaterial whenever you want to backfill the drift, those will stay there in the rock and will still do some service in order to keep temperature gradient lower and transfer some more heat so it doesn't have to stop work, we don't have to assume that it stops working, when it is backfilled. So the heat pipes can be operational even after backfilling. But, ventilation has got to stop and I did not assume ventilation; I assumed natural convection for drift emplacement for a longer period of time, while for a short period of time ventilation could be efficient to keep temperatures lower.

So, again to summarize my answer, whenever you backfill that will stop the container-to-air enhancement but the rock-to-rock enhancement will still give us some favorable service to keep temperatures more even along the site.

DR. DEERE: The motive of my question was to see when one might be able to use a backfill material including bentonite, and in that case you might need ventilation, forced ventilation, assuming that bentonite should be used at a temperature not greater than this morning, we heard 400° for short term, but possibly a little over 100 or 150°.

DR. DANKO: 100 to 150° celsius. Right. It is a question of simulation to see how long a period of time you need for ventilation. But, I expect to see a significantly short time if rock-to-rock enhancement is used instead of just relying on sheer ventilation. So that needs to be seen; needs to be calculated and we certainly need some work going on in this area to look into this interesting question, how long a time span is needed for ventilation?

DR. DEERE: Did you say that the rock bolts would be just an ordinary rock bolt?

DR. DANKO: No, it will be a special rock bolt. It will be an ordinary rock bolt in terms of strengths of the bolt, but we are envisioning a core within the rock bolt with a heat pipe. So, it will be an integrated rock bolt with a cooling device. That is the idea.

DR. LANGMUIR: This may not be a fair question for you, but we have been talking for the last day or so about elevated temperatures around the waste, and we obviously have this requirement of retrievability for 50 years. This certainly has to be a factor, if we are getting human beings down in this space, this certainly would influence the temperatures you could allow or you would want to deal with in a system like this. How does that factor into how you ar handling the heat dissipation and how do we deal with that?

DR. DANKO: There is a plot of mineral data that will tell us that temperatures will be generally lower if we use this thermal enhancement, so it will be relatively easier to cool down the emplacement drift surface area for entering the temporarily closed emplacement drifts. So if we don't have ventilation for 50 years, but we do have ventilation for say ten years as Eric Ryder assumed, and then after this ten year period, the emplacement drifts will be temporarily sealed and just a very little amount of ventilation will be maintained. Without rock-to-rock enhancement the temperature is going to build up and there will be a thermal shock when it is suddenly decided to cool down to a temperature level of 50° celsius or so for entering.

With thermal enhancement devices all active without ventilation, the thermal shock will be lower, and it will be easier to cool down from 80° celsius to 50° celsius from 100-120° celsius to 50° celsius. That is how I see it. And, on the other hand, these cooling enhancement devices will not transfer backward significant heat from the pillar area. So that is one interesting feature in a heat pipe characteristic that it is almost a one way--it can work as a dial, a thermal dial that transfers heat from the emplacement area into the pillar area, but if you for some reason decrease the temperature in the emplacement area, this heat is not going to come back along the same way. It will stay still there. That is a unique feature.

DR. PRICE: Dr. Danko, thank you very much for your presentation.

DR. DANKO: Thank you, very much.

DR. PRICE: Our next speaker is going to be the first one for the Board, because we are going to ask Russ McFarland if he would provide his presentation very short summary of geologic heat pipes in place of the TRW-BMO presentation of a state of the art review, which they were unable to provide for us at this time. So, Russ is going to provide a very short summary that topic as he sees it.

MR. MCFARLAND: When I recommended that TRW be asked to come and present a program that they conducted in the '70s, on heat pipes, I had not even the faintest recollection I would be put in this situation. Apparently at the last moment a blue suit at Andrews Air Force Base, Air Force Systems Command in Washington decided that they didn't have adequate time to review the presentation that was to be presented here today. So, having in archives, the number of viewgraphs from that time period, I offered to do a very quick thumbnail presentation of what would have been a very interesting presentation.

The USAF in the mid-70s had a program look at very deep facilities to look at surviving weapons effects. They initiated a two year program in 1986 to use geologic heat pipes to dump heat from thermal systems, power systems, air conditioning systems, directly into the rock for command centers, that could not have any openings to the surface.

Now when I heard George's first presentation about a year ago, immediately went back to this, and if some of you remember back to the early '70s when the Alaska Pipeline was built wherein they used 130,000 heat pipes in order to support the pipe in permafrost and not have heat from the heated oil go down the supports and melt the permafrost and settle, those heat pipes were ammonia; they are still working today. Apparently the pipe is still suspended above the permafrost.

The Air Force went into a testing program briefly to try to apply this technology. They took a look at the grouting development and found out that ordinary cement grout was quite effective. They developed a 15 foot heat pipe and a 100 foot heat pipe in the laboratory to examine the effect of tilt, to examine the diameter, to understand the physics of the design of the heat pipe, where up to that time it had not been optimized. They then decided having a laboratory test, conducting laboratory tests to do a field test. They selected a site some place north of Las Vegas, about 100 miles north of Las Vegas in the quartz monzonite, above the water table, about 400 foot deep, rock temperatures about 50°F.

Within this mine, they installed three 100 foot long heat pipes. They were three inch boreholes, two inch diameter copper heat pipes working through it was Freon-113. The close second working fluid was distilled water. The distilled water in a closed system with a slight pump down was a very, very effective working fluid. It turned out that the Freon-113 was more efficient; the Air Force was interested in efficiency. A boiling evaporator was used and a test was conducted over a period of five months.

Now, this was a schematic taken from one of their documents. Really three heat pipes, a freon boiler, nothing more than a heat exchanger, a pump, instrumentation; very, very simple as George indicated. The inclination of the heat pipe was about 20 degrees from the horizontal, the wicking effect, the feedback of the fluid was condensed and was greatly improved.

Test results, total operating time of 4500 hours, the most reliable part of the test was the heat pipe. According to the documents, the computers failed at one point and they had power outages. But, the heat pipes each removed about 7 kw. I was shocked when I read that, and with a temperature differential of only 110°F. Now this converts to about 2.1 Btu per hour a foot degrees fahrenheit. The heat pipe's efficiency is a function of its length; the temperature differential; and, of course, the time you have to use it.

As far as I know, the results of these studies, this test was never brought into the open literature; it was not classified; I have it in archives. But, it was never published; it was never brought into the open community. The Air Force, for reasons only known to them do not make an effort to bring their research out into academia or into the industry, so it was with some shock when I saw George's paper and wondered where he found this material. And it is the old adage of re-inventing the wheel; very efficient.

With that, I have honored my commitment to TRW and I would like now to turn the meeting back over to Dennis if there are no questions.

DR. PRICE: Thank you, Russ.

Our final presentation for the day, and the tail wind hasn't blown quite strong enough to get us to schedule, but we are getting toward the touchdown time, is an overview of Pre-Closure Ventilation Options. This paper to be presented to us authored by Gary Sandquist, who is a professor of mechanical engineering at the University of Utah, and Antony Ivan Smith who will be making the presentation who is Chairman of TICA and President of Tunneling Technology Corporation and a consultant to Sandia National Laboratories.

Mr. Smith.

MR. SMITH: It is my pleasure to deliver this paper to the Technical Waste Review Board and a little nervous as you can probably tell. It has been a terrible week with the vagracies of Harvard Graphics and Ventura Publisher and all that can go wrong; everything has gone wrong.

A little bit about myself, I am an Australian, fourth generation. I was brought up in England and educated in Canada and first came at McGill University. And after a short tenure there I went to the United States and worked for the Union Carbide Nuclear Company, and I have been involved in the mining and tunneling industry ever since. My particular expertise is in tunneling and tunneling technology, and I have worked underground for the last 25 years.

This is the title of my paper here and I will try to make this a more abbreviated session. Any repository for high-level nuclear waste requires the utilization of vast concepts and planning, engineering and construction. The needs of the retrieval unit demands a very careful interaction of these skills. In the proposed concepts of excavated tunnels for access and storage, the tunnel boring machines, the ventilation systems become quite critical. And as we move here to this next slide, what I have done is broken down the ventilation requirements in the repository.

The major activities during construction would be: portal excavation, access ramps, underground excavation, and emplacement tunnels. These all require very basic concepts of ventilation.

The emplacement operations are once again; site preparation; water transportation; canister installation; and, emergency removal. These are individual separate functions that would require different ventilation concepts.

Pre-closure operations would once again be maintenance, monitory, emergency removal in case any canister has to be removed.

The last area is post-closure operations which would be in terms of backfilling.

As we go to what I've identified here as heat sources, natural heat sources or incurred heat sources that would be found in the repository. The one on the left of personnel would be individual; people. People create heat; people have requirements. Kitchen facilities, whatever the requirements underground, these are individual heat sources that are applied into the community. Ancillary support would be locomotives, trains, or loaders or any form of transportation within that aspect that individuals require.

The equipment would be the tunnel boring machines, loaders, transformers, compressors, conveyors and transportation devices.

The natural would be local rock ambient, this has been addressed in earlier papers; water ingress, I show there is very little here, but in some areas that any water brought in to the environment is a source of heat; ventilation duct, very critical and I placed it in that natural area. In terms of having to ventilate any tunnel or an emplacement area or in any work area, ventilation ducting would have to be placed in order to extract air. This ducting can then transfer heat back into the incoming air and this will be addressed a little bit later in my viewgraphs.

Compressed air ducts; tunnel machines will be required to have sources of compressed air and these would once again radiate heat outward from this tubing and ducting. Water ducts, the same would be applied there. Discharged ducts, the same; in tunnel boring machine, for example, generates a great deal of heat. It is often removed by coolers and then that is transferred back to discharge system and brought to the surface. Once again, this heat is radiated back into the incoming air. And the last, but most critical of course, is the nuclear, the heat emanating from the nuclear waste canisters themselves.

So each one of these components, I have just taken for example just construction operations. each one of these components could be broken down into what would be the minimum system requirements, so here we are looking at one item. On the other viewgraph we had a whole series. Well, anyhow, I have just taken this one as an example. These are base minimums that are required to have a tunnel or a work area underground. So, we say approximately 150 men are working in this environment. They require 200 cfm per man. That is by law. It is actually implied per employee. So whether you had ten underground or 100, the implications are that your minimum requirements would be for that particular amount; 30,000 cfm, moderately significant. Equipment; diesel. Transportation would be most rapid by using a small cars or maybe trains and such equipment. So 1,000 horse power of diesel power is not very significant for a project as large as this. 100,000 cfm moderately significant amount of air.

In terms of the tunnels I've taken 60 feet per minute and I have to refer back to the actual CFR, it states that in a non-coal mine environment, 30 cfm as being the minimum requirement across the bore of the tunnel. But, I have taken 60 feet per minute as being the absolute minimum that is currently enforced through MSHA and OSHA which is applied to coal mines. But when one considers the basic requirements of the system as utilized in current technology using 60 and 100 feet per minute, is the normal basic requirement for day-to-day work underground. So, I've taken 60 feet a minute as an example. So in the approximate 22 foot main tunnels, 44,000 cfm would be required to maintain just the bare minimum air by law. And in this case here there are twin bore tunnels for the emplacement drifts; there would be two pairs that have been completed and finished and there are two pairs under construction. So this would be the base minimum just for a man to walk into the tunnel and do nothing else but just be there. This is the base minimum. This adds up to 264,000 cfm, which is moderately significant because we are just really looking at the air requirements in the very initial phases of the repository.

As I have commented in my paper, if I may refer to it momentarily, is if we looked at the actual construction phases by the rule of thumb, we would see that in the construction phase of the 22 foot main tunnels, we would approximately double that air requirement during those phases. And during the construction of the emplacement drifts which would occur a little bit later on, that the 15,000 cfm would go up depending upon the size and of course the tunnel machine. So, we would get up into the 300,000 to 400,000 cfm range just based upon the normal activity during the initial phase of the repository.

I had intended to read this paper, but I think this is going a little better. What I have done here is broke down these phases into, and excuse the dates in here, the vagracies of how the graphics didn't quite come out this way. But, anyhow I have just taken the two year jump and some approximations in a hypothetical case. But, I think it is indicative that if we see TBM access, this would be the possible requirements during the tunnel boring phase of the access tunnels. And if we note on top of that we have excavations, this would be underground excavation, this would be drifting, this would be chambers for transformer or such and so forth, and right about that we have access minimums. In essence the minimum requirement in an active place underground would be represented by that value.

So, as we move along in terms of time, we are coming to the fact where the boring of the emplacement tunnels would come into place and which I chose as TBM waste. That is shown as of 1998. I mean, this is very arbitrary dates, but as one notes, this becomes a larger and larger aspect, looking on the bottom line here, this becomes a fairly basic function. The same amount of work should be done every year and so the ventilation requirements for that phase would be fairly consistent. But, as we note coming out the access minimums have reached a certain point, because we know that they have not chambers or work areas or laboratories. All of these things would have reached a certain particular size. So there is nothing increasing demand in that area.

The waste minimums reach up a little bit, because this is what is required in order for a person to access that tunnel. I mean, one might have eight, nine, ten of these waste tunnels completed, but in order for a person to enter that tunnel, the minimum requirements have to be met. So, what I have done is demonstrated a slight increasing requirement for the waste minimum.

The last category I call additional cooling is what I feel would be the effects of the nuclear waste and the loss of the heat and this would be the minimum requirements to withdraw that heat to the outside. So, in this approximation we are looking at 500,000 cfm there.

We will move to the next viewgraph here. We are moving to what would be a cross-section of a typically bored TBM tunnel. And what I am just trying to suggest in here, the normal ventilation that is very simplified, that our tunnel boring machine would require in its own tunnel. We have a base minimum where you can reach a minimum velocity, and this is a very arbitrary value, which affects man in working. And also affects equipment and for example conveyor belts.

In one of the scenarios, in the tuff ramps, the conveyor belt would be removing material from the tunnel. And, at a certain velocity, this conveyor belt would be starting to contaminate the atmosphere. So, we are very dependent upon a fairly moderate philosophy in combined tunnels. A man working in a tunnel cannot work very well in velocities that are around 1,000 linear feet per minute. Typically we find in tunneling is 100 feet per minute is reaching an average value for that.

The other point that shows up in this viewgraph is the fact that there is a transfer of heat from a heading through the outboard exhaust here that is taken either to the shaft or to the portal. And at Buckskin Mountain in Arizona during the construction phase of the central Arizona project, this tunnel machine, about 22 foot in diameter the ambient got extremely hot, above 80 and 90°, so they installed air conditioning on the tunnel machine.

Well all of the exhaust heat from the air conditioning was then transferred back into the vent pipe, back outside. But, actually the ambient on the outside was less than 10°. So, you have a tendency to bring up the heat level of the working environment.

Excuse the spelling, because this was another one of the things that went wrong this week was the vagracies of how the graphics and everything, but anyhow, what I have attempted to demonstrate in here is a fairly typical system that might be important. One of the requirements is that we have, there is a single exhaust shaft. If one has a single tunnel that is entirely utilized for ventilation, then this tunnel can have much, much higher velocities of air. And so that in the scenario here where the blue represents the normal flow of air that is brought from the outside, from the access drift and from the ramps and carried through, the green air would allow for forced air to be passed through as needed and the red line is the exhaust air that is under suction.

What I have attempted to suggest to you is that if an invert segment is laid in the tunnel, that the canister could be placed either horizontally or vertically, and allow for natural convection to this air flow, a greater efficiency might result. So this is what this concept is there.

I think in the final review here, I think that we need to look at a few points here. One is the gross bore ventilation. The diameter of the tunnel is very, very important relative to the base minimums. This was apparent in an earlier viewgraph. So in terms of any design, the difference between an 18 foot diameter tunnel and a 22 foot diameter tunnel is very significant. So it is quite important in terms of ventilation, overhead cost requirements to go ahead and maintain the minimum diameter as possible.

The second one is the heat pipe or augmented cooling. This is an area of great interest and I think it has great potential in terms of the fact this ventilation system could reduce and improve the cooling process.

Canister placement in terms of logistics, the horizontal placement of the canister would have a tremendous effect in terms of construction cost and also in terms of the minimum requirement of air. If a placement tunnel is 16 foot in diameter rather than 22 foot in diameter, it makes a tremendous significant cost in requirement in terms of the overall ventilation.

Dust and particulate control, very, very important in terms of all the mechanical excavation that is required. This is a very significant area.

The high air velocities in access tunnels, this was addressed a moment ago in terms of conveyor belts, human beings, people, equipment, is to bring down the velocities in those tunnels.

Separation of construction and emplacement zone. This was brought up by the NRC and it is rather important that this area has to be considered a being very, very important.

Ventilation shafts in tunnel, once again is looking at the possibility of a separate drift or tunnels to solely dedicated to ventilation.

The last one is the detrimental effects of the waste carriers. These plan to be large pieces of equipment that will be moving through and blocking sections of the tunnel and they will have a very detrimental effect upon the overall ventilation.

So, I have attempted to abbreviate my talk here and if anybody has any questions?

DR. PRICE: Any questions from the Board?

DR. DEERE: If I understood correctly, you said there would be a considerable advantage in ventilation with the 18 versus a 22 foot diameter?

MR. SMITH: That is correct. Going back to, and let me find the viewgraph here if I can. The minimum requirements for the tunnel are 60 linear per feet across the gross bore of the tunnel. So basically this is by law and is a very specific requirement. So just upon that value alone in terms of the sheer quantities of tunnels required, especially in the emplacement area, the ventilation overhead as I called it, I believe, a significant difference in ventilation overhead would result and this is addressed in the paper; a significant influence for example we are looking at waste minimums here. Waste minimum tunnels, which is the cross-hatch right here and here and here, that value would continue through the life of the repository, but would be very much affected by the diameter of the tunnel.

The access tunnels which are approximately say 20,000 to 30,000 feet long, are not that significant in terms of the overall ventilation requirement.

DR. PRICE: Thank you, very much.

MR. SMITH: Thank you.

DR. PRICE: I think I want to pause just right at the end of our meeting here to ask if there are any questions from the audience or any comments on any of the speakers or for any of the speakers this afternoon?

Absent any such questions or comments, I want to express the appreciation of the Board for those of you who made presentations this afternoon. We are in your debt and do appreciate very much your giving of your time and talents this afternoon for us.

Thank you very much. I think without anything more then, we stand adjourned.

(Whereupon, the proceeding was concluded on 10/9/91, to reconvene 10/10/91.)