UNITED STATES DEPARTMENT OF ENERGY

OFFICE OF CIVILIAN RADIOACTIVE WASTE MANAGEMENT

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TRANSCRIPT OF PROCEEDINGS

PRESENTATION TO NUCLEAR WASTE TECHNICAL REVIEW BOARD WASTE PACKAGE CONTAINERS

LAWRENCE LIVERMORE NATIONAL LABORATORY
SHERATON HOTEL
PLEASANTON, CALIFORNIA

JANUARY 18, 1990
JANUARY 19, 1990

NUCLEAR WASTE TECHNICAL REVIEW BOARD MEMBERS PRESENT

DR. DONALD LANGMUIR

DR. D. WARNER NORTH

DR. ELLIS D. VERNIK

DR. DENNIS L. PRICE

DR. MELVIN W. CARTER

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P R O C E E D I N G S

DR. PRICE: Welcome to the first meeting of the Containers and Transportation Panel with Lawrence Livermore National Laboratory. This is the first panel meeting principally focused on the selection of container materials. Dr. Jardine will make comment on this separately.

The meeting is being attended by panel members Dr. Carter, Dr. North and Dr. Vernik. Dr. North is yet to come. Also we expect Dr. Don Langmuir, a Board member, to be present a little later.

Our technical support is provided by Dr. Chu and Mr. McFarland. These are senior staff members of the Board.

I would like to take this opportunity to ask panel members if on an occasion they wish to have an action item, that they specify it as an action item very clearly and state their request very, very clearly and specifically.

In addition, I would like to acknowledge the presence of Mr. Voiland, a consultant to the Advisory

Committee on Nuclear Waste; Mr. Carl Johnson and his associates from the State of Nevada; DOE representatives, including in part Mr. Stein, Dr. Isaacs; Messieurs Delaney and Hale from headquarters, as is Gertz and Clonginger from the Yucca Mountain projects office. (Mr. Gertz and Mr. Delaney were absent.)

Among the numerous attendees from Lawrence Livermore Laboratory are Drs. Rams Spot and Les Jardine. The NRC staff are represented by Bunning and Ballard and other attendees, including personnel from the Center for Nuclear Waste Regulatory Analysis.

For the audience, the panel is in the information-gathering stage of our effort. Because of this, presenters at this meeting may be questioned by panel members and the NWTRB staff members also.

If anyone has not yet signed the list of attendees, please do so at the break. That list should be out at the table and you've had an opportunity to sign.

Transcripts will be made of the open meetings. Mrs. Enerson of the Board staff can assist interested parties in obtaining copies of the transcripts.

There will be a tour of the Laboratory facilities this afternoon. There will be closed meetings of the panel Board members and staff at the end of the day.

At this time, I will turn the chairing of this session over to Dr. Ellis Vernik.

DR. VERNIK: Thank you very much, Dr. Price. It is a pleasure to be here with you and thank you for attending.

This is the first in a series of meetings on the engineer barrier system. We are developing plans to receive separate briefings on host rock and waste form interactions. These meetings may well be held with other panels such as those concerned with structural geology and geoengineering, hydrology and geochemistry and the Risk Performance Analysis Panel.

A set of generalized questions has been prepared and circulated to DOE that outlined suggested lines of inquiry that the panel wishes to follow in these meetings. These questions are items that the panel and eventually the Board will address. These questions consider the impact of possible modification of the current plans for emplacements.

Specifically, they ask, one, if shifting additional responsibility of the waste package from the host rock for the containment of isolation of wastes is an advantageous alternative.

And two, if in placing the waste under conditions that avoid the near-field temperature rising above the boiling point of water has a beneficial effect on container life.

These questions are to be addressed separately by other panels from their respective viewpoints. It is hoped that within one to one and a half years, the Board will be able to provide viable answers to these questions.

With respect to the recently-announced scheduled changes in the development of the repository, we look upon them as offering the program an opportunity to broaden and deepen its database supporting the engineering barrier design. To do this, it may be desireable to expand the current program to consider both alternative materials and alternative designs for the waste package.

I wonder if any of the other members of the panel would have a comment that they would like to make in a preliminary way.

DR. CARTER: No.

DR. VERNIK: If not, I would like to turn this meeting over to you, Dr. Isaacs.

DR. ISAACS: Thank you very much. Good morning.

On behalf of the Department of Energy, it is a pleasure to be here to see you all again. It is a very important meeting.

I'm somewhat taken back by the popularity of the high level Waste Package Program. It is quite interesting. I'm sure it has nothing to do with being in Pleasanton in January. I do understand that you are considering Camden, New Jersey in July for your next panel meeting and we'll see if we get the same attendance.

On a more serious note, it has been about a year since the Board came into existence, so I think it is worth reflecting just a couple of minutes, even though this is a panel meeting, that I believe the general view of the department, and certainly my personal view, is that the Board and the various panels have done a great deal in accomplishing a really remarkable amount in getting organized and getting on with the actual work in this first year.

I think we are quite comfortable with the fact that the Board has invested itself across the entire program and has had wide-ranging discussions and gotten involved in many of the key issues early on and I think that it shows already. I've said this from the very beginning, as soon as I started interacting with the Board, that it is quite clear that there is going to be a significant benefit to the program, not only technically, but to the overall structure and credibility over the long term to the program by having you look at our program and its key issues.

I think it is fair to say that we are looking forward to the Board's first report. We obviously have dealt with all the panels in a fairly open way and have a fairly good sense of what some of the key issues are and some of the key concerns. Nonetheless, that report will be taken to heart and certainly provide us with a great,

great opportunity to sharpen some of our program issues that need to be addressed.

Today's discussion and tomorrow's, as you mentioned, are focused for the Board on the Waste Package Program particularly the environment and the Containers Program.

We plan on talking about the waste package environment, candidate materials that are under consideration for use in the containers, the strategy that is being used currently to select the reference container material, the corrosion properties of the potential candidate materials, and alternative materials and concepts.

But I think it is also fair to say that nobody believes, I think, that we have a corner on all the information or knowledge that is necessary to be successful in this program. It is a very demanding issue. I take to heart the comment that he just made as to the change in the program schedule and the opportunity to broaden and deepen this program.

I think it is fair to say that there are a number of key issues about waste package performance and overall repository performance that need very serious looks.

Issues, for example, about the viability and demonstration compliance and regulations as they currently exist, such as whether or not there is value in targeting the number of waste packages and a number of other very important issues.

I think the idea of expanding the program is one that we hope we will have a better sense of after we have a chance to talk. Obviously, we in the department have to realize and you have to realize, too, that an era of restricted budgets that the department is facing this year and next that priorities become important, and it will be valuable not only for the panel but for the Board to help shape both where they think some of the priorities are in terms of resource applications and it also wouldn't hurt for you to take enough of a look at whether you think the program scope, and therefore, the resources that apply to this program are of the right magnitude. In other words, whether we can get the job done with the resources that we do have and to realize the budget realities in that regard.

The agenda that we set up in this meeting was developed directly in response to a letter that we got from Executive Director Coons on September 13, and I'm very hopeful that this process of early consultation on setting the agenda early so we can concentrate on what we are interested in, I think, that is helpful and I want to continue to encourage it and I have every expectation that your agenda here will meet your needs as well.

As the Board has asked, and I think is appropriate, the presentations will be made largely by the specialists who are actually doing this work. And in this case, it is predominantly people from the Lawrence Livermore Lab.

And without further ado, unless you have any questions or comments that you would like to address to me, I plan on turning the meeting over at this point to Jack Hale who is the chief of Surface Facilities and Waste Package Branch from headquarters and who will make some further introductions.

DR. PRICE: Dr. Isaacs, I would mention that Dr. North and Dr. Langmuir, who we mentioned earlier, are now at the table.

DR. ISAACS: Thank have you much.

MR. HALE: At this time I would like to take the time to acknowledge several individuals who do play a key part in the Waste Package Program. I believe Ralph Stein has already been mentioned but Ralph is not with us today. He is the associate director of Systems Intergration and Regulations headquarter's office.

And we also have Leo Little who is the division director for Engineering Development Division at the Yucca Mountain Project. The Waste Package Program is under Leo Little.

We also have Max Blanchard with us today who is the division director at the Yucca Mountain Project Office for Regulatory Citing and Evaluation Division, and I believe Max is here. Yes, Max is here.

I have also with me Allen Bersch who works for me and he is the program manager for the Waste Package Program.

I just would also like to express my appreciation to the Board for the opportunity to work with Dr. Jack Parry. I understand he is ill today, but I did have a number of interactions with Jack in developing an agenda which we hope will satisfy your needs, and I'm sorry that he wasn't able to be here today to realize the benefits of his work.

At this time I would like to introduce Michael Cloninger at my right who is the chief of the Field Engineering Branch at the Yucca Mountain Project. He works for Leo Little and is directly responsible for the Waste Package Program. Michael Cloninger.

MR. CLONINGER: Thank you, Jack. I think you'll find the Waste Package Program and the Engineer Barrier System Program to be one of the more interesting technical challenges probably in the entire human history.

Technical responsibility for that program rests at this point with Lawrence Livermore Lab under the direction of Dr. Les Jardine.

Without further delay, I would like to turn it over to Les Jardine. Les.

MR. JARDINE: Good morning. I'll break the rule by standing up.

Mr. Chairman, members of the Board and members of the Container and Transportation Panel, I'm Dr. Leslie Jay Jardine and on behalf of Lawrence Livermore Laboratory and the Yucca Mountain Project staff, I want to thank you for the opportunity to discuss various aspects of the technical and scientific programs being conducted by Livermore as part of the Department of Energy high level waste program.

I've appeared before you today as the leader of the Lawrence Livermore National Laboratory Yucca Mountain Project and I'm accompanied by members of the Lawrence Livermore Laboratory lawyers technical staff, ten of whom will be making technical presentations to you over the next two days that are in response to that September 13th letter that was sent to the Department of Energy.

Today, day one, the staff will discuss their technical activities involved in characterizing near-field environment in which the waste package is emplaced. Tomorrow, on day two, the staff will discuss various aspects of the container material studies that Livermore is involved in.

By way of background, and before beginning a kind of an introductory technical introduction, for me to set the stage, I want to take the time to say a minute that Lawrence Livermore National Laboratory is a Department of Energy National Laboratory. It was originally created in June of 1952 by Ernest Lawrence and the Atomic Energy Commission.

The Laboratory today is operated by the University of California for the Department of Energy. It occupies approximately one square mile and is about 12 miles due east to your back side of this hotel.

The Laboratory currently employs a multidiscipline staff of 8,000 career employees and about 2,000 additional multidiscipline contract people who are used to supplement this career lab work staff. The annual laboratory budget is about one billion dollars.

Lawrence Livermore National Laboratory is a large multiprogram Laboratory with major programs, which include nuclear weapons, design and testing, beam research, laser and magnetic fusion, energy, biomedical, and environmental research programs, among others.

In fiscal 1990, the Lawrence Livermore Yucca Mountain Project, which is the project that the speakers are involved in, employ the equivalent to 70 full-time staff members in terms of our budget for the fiscal '90 year. In addition, we have about 40 to 50 additional subcontract personnel at laboratories, other national laboratories and universities.

The Yucca Mountain Project at Livermore reports to to Mr. Carl Gertz at DOE, YMPO, the project office.

I'm the technical project officer for Lawrence Livermore which is the major participant in the DOE Yucca Mountain office. And I, as the TPO, am responsible to the Yucca Mountain Project for carrying out the assigned work scope according to the DOE guidance.

That Livermore work scope includes -- and I'll get a little more into that -- the characterization of the near-field waste package emplacement environment, and this includes the testing and modeling of the hydrology and reactive radionuclide transport and the altered geochemistry due to these temperatures, pertubations of the emplaced waste, the rock fluid interactions due to thermal and radiation effects, and the related field tests that are appropriate to develop this information on the characterization of the near-field environment.

We also are involved in materials testing of both the potential waste container materials and the waste forms. We also have work scope that involves geochemical modeling, performance assessments of the engineered barrier system and the waste package and the waste forms. There is also some engineering studies that are performed as part of the whole waste package engineered program at Lawrence Livermore.

So why don't you put up the first slide and let me sort of say what I would like to do is I've made some introductory-type remarks and what I would like to do is give you a flavor for the organizational summary that we have at Livermore and move in to give you a real picture about the waste package, as to how it relates to the repositories, as it refers in any given version to repositories. So the next slide, please.

Regarding the organizational aspects, this is the Yucca Mountain Project program office. As I said, at Lawrence Livermore down here, it reports to Carl Gertz. There are three other laboratories and U.S.G.S. that have scientific responsibilities for the program. So let me go to the next slide.

The organization and use at Lawrence Livermore has this kind of structure where you can view this bottom box across here as the technical and scientific program. I, as the Yucca Mountain Project leader, have reporting to me resource, planning and control. What this represents is the planning and project control activities of our project. So this is where we do our long-range planning, our networking and also our accounting of the costs that are incurred in doing the work.

On this side we have a project administration side which are records and documentation control. A very important part of a project operation is controlling and documenting and capturing all the scientific and technical work.

I don't want to leave out the Quality Assurance Program either, which is the most important and at the highest and I want to make a few comments about that.

We at Lawrence Livermore were audited in June of last year and received several months later through the review and approval process -- and it is not an approval but an acceptance by the NRC and the DOE of the Lawrence Livermore QA program as fully qualified to do work.

That was in June of last year, and we are quite proud of that accomplishment and that is why we have a very strong project administration activity that supplements that and will keep us on track.

In terms of the technical programs, there are basically six technical areas represented by those boxes. Today the agenda will not talk about our activities and waste form modeling and testing and geochemical modeling, performance assessment or systems engineering and engineering studies.

Those will be touched on but the agenda really revolves around the near field environment modeling and testing which is today's topic and Dale Wilder, who I'll introduce later, will lead that discussion.

The topics that this technical group is involved in includes hydrology, geochemistry, mechanical attributes, G-tunnel tests and radiation interactions. These are the five bulletin topics that you'll be hearing about later this morning.

A second major technical area, which represents tomorrow's discussion, is container materials, modeling and testing technical area where Will Clarke is the technical leader. Tomorrow's agenda will touch on the topics that were involved in material selection, material performance, corrosion properties and alternative materials studies done to date.

Now, to set the background for the subsequent speakers today and tomorrow, I want to put this up and not spend a lot of time on it but basically this is the Yucca Mountain site, the surface materials out here and the access to the underground by ramp. The waste packages that we are going to be focusing our discussions on are the underground repository horizon in this area.

So let me have the next slide and make some comments to put a little perspective in the back of our minds about some of the things that go on in this repository, the annual throughput involved 3,000 tons of fuel each year, 400 MTU of the vitrified high level waste plant. And this is basically a 70,000 storage ton capacity with mixtures of spent fuel and high level waste.

These are the products, if you like, that we have to put into the underground repository horizon. Depending on the specific packaging configuration, this translates to over the life of the repository, somewhere between 25 to 30,000 spent fuel containers, or containers filled up with the spent fuel, and about an additional 14,000 containers of the vitrified high level waste that corresponds to this level of capacity. That is the quantity, if you like, of containers. Let's go onto the next slide.

Now, moving into this in a little more detail, as far as the repository horizon, the waste would come down from the ramp of the underground and find its way through tunnels and drifts to the emplacement drifts. And then the emplacement drifts -- and I'm going to blow this up and I'm not going to spend a lot of time -- but these lines, these drawings, represent the emplacement drifts.

Then we go to the next slide, and we are moving into the waste packages and the containers of the waste that we want to be emplaced.

Now blowing up one of these emplacement panels where the drift is spaced not only 126 feet apart, if you look at a cross section through the emplacement drift, what you begin seeing is in the reference, SCP-CDR design, that the waste waters are alternating between the spent fuel container which is a little longer than the defense high level waste glass container and spent fuel alternators.

Nominally, they are ten feet below the base of the drift and emplacement. The width of the drift is 16 feet, and the standoff distance, you don't fill the spent fuel containers and the commingled configurations all the way out. Indeed, you stop somewhere, like, 85 feet short of an intersection of one of these drifts.

So the topics that we are talking about are these particular waste containers which are in boreholes in the emplacement drifts. Then go to the next slide.

Now, looking at a little more detail of what those waste containers look like, there are two types, as I mentioned. Those containing spent fuel are nominally 15.6 feet tall and 26 to 28 inch diameter, depending on the configuration of the internals which is a function of how much is consolidated or whether it is intact.

The wall thicknesses range between one centimeter to three centimeters depending on the alloyd that is under consideration. The defense waste glass containers are slightly shorter at ten and three-quarters feet, and that is the outside.

And what is inside of the actual container for the defense high level waste glass is the pour canister that would come from the defense program, such as Savannah River or two types of packages that we are going to Hanford. So it is in a container that is normally ten-feet high, 24-inch diameter and contains the vitrified high level waste glass.

So in most programs, the two types of packages that we are going to be emplacing and talking about in the next few days involve a container which contains spent fuel within its internal or a second type, slightly shorter but not the same diameter nominally, but which would contain the defense vitrified high level waste glass.

So if we can go to the next slide and blow this up one last time. One, in terms of whether a container, be it spent fuel or high level waste glass emplaced in a borehole, what is it that we are looking at? And this is our reference configuration which is a vertical configuration. And if this represents the drift floor in this cartoon form, nominally ten feet below the floor is where the drifting pintle would be on those containers in all of the configurations.

The borehole consists nominally of a hole, a borehole into the rock. The reference concept uses a liner which is a partial liner which comes down below the pintle. The idea and the concept is that it comes down far enough to get there to prevent other rock falling or other materials to come down to impede the ability for a retrieval of that container to come back with the replacement machine and remove this container. So the current concept in the SCP-CDR is a partial liner.

Looking at other aspects of this, there is a air gap or space, if you like, between the container and the bore hole wall. And since the liner stops up there, there is a space between the container wall and the rock.

Now, the near-field environment encompasses for about nominally meters, 10 to 20 meters or so. It is somewhat hard to define specifically the dimensions of how far the near-field environment is, but my perspective is where there are temperature gradients above the ambient perturbations due to the emplaced waste.

If you could go back to the earlier commingle of spacing, you realize that the spent fuel spacings are about 15-foot centers and there is spent high level waste glass between them. So at least there is a raise in the near-field environment overlaps between different wastes and fuel containers and the emplaced waste glass.

Now, I think that is kind of what I wanted to present here was a little bit of a overview to give you a picture of the underground repository configurations. The other speakers are not going to be doing this in detail. They are going to be focusing on the waste container and/or the near-field environment.

As I said, today will be over the near-field environment issue. The way that I've selected to run the program is to be fully responsive to your request and to get the real individuals doing the work up here presenting that. And the way I'm going to get there is to let Dale Wilder, who is the technical area leader for the near-field environment testing and modeling technical area, lead the discussion and be sort of the moderator of the technical work and the discussions today.

Tomorrow, we'll do it with Will Clarke who will lead the discussions of his people who are working for them directly and with actual scientists and engineers, but mostly scientists who are doing the technical work. That way, you'll be getting what I hope you want, such as people who are doing the work instead of figurehead types like myself or the managers.

So with this, I would like to introduce Dale Wilder who will lead the discussion for the rest of the day in terms of more agenda on the near-field environment.

DR. PRICE: I would like to make a procedure point just to be clear about the way the Board might interact with the presenters because I didn't make such a statement in my opening and perhaps I should.

I think if one of the Board members or panel members has a question that they want to ask, they probably will interrupt the speaker, not for the purpose of making it difficult for the speaker to proceed, but rather because it is on the mind at the time and it is just a little easier to work that way and it makes it a little more informal and we hope you understand this kind of procedure.

DR. CARTER: Let me ask you a couple of questions about things that I certainly as a Board member would like to hear addressed during the next two days. I'm not necessarily looking for a specific answer at this time.

One of those I'm very much interested now is measurements, laboratory experiments and field tests. In other words, actual hands-on research or testing versus literature, computer simulations and so forth. And I wonder if the speakers, if there is any question about it, would make it abundantly clear which of these particular areas, if I can divide them up that way, are being **discussed. This is one of the things.

The other I'd like to ask is whether similar or parallel efforts are underway as far as this kind of program mainly dealing with containers now, their interaction with the media at Yucca Mountain, and I'm particularily interested in if this is the case the technical scientific interfaces between the Livermore program and these other national efforts that may be going on.

Also the same would apply not only to such national efforts, but also those of an international nature. Again, I'm interested in the working relationships of the scientific and technical level.

The third area I would like to hear addressed, you mentioned the facts that you'd received or Livermore had received approval, if you will, for their QA program. I'm quite interested on the realty nature of this, how much of the program effort again from the scientific technical standpoint goes into QA efforts.

I think this thing almost has a life of its own or a driver of its own and I'm sort of interested on a national basis of finding out how much effort is really going into quality assurance. Is this 10 percent or percent? I dare say in many cases it might be considerably more than that.

So I would be interested in the perspective now of individuals doing the work as far as the QA contributions of either the helpfulness they are getting in that area or if it is an onerous responsibility, I would be interested in that as well.

So those are three things, and I hope that I've made them clear, that I would like to have addressed during our next two days of meetings.

DR. JARDINE: Should I respond to any of those?

DR. PRICE: Sure.

DR. CARTER: Be our guest.

dr. JARDINE: Well, we will be in varying degrees because in terms of whether something is a laboratory or field experiment versus theory, I think the experimenters will or the presenters will need to make that clear and we can do that.

But in terms of what is coming up on your agenda, on one extreme is Abe Ramirez with a total field experiment in Nevada. And then some of the other speakers, like Dr. Glassley, are a mixture of Laboratory experiments and some theory and we have some pure theory in the Nitao-Buscheck but I'll ask them when they start off to make it clear what it is, how much so you can get that, so the Board panel members can get that.

I think as we go through the program we'll have to address where the overlap occurs, if there is any. Basically Lawrence Livermore Laboratory is responsible for the all of the near-field environment. And others may take exception to the statement that basically the Livermore scope of work and responsibilities involved, where there is temperature deprevations above the ambient field testing site. So in some sense, we have a strong relationship of defining the near field to far field interface and Los Alamos sits out in the far field and Sandia co-exists.

MR. CARTER: I presume that might be a fuzzy line in some cases.

MR. JARDINE: Yes, I think it is, because how you define this depends on the radionuclide driving forces, the temperature gradients, pressure gradients. There is overlap and more work has to be done in defining interfaces to make sure that the pieces fit together and that all of them are being addressed.

On QA, that is something that is still being -- I quess I'm trying to extract what is the impact on the scientific staff -- but I'm relatively new to Livermore, joining them last April, and I came in new to that organization. They stopped in December and the staff stopped the technical work and began the implementation of a QA program with help of supplemental labor and outside labor people experienced in nuclear quality assurance programs.

What I found was an amazing workable system of procedures and processes that were rather straightforward in the sense of allowing, you know, project control type things to be done. They got there by using the outside experienced nuclear people and the scientific people working together with the scientific people writing the procedures but interacting very closely with the experienced engineered types that knew the procedure was in the 6 to 10 page range as opposed to an order of magnitude higher.

So the program at Livermore is implementable in the sense it doesn't tie yourself into knots and the QA program, in a sense, looks like it can be done. The difficulty, what happens is that it requires an up-front planning and commitments of all the work you are going to lay out in the scientific R & D process.

This is where it starts to get difficult in our research environment is how can you write down and do all your up-front planning which the QA program requires because there is some connection in the process between you have to do some scientific work and find out where you are and go back and make a change, but your planning has to have a way to accommodate this.

So what we have been doing in the last year is doing this up-front planning, of what is called scientific investigation plans or study plans, and then we write lower-tiered activity plans which are more detailed planning of the work and experiments that the scientific staffs are going to do.

Additionally, you have to write additional technical procedures which might involve colleagues in how the experiments are conducted, and this translates to a lot of time to the staff. Not only are the procedures in place with this up-front planning but in getting all of the requirements in place before they are allowed to proceed. And we use the word readiness review or preparedness review to be allowed to start work.

And at this point, we have about 13 technical activities that have been through the readiness review in order to start technical work that is under the fully accepted QA program. And it translates to somewhat of a time delay on behalf of the staff to be out, to be able to be in a position to go forward with the work.

In terms of resources, it is hard to quantify that because the program was basically accepted in June. The planning is underway, and we have some technical areas that have worked that are being implemented now.

If I had to make a projection in terms of the cost, there is no question it slows down because you have to get the planning in place before you can start the work. How you quantify the dollar amounts or the time amounts are difficult and they will be changing. But if I had to guess, they are in the 25 to 40 percent range for this year but I expect those to go down, not much. I mean, the range won't go down. The range will narrow.

DR. PRICE: What is 25 to 40? What is 25 to 40 percent?

DR. JARDINE: You mean in terms of dollars?

DR. PRICE: No. 25 to forty percent spent in readiness planning.

DR. CARTER: For quality assurance.

DR. PRICE: For quality assurance?

DR. JARDINE: Well, getting the up-front planning done before you go into the Laboratory or into the field or into your computer system to start doing what might be a technical product. There is a lot of up-front work that is needed. And once that is in place and you can move forward, it still will force a different way that the work is done by the scientist and the engineers in an R & D environment. I believe it can be done.

There are some people that would make the comment that there are still some scientists, some of the better ones, that will struggle harder to write down and do this up-front planning. But we have a very good success rate at Livermore because the staff wants to do this or is willing to do this.

DR. CARTER: Well, my comments, I was not only interested in your views on this but I think I would also be interested in the views of the individuals that are involved in various aspects of the work, because eventually we have to come to grips with the amount of resources that are going into QA and whether or not this is a balanced and reasonable kind of effort.

Obviously, this may take time with a lot of people working in the area. But I dare say a lot of them are not used to these kinds of procedures on a formal basis, even though they may have gone through quality assurance, quality control in other ways.

But the question is is it really intergrated into the total program now on a working basis or is it just an appendage that you have to work with that is completely separate.

So we are have been much interested in this intergration of QA. And like I say, also the amount of resources that go into it. Does it cause substantial delays in getting the job done and other kinds of pressures such as that.

DR. JARDINE: I think within Livermore we are very intergrated in the sense that our whole QA program is in place. It is just that it is difficult for a lot of R

& D types to make a decision and write down the plan and commitment to how they are going. What it translates into is a little bit of a delay. Maybe that is the wrong word for it but what is it that you are going to do, and how are you going to do it, and in the end you better have the paperwork to show that you have done what you said you were going to do.

And that is one way to summarize the steps of QA. But I think we are getting there and we are just about to begin implementation of our softwear QA program plan which was approved in December, and so we are now moving into a newer era of trying to implement the software quality assurance program requirements into the programs at Livermore, and we are just now beginning that having received approval in late December.

DR. PRICE: In your organizational charts, you presented six different technical areas or programs. Are these about evenly described or does the distribution vary with time?

DR. JARDINE: I would say it varies with time. But during fiscal '89 it represented a reasonable balance, although the lighter areas might have been in the systems engineering, engineering studies type activities and the performance assessment integration portion. And we are still adjusting for the fiscal '90 work scope.

Any other questions?

If not, then I would like to turn the program for the rest of the morning over to Dale Wilder, who I mentioned is the technical area leader for the near-field environment modeling and testing technical area. Dale.

MR. WILDER: I appreciate being able to speak to you this morning. I'll take just a few minutes to give you a little bit of background as to what it is you are going to be hearing in the environmental area.

A couple of things that I would like to point out. And that is that in terms of the waste package, the environment that is of consideration is what we term the near-field environment. I would distinguish that from the the overall repository conditions because we are going to be concerned about that rock that interacts with the package directly.

In defining near field, as Les pointed out, it is not always easy to define where the near field stops and the far field actually begins. But in defining that, I would point out that it is going to depend on what the process is that is of consideration.

For instance, if we are concerned about temperatures, the near field may extend at a different scale than if we are talking about stress relief and boreholes. And so we define near field based on what it is that we are looking at rather than some specific geometry.

The other thing that I would like to point out is that for the discussions today, we are going to be focusing on the environmental container material interactions and, therefore, we are stressing this thousand-year containment period. We certainly are considering environmental conditions beyond the thousand years, but those have more to do with the interaction with the waste form than with the container materials.

And so today, you'll be hearing presentations largely focused on the thousand-year containment period.

The next slide shows the areas that we are going to be discussing today. Specifically, we are going to be looking at those issues of concern in terms of interaction between waste package container materials and the environment, so we are going to be focusing on the amount and transport of water and water vapor that can contact the packaged materials, the composition of that water, the mechanical loading on the container, the thermal conditions and the radiation-chemical effects.

I would like to point out that we are going to be using some terminology that I need to discuss to make sure that it is clear. Two specific terms that you'll hear repeatedly are "expected" and "bounding." When we refer to expected, we are talking about conditions that we judge are most likely to occur during the period of interest, as contrasted to bounding, which these are conditions that we judge to be beyond the expected values but still within possible ranges.

As an example, to maybe further clarify this, let me talk about matrix in the next slide, the matrix saturation conditions as we currently understand them at Yucca Mountain. We expect 65 percent saturation. It is an unsaturated site but with approximately 65 percent saturation with ranges that have been estimated right now, plus or minus 19 percent so it would be between 84 and 46 percent major saturation.

However, calculations that have been done based on at least one scenario for flux of one millimeter per year indicate that we may be able to expect as high as 95 percent saturation under those conditions if we use the characteristic curves as they will have been put together by Sandy in the Laboratory.

Therefore, we are going to consider in terms of the design envelope those conditions that are bounding, not just the expected conditions. So we are going to spend a fair amount of the morning talking about bounding conditions, not just expected conditions.

The next slide will summarize the conditions as they exist. Call them either unchanged or ambient conditions. In terms of hydrology, we expect an unsaturated site with about 65 percent saturation.

The air that is in the remaining unfilled void, we do expect to be moist there, and our best estimate is that it will be an equilibrium with the water in the flow and, therefore, at 100 percent humidity.

There is going to be a very high matrix suction potential. And therefore, we do not anticipate fracture flow. Some estimates of fracture flow, if it is at equilibrium with the matrix, is that we will not initiate practical until we get to about 95 percent saturation.

In terms of water chemistry as we understand it, it is dilute bicarbonate water. We expect that the vadose water chemistry will be in equilibrium with the Topopah Springs Tuff. Now, I'm saying it is unknown. I should say actually it is not well understood at this point.

We certainly have got some samples available of water but not from the Topopah Springs repository rising rock.

You'll also hear discussions of J-13 well water, and that is representative of the water but it is not an equilibrium of Topopah Springs. Let me just point out that J-13 is a well that is completed over roughly a 40-mile wash where the water table is within the Topopah

Springs unit.

The repository is in Topopah Springs well above the water table. So J-13 water is merely one of the water chemistries that we are considering, but we are not going to say that J-13 is representative necessarily of what we will come in contact with in waste packages.

If I can go back to the other slide. In terms of mechanical loading, we do not expect any hydrostatic loads obviously if it is an unsaturated site. And because of the air gap and the borehole design as currently existing, we are anticipating no lithostatic loads and no point loads.

In terms of the expected conditions for thermal, we do recognize that there will be conductive, radiative and convective or latent heat transfer.

In terms of the bounding conditions, I'm sure that some of these numbers are familiar to you from the SCP site disposition plan, but in terms of the performance allocation that was made for the waste package, taking into account as five liters per year per borehole for up to five percent of the waste packages.

Now, this requires either a much higher than expected flux or some sort of a high infiltration pulse. Just to put this into perspective, at a half a millimeter per year flux, which is currently our best estimate or the project's estimate of flux, you would have somewhat less than .3 liters per year if it was strictly matrix flow.

Therefore, as I say, we would have to have much higher than expected flux for some sort of fractured flow to occur. We are also for design conditions considering the chemistry much more concentrated than expected and some of the testing that you'll hear discussed have used concentrations of 20 times J-13 water.

Some of the other work is looking at the ranges of chemistry that will bound the program.

In terms of loading, we have considered the possibility based on the size of the borehole and the fracture spacing of blocks up to 3,000 kilograms being possible to come into borehole. Of course, that could result in point loading.

We are also looking into the possibility that sloughing might occur, which could provide some sort of bridging for partial application of lithostatic loads.

In terms of the thermal, the two things that we are considering in terms of bounding is that there may be more liquid water present which would result in greater vapor transport and, therefore, influence on the thermal gradients and also geologic variations in terms of the thermal productivity.

DR. CARTER: Let me ask you a question about the chemistry there. When you say chemistry more concentrated than expected, do you mean certain ions are more highly concentrated than other ions?

MR. WILDER: We are considering individual ions and we are also considering taking the chemistry of the water and esentially just evaporating it into a concentration which is 20 times, in which case all of the ions will be more concentrated.

We are going through both approaches, and Bill Glassley will be able to define that in some detail in terms of how we find that.

DR. CARTER: Thank you.

MR. WILDER: Now I would like to make some points about the waste package environment, and I've already alluded to one of these, and that is that the emplacement environment is not going to be an ambient environment because it is going to be perturbed by a number of activities, one of which is construction activities.

Certainly in terms of once waste emplacement takes place, there will be perturbations and those perturbations will be changing with time. That is, the thermal, radiation and chemical reactions are all functions of time themselves and, therefore, the perturbations will be changing with time.

And once again, hold onto that slide and go to the next one.

Let me as an example look at the issue of the quantity of water. Now what you are looking at are computer calculations for vertical waste emplacement based on spent fuel activity about eight and a half years out of core. And as you can see, the isotherm does move out with time.

In this case, we are only looking through ten years of time but there are some things that are taking place and, therefore, the environment is going to be changed or churned with time.

When you first start, the boiling point isotherm is fairly close to the waste package and, therefore, the vapor as it is driven off can certainly flow into the bore hole and it will flow out into the rock. As it continues to move, the vapor will preferentially start going into the rock more than into the borehole as your path length becomes longer. And we also will build-up the water out here in what we call the saturation halo, if we could have the next slide.

DR. LANGMUIR: Is this a 250 degrees Celsius waste package that we are talking about?

MR. WILDER: Roughly. It is 3400 kilowatts waste package and the wall temperature -- I may have to ask Tom to help me with this -- but as I recall, it is about 200 degrees or 250 degrees through the borehole wall. Okay. If I can skip down.

Another way of looking at that same phenomena that I just went through is once again this is a computer simulation but it is showing the distribution of saturation with distance.

At 25 years after emplacement, you see that there is a dried-out zone and referred to that behind the boiling point isotherm, and there is also a zone in which you'll have active drying occurring, a fairly narrow zone.

Now in this particular case, I should point out that these are calculations done for horizontal emplacement. However, the power density is very similar to 15-foot long, 15-foot spacing vertical bridge emplacement. So what we are looking at can apply also to the vertical.

So what we are seeing is that there is an active dried zone and then there is this zone where the saturation has actually been increased where you do have the condensation of the vapor taking place.

One of the points that I would call to your attention is that the temperature in this zone is elevated, so we are adding water to the rock at elevated temperatures, and this is going to be a very active zone for geochemical processes. And one of the things that

Bill Glassley will talk a great deal about is those rock water interactions that will take place.

Now if I can go back to the other slide. It is going to be very important for us to understand the ambient conditions in order to evaluate conditions.

The final point that I will make is that because of the changing conditions with time, it is not going to be possible to come up with a table or matrix containing single value parameters for the environment. Rather, what the table will consist of are functional relationships, for instance, the saturation of the time, the temperatures at the time as we just looked at.

DR. LANGMUIR: Would you be prepared to discuss what might happen should you put 100 degree Celsius waste as opposed to 200 in the same kind of setting? What would that result in terms of the fluids in the system?

Now that might influence the chemistry. I would like to hear something of those possibilities.

MR. WILDER: Okay. We've not gone through all of the calculations for the colder waste. We are in the process of doing that, but I'm not sure that I can give you all of the answers that you are looking for at this time.

I think that Bill will address some of the temperature dependencies of the geochemistry. But in terms of the hydrology, we are going through those calculations right now for essentially a 20-year old spent fuel rather than an eight-year old, so I can't give you all of the specifics on that today.

I mentioned the change of conditions with time. This is an effort on my part to try to show this pictorally. You've already seen the one distribution and I should have pointed out that the distribution that I was showing is conceptual.

We do not have yet sufficient numbers to really say that is the actual distribution of the expected Yucca Mountain but I'm using it to illustrate what we are going to be looking at. So you've seen the ambient condition distribution with essentially 65 percent saturation. We anticipate due to ventilation -- and this is assuming dry drilling or dry mining techniques are used -- that there will be some drying of the rock in the near field.

What that drying is, I can't tell you specifically, but I'm trying to show that this distribution will probably be broadened out and it will be essentially dryer rock. Certainly after emplacement, it is going to be askewed toward the zero saturation in the near field.

Then as time goes on, it will eventually return to the ambient conditions, assuming that the flux remains as it is currently, and that all the conditions are the same.

So when I said that there will not be a single value table of parameters, I think that this illustrates what I'm saying. We'll have functional relationships and those functional relationships will themselves be changing with time.

DR. PRICE: Let me just ask on the ambient, where you have the matrix saturation, in the other slide, I assumed a normal distribution in your bounding and expected conditions were on three sigmas?

MR. WILDER: At this point it is really conceptual but it would be essentially one sigma for the expected and the bounding. At this point I don't have the numerical number to give you, but I'm sure that it would be out at least two sigma.

Would you put that one back on? There was one more point to make. Because the dry conditions that we are expecting after emplacement up to some time between 300 to maybe as much as 1,000 years, we are very concerned about evaluating the pulse flow in fractures.

Matrix flow is not much of an issue in terms of getting water into the waste package. So today you are going to hear a lot of focus on the fracture flow versus matrix flow.

Once again, going back to CODE 3 calculations. As you can see we are going to remain above the boiling point. This is for the spent fuel case eight years out of reactor, 300 or 400 watt power output, for a significant portion of the containment time. As a matter of fact, on this calculation, it shows that we would not drop below the boiling point on the borehole wall itself until after a thousand years.

So that is why I say we are going to be focusing on those bounding conditions in which we could get flows specifically to the fracture flow.

One final point that I would make before we get into the actual technical discussions on the next slide is that there are some scaled dependencies that we have to consider in the work. So I would like to review with you essentially three different types of tests that we have either already conducted or in the planning stages.

The short duration tests -- and by "short duration" I would say somewhere about the one to two-year time frame, are usually overdriven. And we are looking at either overdriving the temperatures or cooldown or something in order to observe the processes that would normally take too long to observe.

We will be talking about one of these, the G-tunnel or prototype testing, certainly our lab testing. One of these is yet in the planning and that is the Exploratory Shaft Test No. 1. These tests help us to identify physical processes and to provide parameter values as well as to test the models over limited conditions, and those conditions are limited because of the overdriving. And that does, therefore, give us some limited model validation but certainly not total model validation.

We also have planned longer duration tests, somewhere in the three to five-year time frame. Although depending on the schedule, they may be longer than that. These tests will usually be partially overdriven.

As an example of this partial overdriving in our Exploratory Shaft Test No. 2, we will be overdriving the heat but then cooling at the more typical rate so we can watch the cooldown. Whereas in Test No. 3, we will be heating at the more typical rate and then accelerating the cooldown.

These kinds of tests allow us to test models over more extended conditions. They certainly give us extended model validation. And it is during these tests that we plan to address the geochemical and geomechanical aspects which we've not yet incorporated into our field testing.

And finally are what I call performance confirmation tests. These are more representative of the actual ratings. And two types of performance confirmation tests that are currently planned, one is the No. 2 test in the exploratory shaft has been identified as one in which we'll have long-term cooldown and that will allow us to validate the cooldown portions of the models because if you use the second test, the actual waste package monitoring, you'll never get into the heating cycle before we run out of time.

And so in this test, we will be looking at the actual heating over the 25 to 40 years that it takes for that heating to peak.

Now, I mentioned the fact we are going to be looking at fracture flow. These once again are calculations that were done for the vertical emplacement case, once again with that same eight-and-a-half-year old spent fuel.

The point is that we will be placing these isotherms between drifts and, therefore, driving the moisture vertically upward as well as downward. And that will allow us to have essentially a heat-driven perching condition in that this rock will be at elevated saturations, possibly approaching 100 percent.

And if you have a fracture which then intercepts a waste package, we need to know what is the potential for water to flow down that fracture.

So the next presentation by Tom Buscheck and John Nitao will focus on this mechanism in which you can impact the amount of water or water vapor that contacts the waste package. And basically they are going to be talking about the interaction of flow into the matrix, specifically looking at conditions from which we can get fracture flow and also looking at matrix dominated flow and the role that the fractures play in hindering or assisting that matrix flow in rock.

So I'll turn this over to to him.

DR. BUSCHECK: I'm Tom Buscheck and I would like to emphasize that my talk and John Nitao's talk are really one talk. And consequently, John will summarize both of our talks at the end of his talk and I'll just provide a transition.

DR. VERNIK: I wonder if you would mind getting the microphone a little closer to you.

DR. BUSCHECK: Because of time today we are not going to be possibly able to address all the aspects of the modeling work being conducted at Lawrence Livermore National Lab.

There are three primary areas that we view our work from. We have been undertaking code development which is taking the TOUGH Code from Lawrence Livermore Laboratory and substantially modifying it and enhancing its performance and adding additional physical effects that we require in our model calculations, and we've undergone some partial modification of that code.

I have just mentioned some other types of models that we anticipate the need for in the future which we haven't currently developed. Our work in our modeling studies fall under nonisothermal modeling studies and in detailed fracture-matrix interaction numerical and analytical modeling studies.

I would like to emphasize, based on a comment during Dale Wilder's talk, that while our work involves numerical and analytical interaction, that our work is done in close collaboration with laboratory and in situ field experiments.

Up front, one of our primary purposes for conducting a nonisothermal model study was to aid in the design of Laboratory and particularily in situ field tests. We needed to know the extent to which the thermal perturbations would occur in our experiment and we needed to know the range of parameter values that we needed to measure.

So it was a very essential part of the design and the implementation of the G-tunnel experiment, which

Dave Ramirez will be presenting later this morning.

Our nonisothermal modeling studies, I won't go into detail here, but the result of looking at Laboratory and field scale models along with mechanistic models and theoretical analysis of scaling laws all together relate in what I call intergrated parameters in situ field scale models, able to present the results of one such model and compare those results with the in situ field tests and G-tunnel.

We also concurrently are taking a detailed look at fracture-matrix interaction, and have developed a comprehensive theory which describes the primary fracture-matrix interaction.

This work impacts our intergrated parameter in situ field scale models and it also will impact case specific scenario deliberations at a development of environmental parameters with which the waste package container people will need to work with in assessing performance of the waste package.

Move to the next slide. Could you show that last slide? Just keep in mind this slide here.

I'm going to show in somewhat detail what the fracture, some of the fracture-matrix interaction of what is occurring within this zone here in the near field environment of the waste package.

This is a conceptualization of how fractures of matrix blocks affect drying and wetting behavior within the near field around the waste package. Heat is radiated, while heat flow from waste package primarily occured as thermal radiation from the waste package to the wall of the borehole. Also heat conduction and convection play a role here. Heat is then conducted into the fracture of rock mass, and these represent idealizations of fractures. As the temperature rises in matrix blocks, evaporation and then eventually boiling occurs. And because of the fact that there was gas pressure radiant required to drive the vapor out of the block, we get an elevation of pressures as we move into the block to consequently an elevation of the boiling conditions.

So as a result, boiling then subsequently occurs from its regularly released matrix block into the center of the matrix block. We found in the field and in our studies that fracture in the annular spaces within the waste package and the borehole wall exists ambient pressures due to the fact that the hydrolic productivity of the fracture in the annular space is quiet high.

So gas, then, or water vapor reaches these fractures and is driven under gas pressure, including buoyancy effects on the fracture network of the rock. And this vapor flow occurs until it reaches temperatures which are cool enough to cause condensation to occur along the walls of the fracture.

Then one of three things can possibly happen. The water can be immediately imbibed into the matrix block.

It can also, if the saturation level, if the imbibition is not sufficient, the saturation levels may be obtained high enough to result in liquid mobility of water in the fractures. For fractions oriented downward, this results in gravity drainage back towards the region of boiling where we get what I would call a gravity driven heat pipe effect.

In other fractures which are radiated away from the boiling zone, we can get net drainage of this condensation away from the boiling front. And then within the rock mass itself, water vapor will also be driven towards the condensation front, condense, and then water under imbibition forces can be driven back towards the boiling region and possibly form some sort of heated pipe effect, though it may take some time for such heat pipes to develop.

As I stated before, as the water is moved along fractures, it will be continually imbibing into the matrix blocks, developing that condensation "halo" that Dale referred to earlier. Next slide, please.

Dave is going to present pretty much the up-front preliminary validation picture of our integrated model as compared to the G-tunnel experiments.

I just want to make a few conceptual comments about the results of our numerical studies, and particularily how these results impact future planning and testing. We find that the waste package geometry and particularily the heating rate of the waste package very strongly effects the near-field moisture movement and temperatures.

Particularily we found that through our scaling law analysis, that the drying rate is strongly dependent on the heating rate. And for radial flow, we found that the drying rate is dependent on the heating rate five halves power, which is very strongly superlinear.

What this implies is that -- well, in G-tunnel, we overdrove the rates but, in fact, the rates relative to the lower end nominally for waste packages were overdriven by a factor of approximately three. Applying this relationship, we would require at least six years to conduct an experiment which was conducted roughly over a one-year period of time to reconduct the G-tunnel experiment at the low and nominal rate.

The reason I mention that as an important consideration is that later on this morning we are going to be hearing hydrothermal-geochemical coupling and thermomechanical coupling in the near field. These couplings can be substantially distorted for tests which are strongly thermally overdriven. And while the thermally overdriven tests are important inasmuch as we have a relatively short period of time to thoroughly perturb a large volume of rock, it is still very important that we currently conduct tests which heat the rock at the actual nominal rates that the waste package will be heating in the environment.

Consequently, based on the strong nonlinear relationship, we find that we are going to be needing long, long periods of time to appropriately thermally pulse the rock in order to see the types of couplings which will pertain to actual repository conditions.

Another point that needs to be made is that the fractured rock itself is a very heterogenous system. In fact, fracture spacing can vary quite widely. And inasmuch as we are going to have to validate models in situ, it is going to be very important for us to hydrothermally perturb a volume of rock which extends over the scale of heterogeneities, mainly to the fractures and matrix heterogeneity and the matrix properties.

In G-tunnel we perturbed about 1.4 meters of rock with respect to saturation changes. And that, in fact, is substantially smaller than the scales of the observed heterogeneities, so that should be bore in mind when it comes to planning future testing and the length of testing that we reequire for adequate model validation. The next slide.

The focus of the rest of this talk, because of the importance on waste package performance of the fracture matrix interaction, and as Dale stated, the significant amounts of liquid contacting the waste package, we feel can only be facilitated through what we call fracture-dominated flow.

Besides temperature affecting the performance of the waste package, two other very important perameters is the quantity and chemistry of water contacting the waste packages Two of the important questions that need to be answered, first of all, is where can this water come from?

There is a possibility that low permeability lenses above the waste packages could result in the occurrence of perched water. The introduction of the drilling in the construction fluids and drilling fluids, and the construction fluids will result in conditions being other than ambient at the time of emplacement.

Vapor condensation as we've shown earlier can be quite significant during the heating phase and can result in areas of full saturation around in areas of the repository. Also the variability of rainfall events needs to be considered.

And another topic which I think we will be receiving additional look in the future is the seismic pumping of the water table. And that fracture-matrix interaction also affects our analysis of that problem.

Then in order to understand the chemical evolution of that water as well as the quality, we need to know how it is getting there, whether it is a fracture-dominated flow system or matrix-dominated flow.

This will tell us if the residence time of that flow will affect quite a bit of that chemistry, as well as where it comes from will affect the chemistry of that water, and it will also affect the flux of water contacting the waste package. Next slide.

We have been addressing fracture-matrix interaction with a combination of numerical analytical models. This process has been a very interesting one. We additionally conducted some numerical experiments that were done to scope out the impacts of drilling fluids near field, drilling fluids which would be used to put instrument boreholes into the heater borehole from the G-tunnel experiment.

So from that work, we found some fundamental theoretical relationships which, in turn, we then looked to analytical models to further understand. From our analytical models, we then went back to our numerical experiments to corroborate the applicability of the analysis and it also brought up new problems that we thought would be relevant to important questions of waste package performance, and I'll focus on the numerical experiments in the next few minutes.

In this analysis we found that there are three primary types of flow: Fracture-dominated flow and matrix-dominated flow, the two that I've already mentioned, and also a transitional fracture-matrix dominated flow which occurs.

Through our analytical work we have developed flow criteria which tell us which type of flow, whether it was fracture matrix or transitional flow, is likely to occur. And John will describe that in the following talk. Next slide.

I have to quickly give an example. One of the early samples that we looked at for fracture-dominated flow -- what we have here is a system of vertical parallel fractures which are uniformally spaced. We have a fixed pressure boundary condition at the top of these fractures which would pertain to a pond of water at a constant depth.

These fractures have an aperature of 2b and a spacing of 2a. The matrix is initially less than 100 percent saturated, which in the case of this particular example of 65 percent, and due to the capillary forces in the matrix, the fractures are essentially drained to water under these ambient conditions.

Because of the periodicity of this problem, we can focus on a subcell problem utilizing the symmetry down the midplane of the fracture with symmetry down the midplane within the matrix block. And in this example we are considering a 100 micron fracture or one with a 50 micron half width and a .5 meter spacing.

We conducted this experiment for 48 hours with a fixed boundary pressure which would pertain to a constant head of drilling water in a borehole and could pertain to a variety of problems. On the left here we are focusing on saturation changes within the matrix block.

I would like to point out that the matrix block, that this is 1.5 centimeters. We actually go out 25 centimeters. But for the space, we are only showing this amount, so actually the midpoint of the matrix is some distance out here.

What we have is that the fracture that we saw in the previous slide, this is the midplane fracture. This is the distance into the matrix block away from the fracture. And you see that we go down to 100 meters in this case, the vertical depth flow and infiltration flow.

What we've plotted here is the mid-saturation contour between 65 percent and the initial saturation of 100 percent. So this is sort of like the mean perturbations in the matrix with time. And we have looked at two-hour intervals going up to 48 hours along into the matrix block.

I would like to point out that this particular problem is reference parameters for the repository horizon which we considered the best possible available data to do this analysis.

In this plot here what we are looking at is the liquid saturation level in the fracture with respect to vertical distance below the constant pressure boundary going from zero to 100 percent saturation. So this is showing the liquid pulse of water moving down the fracture in two-hour intervals.

Something that became very apparent early on in this study was that for this particular example, and it is fracture-dominated flow, we found that there is a strong amount of matrix interaction which we found resulted in the liquid front moving it at a two to the one-half power.

Even though we are fracture dominated here, conditions in the matrix have a very, very strong influence on the movement of that liquid front down the fracture, so we should not be misled by talking about fracture versus matrix flow. In fact, there is a very strong coupling in particular for a fracture-dominated situation.

DR. LANGMUIR: Can I ask a question?

DR. BUSCHECK: Yes.

DR. LANGMUIR: Supposing the fractures are lined with secondary mineral phases. Is this going to influence your movement into the bores?

DR. BUSCHECK: It will. We are starting to look at that type of problem. We don't have results today to show you but that is being considered.

DR. LANGMUIR: Isn't that a fairly realistic condition to be expected?.

DR. BUSCHECK: It is. I won't speculate now but I'll show in the future what that impact will be.

Next slide.

If you look at what happens at the end of the infiltration of that source, now after 48 hours we've removed that source of water. This is the main saturation deprivation in the matrix at 48 hours. And now we are going to look at what happens to fracture flow once that infiltration source is removed. And these are 0, 2, 4, and 24 hours after the removal of that source of water.

What we find is that within the 24 hours, that pulse of water in a fracture is essentially almost completely imbibed in the matrix with a net result that the toe of the liquid front barely moved than it had moved in the 48-hour event.

The result here is showing us that when the pulse of water or the source of the pulse of water is removed that matrix imbibition will mitigate any subsequent movement of flow down the fracture. Next slide.

We also found that the penetration depth within along the fracture is very strongly dependent on the aperature which is seen here. We found for this example if we went from 100 micron down to 15 microne fracture, that only a two-fold difference in a fracture aperature resulted in an eight-fold difference in fracture penetration depth.

John will show in his theoretical analysis why that is the case. But an important point to point out here is that fracture flow for these reference values is quite significant, 100 meters, and I think than points out the need for further study of this problem.

We conducted a parameter sensitivity study of fracture-matrix flow and considered using repository data horizon. We found for fracture spacings that are anticipated for the horizon, which remain yet to be confirmed, that we found out most of these cases result in a "t" to the one-half power dependence.

What we are finding here along the normalized fracture penetration depth with log of time -- and I won't describe what the normalized depth is but it is very proportional to the actual depth. And this is a very important observation which was made which aided subsequent analysis or analytical work that John will present. Next slide.

If we find, however, that if the repository that the fracture is substantially closer spaced than we've anticipated -- in this case the fractures are about five centimeters apart -- we find that, in fact, the flow fields in the matrix blocks for two neighboring fractures.

And again, this is a case where we have a system of uniform space vertical fractures. So that fracture spacing is substantially reduced on the order of a factor of 10. We find that the saturation perturbations in the matrix interferes between neighboring fractures.

And, in fact, for this example, that it only required three hours before the saturation fields and neighboring fractures started to interfere with each other. We found that as the saturation fields interfered, that the liquid front movement changed to a "t" to the one-half power dependent transition to a linear time dependence.

We can see in the movement of the liquid pulse down down the fracture, that if we plotted these earlier times, we can see that they are changing "t" to the one-half power. But in the transaction here, we transition to a linear time dependence which is much, much faster than a one-half power dependence.

Quickly we talked about matrix-dominated flow. An important outcome of our work is that we found that contrary to what some may believe, fractures are not capillary barriers to matrix-dominated flow.

What we have here is a fracture which is orthogonal to the direction of matrix-dominated flow or matrix plot. What these bridges here represent here is the fact that there are sparing contacts between neighboring matrix blocks and we have modeled these contacts essentially being where the fracture aperature goes to zero. Again, the matrix initial saturation is less than 100 percent or 55 percent and the fracture drained to water. And we also maintain a constant pressure boundary at the top of the boundary.

If we look at a subcell which occurs due to the periodicity of the problem, we get this particular sub element here. An important parameter is the length of the aperature, and this is the total length of that periodic subcell. And this ration, l "sub a" or l "sub t" is an important parameter to consider while viewing matrix-dominated flow across these asperities.

Next slide. Here we are looking at from the top of the infiltration source itself going down into the matrix vertically. We are looking at various values of l "sub a" or l "sub t". It is 100 percent down to zero percent which is virtually impossible, but we considered it to show what will happen if there was actually no contact between matrix blocks.

For 100 percent, we just get pure matrix imbibition into the matrix block and this is 1440 hours. We find that even for a very, very small value of one percent contact area, which is this curve here, we find that this has a very small effect on retarding flow across the fracture.

Even in the case where we have zero percent contact, we actually can get flow across the fracture due to the fact that the vapor phase is also active in addition to the liquid phase.

What happens is that we get relative humidity gradients across the fracture which result in an evaporation on the upscream side of the fracture and condensation on the downstream side. Next slide.

Right now I would like to introduce John Nitao. He is going to emphasize the analytical studies that were done in conjunction with our numerical experiments and he'll also illustrate our flow criteria that were developed.

DR. NITAO: My name is John Nitao and I'm a hydrologist working in the Hydrology Department at Yucca Mountain Project at Lawrence Livermore. I would like to talk on the subject of fracture-matrix interaction, continuing some of the discussion that Tom presented.

The emphasis of my talk is on some of the analytical modeling that we've studied here in conjunction with the numerical modeling as an aid to experimental and field tests. Next slide, please.

This is the same graph that Tom showed. This is just to show the importance of fracture-matrix flow for the waste package environment. And whether the flow comes through the fracture or the matrix is important because if it goes through the matrix, then the geochemistry would be different than the fracture flow. And also with matrix flow, you would get less of the liquid flux onto the waste package than if you had fractured flow. The next slide.

This is an outline of the work that we've done in local modeling. We've looked at a system of parallel fractures and performed two dimensional simulations and theoretical analysis, and we derived dimensionless groups for fracture-matrix flow and found the criteria for when matrix-fracture flow occurs.

By deriving dimensionless groups, we can reduce the number of parameters that would be necessary for experimental and field testing and also in our computer modeling that helps us to determine what range of parameters are realistic parameters for Yucca Mountain.

Hopefully it will be used for future computer models in a system analysis.

We've looked at both fracture-matrix dominated flow systems and found the approximate formulas predicting the travel of the water movement and found the expression for parameter dependence of various hydrological parameters environment and fracture-matrix based on flow regimes.

We believe that a physical understanding is necessary in order to have confidence in our computer models and in the experimental work. And we've compared our analytical work with numerical solutions and have pursued applications. Much of these areas are continuing development.

I believe we have really made significant progress in these but we are continuing in the effort of determining the dimensionless groups in developing more of a theoretical understanding of hydrological physical processes.

This is a similar slide to what Tom showed. This is the type of system that we are looking at. We hope to look at other types of systems in the future. We have a system of parallel fractures here with aperature 2b and spacing 2a. We are looking at this single cell here in our subsequent viewgraph.

Our analysis includes a few types of boundary conditions, constant head value at the inlet to the fracture which is corresponding to ponding at the borehole surface or at the drift floor. The other boundary condition we look at is the constant flux into the fracture which is correspondent to condensation of water vapor or seepage or surface infiltration.

Today we will just look at one aspect of our analytical work, and that is to answer the question: When does fracture flow occur for the type of systems that we consider? We first determined dimensionless groups for flow in fracture-matrix systems and we found that critical values for dimensionless groups for fracture flow to dominate.

An application of this or an outgrowth of this is to help to determine the range of parameters for which the equivalent continuum approximation is valid. The equivalent continuum approximation is that there is an equilibrium between the matrix and the fractures.

This viewgraph illustrates the principle that I'm trying to illustrate today, which is that if the inlet to the fracture is much less than some critical flux, then we'll have basically matrix-dominated flow where most of the movement in the water will occur in the matrix instead of the fracture.

If the inlet flux is much greater than the critical flux, then most of the flow occurred in the fracture and the flow in the matrix will occur orthogonal to the fracture. Next slide.

DR. LANGMUIR: Tom, can I raise the same question that I did earlier? How do you factor into the modeling effort, the real possibility of fracture of the mineral line? And therefore, you don't have continuity of fluid flow into the matrix.

DR. NITAO: The analysis, the results we show here do not take that into account, but the analysis used basically an imbibition of flux function between the fracture and the matrix. Right now we are in the planning process of looking at simulations which will basically modify that function and put a low permeability barrier coating on the fracture phase. Those are simulations that we plan to do in the future.

DR. LANGMUIR: Thank you.

DR. NITAO: In the previous slide we had a critical flux which we called a U* and this new graph shows a way that a motivation for what that U* is.

We found from our analysis that the flow in the fracture, most likely the square root of what we call a fracture diffusivity times time, and this fracture we found was related to the inlet flux times the half fracture aperature, the perosisity of the matrix, one minus the initial saturation of the matrix and the matrix diffusivity constant.

The flow in the matrix goes as the square root of the matrix diffusivity times times time. If the diffusivity in the fracture is much greater than the diffusivity in the matrix, we'll have fracture-dominated flow. And vice versa, if it is much less, then we'll we have matrix dominated flow.

And basically by looking at these two expressions here, we obtained that our value of U* down here is this expression here. Next slide.

These are computer simulations which confirm this theory. Here we have inlet flux is equal to one-tenth the critical flux and we find that the fractures here, the matrix here, these are liquid saturation contours which are the contours of constant moisture that is being absorbed into the matrix. We find that the flow is basically symmetrical and it is mainly in the matrix, occurring mainly in the matrix.

At the equal to the critical flux we find that the fracture flow is starting to increase. And when the flux goes to 10 times the critical flux, we find that most of the flux is occurring in the fracture with the flux into the matrix occurring methodical to the fracture. And I notice that these are plotted at different times here because in order to show that this is a much larger time than this, which of course shows us that the fracture flow occurs much faster.

And we found mathematical approximate expressions for the movement of the flux in the fracture according to these expressions here. Next graph.

This is a similar viewgraph. Here we have the fracture is interfering with each other and here we have fractures of .2 meters apart from each other. We find similar other things, except that here because of the interference with the neighboring fracture, we find that for matrix-dominated flow the saturation contour lines are basically perpendicular, which means that we have equilibrium and continuing equilibrium between the matrix and the fracture. And at 10 times, we have a fracture flow occurring here. Next viewgraph.

Here we have a plot of the penetration of the water into the fracture versus dimensional time, dimensional penetration, and we found that for a fracture-dominated flow, the dependence, linear dependence, for small time is it looks like "t" to the one-half and goes both linearly when the fracture starts to interfere.

The time at which it interferes is equal to what we call lambda squared where the lambda is the initial unsaturated matrix volume to fracture volume. And we noticed that at three times the flow periods here and at Yucca Mountain we found that for a parameter's study, that it seems to occur mainly in flow reaching, too, so it looks like "t" to the half power. Next.

These flow periods can be entered physically as the first flow period dominated by inlet boundary conditions and the capillary condition in the fracture and gravity.

The flow period II is dominated by the matrix imbibition and gravity. Flow period III is when you start getting interference with the neighboring fractures. Next viewgraph.

Just as an application of our study, we looked at the problem where we have the borehole with the water going into a fracture on this idealized fracture here, and we calculated how far it would go down.

Using nominal Yucca Mountain parameters, after 48 hours it would go down 20 meters using this illustrated calculation, and it would go 6.5 centimeters roughly into the matrix, and it will perturb the environment by 20 percent.

What motivated me to look at this problem is we were looking at defective drilling water on the near field environment or on the waste package, so here we were looking at a borehole that was drilled with wet drilling and see what happened.

And for this other case here, we used 1/40th of the matrix diffusivity that is expected at Yucca Mountain. And we found after 48 hours, it would go 140 meters and go one centimeter into the rock and perturb the rock by four percent. Next.

Just to begin to conclude both Tom and my talk, this summarizes some of the work that we have been doing in the hydrology area.

In terms of code development and verification, we have developed and partially verified the V-TOUGH Code. And our nonisothermal modeling studies where we look at thermal perturbations, we've identified some of the key mechanisms of heat and flow fluid flow around the waste packages. And through the G-tunnel experiments, we've partially validated a model.

In fracture-matrix interaction, we have analyized the interaction of the fracture and matrix flow and then identified major flow regimes and developed the fracture and matrix flow criteria.

Pete will go into case specific scenario evaluation by using numerical analytical models which were validated through field and laboratory experiments in order to identify conditions for which liquid flow to the waste package is possible.

DR. NORTH: Before we go on to that, I would like to ask you a specific question in regard to specific scenario evaluation. Let's just take one. For example, Ghost Dance Fault. There is a large fracture with that B cubed relationship that was discussed earlier.

DR. NITAO: Yes.

DR. NORTH: Have you looked at that as a case? How much water could get down through Ghost Dance Fault into the repository and how does that fit into your modeling framework?

DR. NITAO: That is something that we would like to look at.

DR. NORTH: Have you done it?

DR. NITAO: No, we haven't done it yet.

DR. NORTH: Thank you.

DR. NITAO: Our nonisothermal environment will involve long-term field testing and determining more of these scaling laws for drying and condensed flux.

And by using some of these scaling laws in our testing, we hoped to develop integrated parameter models which would allow us to model larger systems than we are able to model now and develop dual porosity models, since the only other alternative to dual porosity models is to use discrete fracture modeling and that would require considerable computer costs.

In our fracture matrix flow modeling, this modeling work is relatively recent work that we've done, so we haven't been able to apply it to future problems yet. But we need to also develop an experimental confirmation of our theory before we feel confident in applying it. And we need to also extend the work from some of the idealized systems that we are looking at to multi- dimensional fractures and fracture systems and use this different mechanics to develop dual porosity network models.

DR. VERNIK: Are there any questions that need to be discussed? I suggest that we take a fifteen-minute break.

MR. WILDER: I wonder if we could follow up on the comments relative to the Ghost Dance Fault, and I think this is an example of the intergration that Carter was talking about.

The evaluation of the water being down at Ghost Dance Fault is really being done by U.S.G.S. but we are having to look at that ourselves to satisfy ourselves that in terms of the way it is impacting us, it is not going to have an impact.

One of the things that is specified in the site characterization plan is that we will not emplace waste in a known fault. So we won't be in the immediate vicinity of Ghost Dance Fault but we are certainly going to interact with the U.S.G.S. to understand that the water appraisals that they have for going down Ghost Dance Fault are not going to impact the waste package.

So there is a lot of intergration between us and the U.S.G.S. on that.

DR. NORTH: It seems to me that this is an issue that we should get on with. I found it very persuasive to go down an end tunnel a few months ago and watch what I was told to be the order of 10 gallons a minute coming out of a big crack.

How much of those are there in the proposed Yucca Mountain area? And how does this modeling study contribute to be an understanding of how we might go about looking for them? I mean, Ghost Dance Fault is one where we know there is a potential problem and then there may be some others that we haven't found yet.

I would like to see some case specific scenarios run potentially as a way of getting on with the performance assessment and finding out what kinds of things do we need to know.

For example, can we rule out that medium-sized fractures and small fractures are essentially unimportant? We had an extensive discussion with some people from the Bureau of Reclamation in December on the importance of small and medium-sized fractures in relation to tunnel-boring machines.

So I would like to see this exercise taken to some bottom lines where we can conclude what issues are going to be most important to performance assessment and which issues we can bound out because we have strong indications that they are not important.

MR. WILDER: Some of the work that Tom and John have been doing to address that issue is that they found that beyond certain aperature, it doesn't matter how much wider the aperature is. And we are trying to get a handle on that and when we get those result --

DR. NORTH: Can we get those results?

MR. WILDER: I think we can, and I think it is already contained in some of the literature that is available.

DR. BUSCHECK: In the fracture sensitivity study we considered, we considered a thousand micron fracture and we can even go larger than that. So, in fact, we do have some data which may contain more closer with Ghost Dance Fault but it would not require a large amount of work to address that directly.

DR. ISAACS: We'll take your comment under advice. My decision is that other parts of the program are probably more oriented to addressing that issue. Maybe what we ought to do is see if we want to put it on the agenda for the March meeting presentation.

DR. PRICE: Were you just requesting some information as an action item?

DR. NORTH: Let's make it a generic interest in seeing this kind of exercise go further, and I don't really feel it is appropriate to make it to these specific individuals, but rather the program as a whole.

DR. PRICE: I mean, there was an indication of some data in there and you said can we get that data. Did you want to make that particular request as an action item?

DR. NORTH: I think we'll leave it to DOE to interpret what it is that they can give us in this area.

DR. VERNIK: Why don't we reconvene in fifteen minutes.

(Whereupon, a recess was then taken.)

MR. WILDER: We would like to continue on. We might ask for a little bit of feedback from those in the back. One thing is that the laser pointer batteries wore out on us. We have new batteries in it, but we are also going to try maybe just a manual pointer, and so I would like to get some feedback later on today as to which one is working better.

We also moved the microphone over and because of using the manual pointer, there may be a problem with the microphone, so be sure to let us know if it is working okay.

We are now going to be moving onto the issue of composition of water and water vapor, essentially the geochemical interactions by Dr. Bill Glassley. As I mentioned earlier, he will be focusing on the rock water interactions. Bill.

DR. GLASSLEY: As Dale said, my name is Bill Glassley and I'm the task leader for waste package environment geochemistry and mineralogy.

The responsibility or effort that this task has is to define what the geochemical environment is within what waste packages will exist. In particular we are interested in generating information that will be used for the evaluation of candidate container materials. We are interested in generating information that will be used in establishing a source term, and finally we have to provide information that will satisfy regulations, particularly those contained in 10CFR 60.135a.

The organization of this presentation will be as follows. It will first describe very briefly the ambient conditions that we expect in the environment, emphasizing in particular water chemistry.

Following that, I'll describe the perturb conditions that we think we are going to have to deal with, concerned primarily with the types of processes.

And then I'll discuss some of the work that we are doing trying to evaluate the consequences of these processes for the waste package environment geochemistry. I'll then describe some of our modeling activities and summarize briefly what we have accomplished to date.

As far as the ambient conditions are concerned, most of them were described by Dale Wilder but to emphasize those things that are particularily important to us, the site is unsaturated and it has a range of possible saturations but the fact that it is unsaturated is critical to what we are doing.

We believe it is slightly oxidizing with an atmosphere that is dominated by air and we believe that the vadose water composition in the Topopah Springs tuff probably approaches that or probably approaches equilibrium with Topopah Springs tuff.

However, it has yet to be established whether or not the water composition or known water composition is, in fact, an equivalent with Topopah Springs tuff, and that is one of the issues that has to be dealt with in the future.

As far as water chemistry is concerned, we have to deal with two different issues: One is what the pore water chemistry will be in the immediate vicinity of waste packages. We need to understand what the chemistry of water could be that would contact waste containers. Those are not necessarily the same thing because of the possibility of fracture flows and high flux conditions.

Shown in this figure in this column is the composition or compositional ranges for particular elements in J-13 water. As you can see, the ranges, although limited are not for some elements, particularly small. There is quite some variation in some constituents.

However, it remains if you look carefully at the composition a very dilute solution. However, in comparison with other waters that have been obtained from the extraction processes, particularly that Al Yang has employed, for samples that have been obtained from shallow wells or relatively shallow wells in the vicinity of Yucca Mountain, those pore waters, extracted pore waters, have composition that overlap with to some extent J-13 but nevertheless can be significantly different.

If you look in particular at silica, chloride and sulfate, for example, there are wide ranges and very different compositions compared to J 13.

Our concern, therefore, is to establish how the chemical environment will behave for the entire possible range of conditions we could see out there. Next slide.

The approach that we've taken, therefore, and most of our efforts recently has been to describe or determine what the range of ground water composition is in the vicinity of Yucca Mountain and use those as bounding conditions for our experimental work and modeling activities.

As an example of what we've done, this figure which has as a vertical action oxidation state, horizontal access, the pH, we've examined or plotted on this figure all of the waters that have been obtained from all of the wells in the vicinity of Yucca Mountain.

They all fall within the shaded region. They are bounded by conditions in which the pH ranges from about 6.8 to about 10 and oxidation conditions of about minus 200 millivolt to about 400 millivolts.. That covers a very large range.

Most of the ground waters fall in the vicinity of the lower left-hand corner where the pHs are relatively neutral and the oxidation state is near neutral to slightly reducing, but they can be somewhat oxidizing as well.

DR. LANGMUIR: Bill, don't these waters basically contain measurable dissolved oxygen?

DR. GLASSLEY: Yes. And the problem is what the significance of that measured oxygen is. As you know, measuring Eh state of any ground water is a very, very difficult problem. What we've done is simply taken the measured potentials that have been reported in the literature and plotted them here.

DR. LANGMUIR: These are measured values rather than theoretical values?

DR. GLASSLEY: That is right. These are all measured values.

DR. LANGMUIR: So when you start comparing them to theoretical diagrams, they may or may not be meaningful?

DR. GLASSLEY: That is correct. This is the approach we are taking in establishing what the ambient water temperature used in the environment is.

Now what I want to talk about is the work we are doing to look at the conditions that could pertain to once the system has been perturbed. There are a variety of things that we want to deal with here. Next slide.

One of the problems that we must address is the behavior of radionuclides at elevated temperature for that scenario in which water enters a container, dissolves the materials, leaves the container and energy environment.

The strategy that we have established to undertake this work involves three points. First, we need to establish what the near-field flow and transport properties are. We need to identify what the solution composition would be that would enter a container and what its composition would be as it leaves. And that includes considerations of interaction with that exiting water with corrosion products that could be present along the can or along the borehole wall.

We also need to establish what the interaction would be of the solution once it gets into the environment where rock is present. What kind of absoption processes would occur, what kind of diffusion and mineral exchange processes could take place, and what the transport properties would be of that essentially contaminated water.

To accomplish this work, there are a couple of areas of work that we need to undertake. One, we need to generate or obtain results of waste form water interaction. We need to know how water interacts with the waste form that would be present in a container.

That work is in progress. It will not be described in this particular meeting but it essentially is the technical area involved in waste form testing.

We also need to understand sorption and transport characteristics of the near field and put that information into computer codes that will be used to make our long-term predictions and calculations, and that work has been in progress. Once we have information that is adequate to conduct modeling and to guide experimental activities, those activities will be undertaken to look at radionuclide behavior.

DR. LANGMUIR: Bill, are you going to be talking about the codes in which you put these parameters?

DR. GLASSLEY: Most of the work of modeling and numerical simulations we will be doing at least at this point will probably employ EQ3-6 which will have in it the capability to incorporate absorption processes, ion exchange, solid pollution and exchange processes that would occur in the presence of the radionuclides.

DR. LANGMUIR: Are you talking about an updated

EQ3-6?

DR. GLASSLEY: Yes.

DR. LANGMUIR: That would handle triple layer and constant capacity models and that sort of thing?

DR. GLASSLEY: That is certainly our consideration now.

DR. LANGMUIR: What is the time scale on those possibilities now?

DR. GLASSLEY: That depends on budgeting.

DR. LANGMUIR: How far along are they now?

DR. GLASSLEY: The solution exchange models are in progress now and are being developed. That is well on the way to being incorporated into the code. Adsorption processes are being delayed right now. We expect to use some of the information generated by Los Alamos in their work in incorporating that into the code, but that has been delayed.

DR. LANGMUIR: Have they looked at just simply taking the very effective working portions of MINTEQ which can do sorption models and form them into EQ3-6?

DR. GLASSLEY: There are a variety of points that are being considered. No decision has been made yet about which approach would be the one to incorporate into the code at this point.

The conditions that we have to be concerned with in the environment, once a waste package is emplaced, involve a variety of things. But to summarize them briefly, the environment is going to experience a thermal peak that could be as high as 240 degrees centigrade in the immediate vicinity of the waste package. That thermal peak will probably occur within 40 years of emplacement and that will be followed by an extensive period of cooldown which could go on for thousands of years.

We know that vaporization of water will occur within the immediate vicinity of the waste package and a saturation "halo" or something similar to that will develop at some distance from the borehole wall. These are considerations that Dale discussed earlier.

Eventually the possibilities exist that that saturation "halo" will migrate back toward the container as cooldown occurs.

We know that reaction products as a result of radiolysis will interact with the rock and will be in the independent vicinity of waste packages, and we need to understand what those radiolysis products are, how they will interact, what their chemical products will be in the environment.

And finally, we have to be concerned with the presence of man-made materials. There are a variety of things that could be emplaced in the drifts or will be emplaced probably and things that could be left behind including human biological waste.

All of these need to be incorporated into our considerations of establishing for various scenarios what the chemistry of the environment will be.

To conduct the long-term modeling when considering all of these attributes, there are a variety of assumptions that we have to make. First of all, we know we are dealing with a system that although in human terms is going to be long-lived, in fact, geologically is a very short time period.

We must assume that although equilibrium may be approached, it probably will not be attained. We,

therefore, have to be concerned with the kinetics of the reaction in the environment.

The thermal stability of minerals is extremely important and the range of temperatures that can exist in the environment obviously will be broad. We, therefore, need to understand what the thermal stability of minerals, particularily the secondary minerals, will be in the environment and we need to understand the effects of solid solution on that stability of a particular mineral we are concerned with.

Finally, we must assume that there will be some interaction of pore water with man-made materials. It is difficult to imagine scenarios where that will not occur. We, therefore, have to have sufficient information to incorporate those effects in our modeling capability.

Next slide.

Now what I want to do is talk about the work that we are doing in establishing, first, the reaction kinetics and our effort in determining essentially rates at which equilibrium will be approached. And then I'll talk about the numerical simulations that we have been doing in this area.

In this figure, I've taken examples of rock water interaction experiments that have been conducted by Kevin Knauss & Associates. They have been looking at the

interaction of water with pieces of Topopah Springs tuff, either wafers or crushed fragments of rock. They have been emplaced in the action vessel and cooked up in this case to 150 degrees centigrade and through time, samples of the solution have been extracted and this provides us with a means of understanding reaction progress.

The top four figures are on the vertical access concentration of the indicated element. The horizontal access on all figures is time and the bottom figures represents pH.

What is important here is to recognize a couple of things. First, in all cases some steady state is certainly approached in the reaction progress, but the steady state is not always the same. There may be substantial scatter. Particularily if you look at sodium, you can see substantial variation from one experiment to the other.

In conducting model simulations and numerical simulations of these models, it is imperative that we can in most cases reproduce what we see in these experiments but that is not true for all cases. There are variations that occur, reflecting the fact that starting materials are not always the same and it is not possible to completely characterize everything that is there.

The second thing is that reaction kinetics is

important and there has been a paucity of data on the ways in which it can be represented which are ultimately the controls on reaction progress.

So we've established an experimental and laboratory effort to define this thermal stability of mineral phases as a function of chemistry of the environment and a program to look at the dissolution of particular mineral phases. And I want to briefly go through what that work has shown us.

As far as the thermal stability of minerals is concerned, as I've mentioned, we are dealing with a wide range of temperatures, as because of these values, we need to establish composition isotherms and internally consistent highly accurate thermodynamic properties and mineral phases that we are concerned with.

We've then attempted to or plan to compare the results of laboratory experiments at variable temperature and water chemistry with our numerical simulations to see the extent to which those results similar.

Once we've done that, it is possible to define stability fields of the particular mineral phase that we are concerned with as a function of the temperature and composition.

DR. LANGMUIR: Bill?

DR. GLASSLEY: Yes.

DR. LANGMUIR: How about thermal gradient studies where you are looking at changes of thermal composition? Are there some studies published in that area? Are you going to be dealing with that sort of thing?

DR. GLASSLEY: You mean thermal gradient across a mineral grain?

DR. LANGMUIR: Across a meter or two or three, that sort of thing, as you would expect near a catalyst.

DR. GLASSLEY: One of the things that we

ultimately expect to be able to do is describe how the chemistry of the water will change as it is moving to the thermal field. And, in fact, one of the modeling activities that is taking place right now is a combined transport chemistry, a combined hydrology-chemistry code which will look at the transport of water across the thermal gradient and establish what the actual thermal gradient will be in that kind of environment.

Does that answer your question?

DR. LANGMUIR: Yes.

DR. GLASSLEY: I want to show you a particular example to show you the direction our work has gone. Next slide.

This is a figure that shows the stability fields of a wide range of minerals in the system, CaO-Al203-SiO2-H20 at 100 degrees centigrade as a function of two different composition parameters. The log activity of silicon is on the bottom of the figure and the log activity of calcium is over the activity square of hydrogen on the vertical access.

Originally when this figure was done -- well, I should describe first some of the important points of this figure. That line separates the phase fields. The dashed line labeled "quartz" shows where quartz is stable. To the left of that line "quartz," the solution would be under saturated in quartz. To the right of that line, the solution is super saturated into quartz.

As an example of a problem that we ran into when we were conducting our initial modeling activities, Clinoptilolite, which is indicated by the field indicated as C-l-i-n-o-p, overlapped with the quartz line. Clinoptilolite and quartz have never been reported in nature before. Therefore, clearly, there had to be some descrepency between the thermodynamic properties of clinophyllite and the way clinophyllite actually occurs in nature.

We undertook a very extensive reevaluation of the thermodynamic properties of clinophyllite. We were able to identify the particular problem in the original work that came up with the thermodynamic properties of that phase. We determined those thermodynamic properties of

that phase. We generated a figure and the figure now is consistent with what is seen in nature.

Clinoptilolite does not exist with quartz. It does exist with mordenite and cristobalite. And both of those boundaries, although not plotted there, pass right through the clinoptilolite field.

That is the kind of work we are trying to do, trying to establish what the thermodynamic properties are and thermostability is of the various mineral phases.

Next slide, please.

As I mentioned before, reaction kinetics are extremely important in determining the rate at which a system is going to approach equilibrium. We've undertaken an experimental program to determine both dissolution kinetics and precipitation kinetics of the phases that are of concern to us.

The ones we have looked at so far for dissolution are indicated as well as precipitation kinetics. The ones indicated by astrix are phases that occur in the rock as it exists now and they are also phases that can occur as secondary reaction products.

The conditions that we have been looking at in determining dissolution precipitation processes and kinetics, the pH range is 2-12 and the temperature is 25 to 240 degrees centigrade.

I want to show you an example of the kind of results that we've obtained and describe very briefly the significance of those results. Next slide.

This is a figure that shows the dissolution rate of quartz indicated by the solid symbols and the rate at which silica is removed from heulandite on the open symbols. Vertical access in log rate in moles per square centimeter per second and the horizontal access is pH. These experiments were conducted at 25 degrees C.

What is important in this figure is that the dissolution rate of quartz and the rate of selica removal for heulandite at high pH's are virtually identical. If one were going to take modeling, using those two phases and try to come up with the chemistry, which solution rate one used or dissolution rate one used wouldn't make much difference in the results.

However, at low pH's the difference in dissolution rate is substantial. In fact, the difference between quartz and heulandite at a pH of 2 is more than 10 orders of magnitude. Any modeling activity that would be undertaken at low pH's that did not take this kind of difference into account would be in substantial error.

We are now trying to incorporate this kind of information in our modeling activities. However, the amount of dissolution data under these kinds of conditions

is very limited. Much work needs to be done to establish this. We are in the preliminary stages. We are in the preliminary stages of incorporating it into our models.

What I want to talk now briefly about is what our bodily efforts have been up to this point but which have not taken into account the kind of dissolution behavior that we've seen so far, at least not quantitatively.

Going back to this figure of the range of water chemistry, what we've done is to try to compute how water chemistry will change as a function of temperature when it interacts with the rock for a wide range of conditions in order to bound what we believe will be the chemistry of water that could interact with the waste container.

What I'm going to show you are results of how pH will change during heating of water that comes from the lower left-hand corner, in other words, moderately or mildly reducing near neutral water and modeling of water that has a moderately high oxidation potential plus 400 millivolts and a high pH of 10.

Compare how the pH's of those solutions will change during the course of reaction progress. In conducting these simulations, we've used the code EQ3-6. What is shown here is how the mineral assemblages change during reaction progress. We are taking -- just to repeat -- water at 25 degrees, heating it up to 90 degrees and

reacting it with tuff in the process.

For the mildly-reducing near neutral solution, well, the horizontal access here is the log of the reaction progress. And as time proceeds from initial reaction on the left-hand side of the figure to reaction completion on the right-hand side, you see the sequence of minerals in that form.

The minerals indicated in bold writing are those minerals that are stable at the end of the reaction process.

What is important to note here is that the minerals that occur during the early stages of reaction into two different waters are very different. But by the time one reaches reaction completion, the minerals are virtually identical. What you are seeing is believing, the fact that the rock is dominated by the nature of the chemistry of the system.

Look at the next slide, please.

You see how the water responses changes in minerology. For both solutions, the pH 7 and pH 10 solution, the pH's remain about where they started out during the early phases of the reaction progress; but by the time equilibrium is obtained, the solution is virtually the same pH.

J-13 water, for comparison, is shown here. You can

see it essentially falls within the bound except for this one data point here during the modeling progress and eventually reaches the same state.

What that emphasizes is at least if the system obtains equilibrium, the rock is going to dominate the chemistry of the solution and we can obtain probably pretty good, very near bounds on what the water chemistry will be. But for those systems which have not obtained equilibrium which are still undergoing reaction progress, there is a wide range of possible conditions that the waste container will experience if water contacts it, and it is that range of conditions that we need to establish and are in the process of establishing.

DR. LANGMUIR: Bill, you need to do it one more time. My comment earlier was that you were plotting measured Eh's and using those measured Eh's which are not at equilibrium.

DR. GLASSLEY: That is right.

Along with some thermodynamic data values here, the minerals and the equilibrium Eh would really be on that water boundary with oxygen present at the top of your figure. So one of your bounding conditions for calculation should be Eh-pH condition at the water boundary with oxygen. Even though it is not measured, it is the theoretical value.

DR. GLASSLEY: Absolutely.

DR. LANGSMUIR: And that should be one of your boundary calculations.

DR. GLASSLEY: I don't disagree with you. All I'm presenting here is a range of computations that we've conducted. The one you are talking about is one that has been conducted. In fact, as I remember, it certainly is included in the matrix of conditions that we need to define and we are in the process of doing that.

The problem is, and it is a problem that is going to exist probably as long as this project is operating. What is, in fact, the Eh-pH of the water that is out there. And what we have to do, as I mentioned before, is simply define the possible ranges both reducing and oxydizing, and let those be the bounding conditions. And define those in such a way that we can be certain that whatever the water will be that will contact the container will be within that range of bounding conditions. That way, we will at least have a good characterization of the pH conditions in the environment.

DR. LANGMUIR: Chances are with this unsaturated condition, that as long as you can measure oxygen, your pH is probably up at the top of that the whole time.

DR. GLASSLEY: Absolutely.

DR. LANGMUIR: So that really is it.

DR. GLASSLEY: That is probably true but we can't prove it, which is the reason why we have to establish bounds rather than take a single value. Next.

In summary, the work that we have been doing has emphasized reaction rate work, trying to establish the kinetics of reactions that can occur, emphasizing water-rock interaction and dissolution precipitation processes.

We are working on generating sufficient information so that we can define precisely the thermal stability of minerals, taking into account taking both the thermodynamic properties of the minerals and the solid solution behavior. We need to characterize the effect of man-made materials and how those affect the environment and that work is planned.

Modeling is proceeding as data is available to us and data are available to us. But one of the things that will be a very important activity in the future is validating those model calulations and those numerical simulations.

Much of the work in the next years will involve developing strategies and carrying out those strategies for validating our numerical simulations for the long term behavior to the environment.

Any questions?

DR. LANGMUIR: One last chemical-type question. As you are aware, there have been a number of papers published in the last five years or so which have looked at the effect of thermal gradients around each source on the geochemical process and hydrology. And one of the most important aspects of what you are getting into or what the program is looking at or should be looking at certainly is to what extent do these reactions alter hydrological properties of the adjacent blocks.

DR. GLASSLEY: Absolutely.

DR. LANGMUIR: And in many cases, of course, they've shown that they seal up the perosisity in the source rock and that sort of thing. This is a very important interplay between the geochemistry and hydrology that should be addressed and I'm wondering if your group is doing this or what group in the program is doing this kind of work.

DR. GLASSLEY: We realize that probably the most important part of conducting numerical simulations for this environment will involve a couple flow and transport code.

There is in existence now a modified version of EQ3-6 looking at flow regimes, and they try to establish how rock fluid interaction will change the hydrological properties of the flow pathways.

Mineral precipitation will influence fracture roughness. It will change fracture aperature and will change pore sizes and influence all of these things. The code is in the early stages of accomplishing this.

Ultimately, what we expect to do and what our plan is to intergrate the EQ3-6 kind of approach with the hydrological kinds of computations that Tom and John have been doing in a much larger code package that will provide the kind of information that we talked about: How hydrological properties will change, what the mineralogical characteristics of those change regimes are, and how that influences those temperatures.

That is the ultimate goal, as you know, of this technical area.

DR. LANGMUIR: How about some experiments in which you are looking at unsaturated movement of water, condensation internal thermal grading? These can be easily done, and more what you are trying to address.

DR. GLASSLEY: That is correct, and some of those activities have been carried out. The Lin-Daily experiments, that I'm not sure that you are familiar with, Wunan, Lin and Daily have been conducting flow-through experiments using pieces of tuffs, thermogradient fracture systems, unfractured rock samples, looking at how those hydrological properties change with time.

One of the things that they've documented is precisely what you are talking about, the sealing up of those materials as reaction takes place. It occurs in fracture samples. It occurs in thermogradients, nonthermalgradients or isothermal systems and a wide range of things.

That is an area that we are looking at very actively and we have an experimental program that addresses that. It also is tied to the hydrological work that Tom and John are doing. And many of the experiments that Wunan, Lin & Daily have done have been guided by the numerical simulations that John and Tom have carried out.

So there is a very strong coupling there and much communication in establishing what the behavior of that kind of system is.

DR. NORTH: Could you tell us a little bit more about the effects of man-made materials? What it is that you are worried about? What investigations do you plan?

There are several things that you listed, and I'm wondering if you could translate these into, for example, the Eh-pH regime. What kind of excursions might be possible given that some things were done in the course of repository operations like leaving some human garbage around. Is that a serious problem and what might be the implications to the overall program in terms of dealing with it?

DR. GLASSLEY: We think that potentially it is a very serious problem. We know that based on experiments that have been conducted so far, it generates waters that have very high pHs of 9 and a half or greater. In fact, the lowest pH of water coming out of cement has a pH of 9 and a half and pH's of 12 are not uncommon. That kind of pH can have a dramatic effect on the performance of the container material.

It can also have a very important effect on how fuel, spent fuel or rock waste forms, dissolve. We are planning an activity or a task area that looks at nothing but the kinds of man-made materials that will exist in that environment to establish what the interactions will be.

The approach that we plan on taking is first going through the lists of materials that have been developed for what will be in the repository, establish on the basis of some reconnaissance work and literature surveys which of those materials probably would be the most reactive. Then establish a laboratory program and numerical simulations program to determine the effects of those materials on water chemistry.

Then when that is done, try to conduct some validation exercises and numerical simulations to see what

the long-term consequence of having that material in the repository would be and how it would interact with the rock that would be in there.

Most of the work that we've done so far is collecting information. We've been in contact with a variety of laboratories that are doing work primarily in concrete because that is where most of the information is going to lead. There is a lot of work yet to be done in human biological waste, volume, composition, impacts on water chemistry. A lot of work needs to be done on paints, solvents of any kind, drilling fluids that could be used. It would be a very extensive program that would require many years of long-term experiments.

DR. NORTH: It is the many years of long-term experiments that concerns me. What I would love to see at this point, and we'll make this a specific data request, is one, a plan for carrying out the investigation with respect to these man-made materials or human biological wastes. And instead of back-of-the-envelope calculations, the effect of where should we work?

You've mentioned the Portland cement issue, human biological waste issue. You didn't go into that. What are the implications? Is it possible to fix this rather easily, like we require everybody in the repository to use port-a-potties, or not throw their lunch somewhere or are

we talking about having to design a new kind of cement that doesn't exist?

DR. GLASSLEY: Well, the task that I was talking about, man-made materials task, would have as its primary goal establishing precisely the kind of guidelines that you are talking about.

DR. NORTH: I'm worried about can we have those insights in the next six months or so as opposed to ten years after the research is finished? It seems to me the that the implications for the program are sufficiently important so that it would be hopeful to get a first cut at these issues very soon.

DR. GLASSLEY: I would agree with you.

DR. NORTH: Second question, this relates to a paper of yours dated October 1, 1986 which I had as part of my reading before coming into this meeting.

DR. GLASSLEY: Could you tell me the title of that?

DR. NORTH: It is entitled "Reference Waste Package Environment Report," and I'm looking at it in the discussion of thermal effects on rock physical properties. And it discusses cristobalite and a particular phase transition from alpha to beta, and notes this phase transition results in a volume increase of about five percent, referencing Helgeson, et al, 1978.

It then goes on to conclude at the end of this discussion and I quote:

> "The alpha to beta cristobalite transition temperature falls within the temperature range expected for the near-field waste package environment during the period immediately following emplacement. The

effect of the associated volume change on the waste package environment has yet to be established."

I'm particularly interested in the last sentence. What research has been done on the issue of the associated volume change on the waste package environment? And if we haven't made any progress since 1986, why haven't we?

DR. GLASSLEY: Two responses to that. First, the work has continued and there was a paper that occurred at the most recent MRS meeting that dealt with some of the work that has been done by Dr. Annemarie Meike and myself looking at the phase transition alpha to beta cristobalite in the presence of vapor phase, It was a HVEM study, trying to understand how that transition takes place and what it really means.

There is also work that is being planned in Steve Blair's task, which he'll describe later on today, that concerns how volume changes really influence the development of cracked growth, possibly the formation of microcracks.

Ultimately those may have an impact on water chemistry and water flow pathways because it could change the permability pathways of matrix.

Those are all concerns that still exist. Work is in progress to evaluate those particularly in Steve's area. It is not something that has been terminated by any means.

DR. NORTH: How about the physical and structural implications of five-percent volume increase from this mineral? Are we talking about spalling or are we talking about mechanical loads on the containers potentially?

DR. GLASSLEY: Those are precisely the considerations that Steve is involved in dealing with.

DR. NORTH: Good. So he is going to answer those questions?

(Laughter)

DR. GLASSLEY: Any other questions?

DR. CARTER: Yes. I have a couple. One, you mentioned one of your most important task functions is to generate source times. How far along has that process gotten?

Have you taken a worse case, a release of all the radionuclides that might be there over short periods of

time? Have you ranked the radionuclides? Is there some things that you are most concerned about?

DR. GLASSLEY: There is a technical area, waste form performance, waste form behavior that is currently headed up by Ray Stout that has been looking at the interaction of waste forms with various waters under a wide range of conditions to establish what kinds of things could come out. There is a vast literature that they've generated. The work is extensive and that work is continuing.

What we need to know to establish the behavior of those materials once they get out of the container is how those materials interact with corrosion products because those solutions will be in contact with corrosion products before they get to the packaged environment.

That information has yet to be generated and it is something that is expected to be available to us but it is not available yet. Once that is there, then we can do the kinds of studies necessary to determine the behavior of those elements once they are in the waste package environment.

So at this point, the basic information is being generated by the waste form materials people determining what the water chemistry would be in the waste container. The next step is to determine what the water chemistry will be as it leaves the container and then we'll take it and play with it in the waste package environment.

MR. CARTER: You are eventually going to get in the loop but you are not there yet?

DR. GLASSLEY: Yes.

DR. CARTER: What about considerations given to the differences between high level waste in a repository, used fuel elements in the repository, and any particular mixes of those two waste forms?

DR. GLASSLEY: Mixed waste?

DR. CARTER: Mixes between high level waste and used fuel elements. I presume the source terms are different for those two waste forms.

DR. GLASSLEY: What we'll do and our consideration is to take the spectrum of solutions that the people involved in the waste form interactions technical area generate and deal with each of those in our evaluation of how they behave in the package environment.

We don't expect to have just one solution of chemistry or one type of solution that we are going to deal with. We have to cover the entire spectrum. So anything that we'll deal with, I mean, it would cover the range of things that you are talking about.

DR. CARTER: I think that the only thing that we've heard today so far, if I'm not mistaken, has dealt

with used fuel elements and not with high level waste per se.

DR. GLASSLEY: We are dealing with spent fuel, processed fuel, glass.

DR. CARTER: Well, when I say used fuel elements I'm talking about spent fuel but not processed fuel in the alloy.

DR. GLASSLEY: You mean glass?

DR. CARTER: Yes.

DR. GLASSLEY: We fully expect to deal with that as well. We are by no means ignoring that. It makes a significant compliment with the material in the repository. It is something that we have to deal with. And everything that we have been doing has been done under the assumption that we are going to have to deal both with the glass waste form and spent fuel.

There has been no distinction made between the necessity of dealing with either of those.

DR. CARTER: But at the moment you are dealing with essentially spent fuel?

DR. GLASSLEY: No.

DR. CARTER: And your temperature generations and so forth are related to that. Is there a possibility that you could have mixes of these materials as far as waste forms?

DR. GLASSLEY: Sure. We've said that.

DR. CARTER: And this would affect all of the things, I think, that you are going to be interested in studying.

DR. GLASSLEY: Absolutely. It will affect both the chemistry and solutions and the temperature that will exist around the waste packages. All of those issues are part of the efforts that Livermore is undertaking. And although we've talked primarily about extreme cases, particularily high temperatures, the 240 degrees, that essentially is the maximum.

There is going to be, because of this mixed high level waste issue, a variety of temperature conditions that we have to consider, including temperature conditions around processed waste forms where the heat will probably be much less intense and the radiation field will be much less intense than it would be around spent fuel.

All of those are considered in the work that we are doing. The presentations may have emphasized spent fuel, but that doesn't mean it was not meant to imply that there was no work being done in glass work form at all.

DR. CARTER: I quess a related question in terms of potential release of radionuclides, are you more concerned with spent fuel or high level waste? Let me ask the question that way.

DR. GLASSLEY: I think for most of the radionuclides of concern, the main source for them will be spent fuel.

DR. CARTER: But not to the exclusion of the glass?

DR. GLASSLEY: Absolutely not, no.

DR. JARDINE: Dr. Carter, I might mention that we have a whole other technical area that is equal or in big as size as the near-field environment that deals specifically with waste form testing, and there is an extensive series of activities, including other national laboratories, doing glass waste form testing. It is not something that we've put together today but it could certainly be done in the future.

DR. CARTER: Well, what flagged my interest was the fact that he put up the things that he is particularly interested in as far as important task functions. And if I'm not mistaken, one of them was generation of task forms and the other was satisfying regulatory requirements. So that is the reason for the question.

DR. GLASSLEY: And I would respond that we are dealing with all of the waste that could exist in the repository.

DR. CARTER: I quess in a way everybody is dealing with those particular questions.

DR. GLASSLEY: Yes.

DR. CARTER: But you are not prepared to discuss those?

DR. GLASSLEY: No.

DR. ISAACS: There is still an issue out of what will go into the respository. It is a consideration of greater than Class C waste, for example, that we haven't talked about. And the important terms there would be to find out the kinds of issues we might have to consider. There would be yet another challenge should we have to start considering accepting that kind of waste of repositories.

DR. GLASSLEY: If that becomes present in the repositories, then the man-made materials task is greatly expanded because chemical interactions with those other materials would have to be considered as well.

MR. WILDER: We are now going to move onto the mechanical loading conditions on the container materials. And the areas that we are going to try to focus on a little bit are the impacts on the hydrology and specifically looking at what some of the impacts could be on the air gap.

Also, looking at geochemistry impacts, and specifically Steve will talk a little bit about some of the mechanisms whereby increased surface area might be available for rock-water interaction. And finally, the loading conditions including the block failures of sand and creep. Now I'll turn the mike over at this time to Steve Blair.

MR. BLAIR: Good morning. Today I want to talk to you about work that we have planned to do on the mechanical attributes, to study the mechanical attributes of the waste package environment.

I want to mention that this work is in the planning phase. We have not completed any tests to date. We've completed a study plan and that study plan has gone to DOE for review and it is presently back at headquarters. We expect to be getting the viewed copies and comments back and we'll incorporate that and resubmit the study plan.

In this talk, I'll first review some of the effects, the thermomechanical effects of the waste package emplacement, and then I'll discuss specific aspects of elements in our study plan. In particular, I want to look at borehole stability, modeling and validation, and then I'll give a brief summary.

I will not be discussing tests to look at the rock properties or fracture properties. Thermomechanical effects of the waste package emplacement -- first of all, I want to mention that just the excavation of the borehole for where the package will go will increase the stress

along the borehole. Increasing the temperature, increasing of the stress, the moisture, the strength, creep rate, and as discussed earlier, may cause phase transformations in the formation. Decreasing the temperature during the cooldown phase will decrease the stress and also change the moisture distribution.

As far as stress around the borehole here, I've shown schematically what stress would look like in the uniaxial stress field. Actually, the formation would be more complicated than this but this slide illustrates when you put a hole into a rock mass or any infinite medium, the stress concentrations are on the borehole.

So you can see that within a few borehole radii, right next to the borehole, the stress will increase dramatically within a few borehole radii it bores back down to the ambient levels.

Here I'm using the same slide that Dale shows, as far as the temperature is along the borehole, and plotted temperature boreholes calculated for pressurized water reactor. What I want to point out on this slide is that very near the borehole, the same area where we have high stress concentrations, we also have high temperatures for a long time.

That is in the areas of one borehole diameter, so we can have temperatures over 100 degrees C for

considerable length of time up to a thousand years.

Here we show schematically how the moisture would be distributed around the borehole, and this is just depicting some of the results from G-tunnel which Abe will discuss in more detail in the following talk.

We see that the moisture is, first of all, around the borehole. There is a dry zone. There would be a saturation "halo", and then saturation back to ambient conditions as we move away from the borehole.

Now, the majority of this talk is going to discuss some issues that we think are appropriate as far as boreholes stability. Over the time periods of the repository, mechanism such as subcritical crack growth or creep may affect the borehole wall. Our purpose is to increase confidence in the estimates of the amount and type of mechanical loading on the waste package by looking at these phenomena.

There are several mechanisms that could affect the borehole wall. In the study plan, we specifically address three of those and in this talk I'm going to concentrate on spalling and block movement. Spalling would have the effect of wicking water onto the container in loading the container. Block movement would also wick water onto the container and cause point loading on the container.

I'll not talk about creep today but if the

formation were to increase, it would reduce the air gap and also bring loads onto the container.

As far as spalling, the rate of subcritical crack growth increases with increasing temperature and stress in rock. Spalling may occur due to the growth of cracks near the borehole by this mechanism.

Over long times that we have available in the repository, this could become significant. We plan to study this using laboratory studies, a series of lab tests and then through some numerical models.

I want to note that the work to date in both G-tunnel and Climax has not shown evidence of this effect in other short-term tests, but we feel that this mechanism should be examined.

Just to orient you to the slide that follows this, I want to show schematically the log of crack velocity versus stress intensity. This is for rock. And we see that as we increase temperature and increase the vapor pressure, that the crack velocity increases at a given stress intensity.

Now here we are looking at some data for granite and what we see are data for cracked velocity at 20 degrees C, 100 degrees C, 200 degrees C and then 300 degrees C. And what we see then is that as you increase to 100 degrees C, you actually may get a decrease in velocity. But then as you go above 100 degrees C, the subcritical factor increases. We also see that as you increase the relative humidity or the vapor pressure, that the crack velocity also increases.

Now, for the repository conditions, we expect to have stress intensities down in this range. And also I want to point out that even at a crack growth rate as slow as 10 to the minus 9 meters per second, that would give us a growth rate of one meter in 30 years. We don't need to grow the cracks a meter long, but we have plenty of time to grow cracks even small distances. Next slide, please.

As far as mechanisms that may be responsible for some of these phenomena, some work has been done on the subcritical crack growth in glass and we see that at the cracked tip, this represents a cracked tip on a molecular scale. Due to the size of the water molecules, you get down to the cracked tip and form bonds.

These bonds are going to be the stress bonds at the cracked tip. Water molecules will come in, form or disturb this bond at the cracked tip and allowing the crack to move as it breaks a hydrogen-oxygen bond as opposed to a oxygen silica bond. So this is just a possible mechanism that we plan to investigate into the next study plan. Next slide.

The water will cause this where some other fluids may not, due to the size of the water molecules. If you can control water molecules to make it down to the stress bonds at the cracked tip, where larger molecules such as Methanol or Analine can't get down to this stress bond and then cause the crack to grow.

Now as far as how you apply this to the borehole, what I show here is some results from some experiments that were performed at Berkeley on mechanism of failure around the borehole. And these were in hollow cylinder tests.

What we see are the initial stages of failure, small cracks growing eneschelon and parallel to the borehole wall, parallel to the tangiential stress. We see these stresses here and another here. And at later stages of failure, given on the next slide, we see that these cracks are then coalesced to form a piece of borehole wall that then spalls into the borehole.

Now, the idea of this study, the aspect of this particular study plan, then, would be to investigate this phenomena in tuff. First we look at the relation of cracked velocity, density, temperature and the potential of water vapor pressure. We look at the stress along the borehole and look at the mechanism by which boreholes actually fail in tuff.

This work has been done on limestone and there has

also been some similar work done in sandstone. Work of this kind today has not been done in tuff, so we don't really know what the mechanism failures around the borehole are.

We also planned some tests to look at block stability around the borehole. These blocks would be formed by sets of fractures that would intersect and form blocks that could move into the borehole at long times.

First thing we would do is, using the

distribution of fractures, identify the blocks and shapes. Then we would do a kinematic stability analysis to identify key blocks that may move in to conduct stability analysis for selected key blocks.

Now, this type of analysis is well established and we would use existing codes, and we wouldn't initiate this particular work until the ESF becomes available.

As far as modeling and validation of this work, we feel that we need to develop models for time dependence, temperature dependence, also looking at the effects of radiation. And then as I discussed before, the effect of mineral phase tranformations.

Cristobalite occurs in very small grain size in the ground mass of the Topopah Springs tuff. Now at the temperatures of the repository, it may undergo this phase transformation at the tuff or microcracks under the conditions then of the high stress and increased temperature flow. Not high stress, but the elevated stress condition and increased temperatures, these microcracks may then grow and lead to changes in the physical and mechanical properties.

As far as once we develop these conceptual models, we'll evaluate the existing codes and incorporate those potential models into FEFFLAP or SANCHO. HEFF is another code that is available. The Block Stability code that is available now is called BSTAB3D.

We also plan on a series of larger laboratory tests. As far as the modeling validation, we've tried to simulate these laboratory tests as part of the modeling validation.

In summary, the purpose of this task is to characterize the mechanical behavior of the waste package environment. We are looking at time dependent and temperature-dependent mechanisms and the work is presently in the planning phase.

Any questions?

DR. NORTH: Do you want to expand on your comments with regard to Cristobalite as to how serious a problem it might be? Is there anything further beyond what was stated on this problem in 1986 in terms of our understanding the implications for the program?

MR. BLAIR: There have been some thermal

measurements done and they show that Cristobalite might not just be the only culprit mineral here. There actually are a couple of other minerals here that give us significant thorough disadvantage. As far as what that will do, as far as the general stress levels and localized stress levels, the calculations have not been made.

We looked at some SEM as far as identifying how much Cristobalite there is and where it is located but we haven't really gone beyond that as far as this.

DR. NORTH: Do we have any sense of how much this problem is reduced if we go with lower levels of thermal loading from the fuel, with taking fuel that has been aged for a longer period as opposed to the reference design?

MR. BLAIR: The work to date shows that thermal expansion is actually significant at temperatures just above 100 degrees C. It starts to go up to 200 degrees C. It is fairly significant.

So if you are looking at waste much below 100 degrees C, then it would probably not be a problem. But at temperatures much above that, I think we would have to look at it.

DR. NORTH: There are no insights at the level that looks like you have a potential for serious microcracks and spalling block formation, et cetera, when you get up above 200, but at 150 it should be much less.

MR. BLAIR: I think it would be much less at 150. I think at 225 we can certainly say that we would probably have the problem. We don't have a lot of hard data to back that up.

DR. NORTH: How long is it going to take us to get that data?

MR. BLAIR: I think if we can get the rock and get the study plan through, then we can start looking at some of the effects of this, as far as the cracked growth rate, as a function of fractured toughness or as a function of stress intensity.

Hopefully, we could start those tests in another year, but right now we are in the paperwork phase of the study as far as getting it moving.

DR. NORTH: Given what you've described, I would like to put a request to the Program. Suppose there is a problem here. Suppose there is a potential for considerable degree of fracturing in the replacement boreholes? What do you do about it? What is the contingency plan?

MR. WILDER: Can I make a comment to follow up? First off, there are some engineering design things which are under consideration. For instance, fully line the replacement hole to prevent the spalling into the borehole would be one approach.

The other thing is that we've made some progress in terms of understanding the physical process, the geochemistry kinds of things, minerology kinds of things. And there are studies that indicated that depending on the amount of water present, we may go through an amorphous stage. And if we do that, we have some fairly significant problems because obviously it doesn't have any structure to give it strength.

So it is not that we haven't made progress. But in terms of the actual mechanical attributes at work, we are very much in the planning stage.

In fact, there was a question that was asked earlier about the impacts in terms of just starting work versus the QA. We have to make sure that we have the study plan approved and in place before we can start this work.

We do envision that within a year we would have the mechanical attributes portions of it done. But in terms of the minerology, that work is progressing.

DR. NORTH: One of my favorite things is the need for contingency plans, and this seems like a very good example of where it is needed. The very concern that I have is what we just heard back to the geochemistry. For example, I think if you have to line these boreholes, are you going to do this with cement? And what does this imply for the pH-Eh regime?

Are we talking about pH's of 9 to 12? And what are the implications that may have for all the analysis that we are doing for the geochemistry? So there are kinds of interaction that need to be explored.

My concern is that I don't see that that has been done. And maybe you do it a very crude back-of-the-envelope level to try to understand how serious the problem is and can you fix it easily or is it going to be very hard, because when you try to fix it, you cause some other kind of problem over here that you then have to deal with.

So I'll put my plea on the record. Do the contingency planning. Lay out these scenarios and figure out what you are going to do if you have a problem. And how immediate it is therefore, depending on the difficulty of doing that contingency response to get this kind of data early as opposed to doing the research on a very leisurely time scale.

DR. PRICE: What is the maximum temperature that you have plans to look at for this?

MR. BLAIR: We were looking at plans up to 250 C.

DR. PRICE: What perplexes me a little bit is that the temperature profile that you showed looked like it as 190 something, and then I've heard 232, 242, 243, 250.

Why is there such a wide range of estimates as to the maximum temperature?

MR. WILDER: Let me respond to part of that because I think some of the comments were during my presentation.

Part of the variations is in terms of what you assume the fuel age and the fuel mix to be and so forth. And so some of the higher values that I use were the bounding conditions, very young spent fuel, probably unrealistic at this point because many of our calculations were based on Code 3 exercises, taking half-year-old fuel. And of course with scheduled controls and so forth, it is very unlikely that we are going to be dealing with temeperatures that high.

The other thing is some of the temperatures that we were reporting, I think, were in response to what are the temperatures on the container itself. They are pretty close to the same as the borehole wall temperatures. There is a slight variation.

I think the differences that you are seeing depends on the assumptions that you make as far as what the emplaced waste is, how old it is, what the mix is, what the configuration is, what the configuration is inside the container. So I think the worst case is 250 to 250 degrees, the worst.

DR. PRICE: Thank you.

DR. VERNIK: Thank you.

MR. WILDER: We are now ready to move onto a study that was done at G-tunnel. This is a field scale study. It doesn't fit one of the five interaction bulletins because it really incorporates all of what we have been talking about.

But this was specifically a field skill test of the hydrologic and thermal response, and so we were testing the hydrothermal models, measurement techniques and procedures.

I should point out that it was a horizontal orientation. The intention was that we would continue the prototype testing in the vertical mode. Later on when we had a greater extent of welded tuff available -- at the time, the welded tuff that we had available was not sufficient vertical depth to be able to do a vertical prototype test.

And secondly, with the limited resources that we had at the time, we were not able to do everything that we wanted to in terms of the geochemistry and so forth, which a reference orientation we felt justified. So we started with the horizontal and this is the work that Abe will be responding to.

Three things, I think, that are key to the work Abe will report. One is that it did provide the understanding of physical processes that go on. It gave us an opportunity to compare the numerical and analytical codes and models for -- well, I'm calling it a generic tuff. It is not the repository horizon tuff but it is a welded tuff with similar mechanical attributes.

And finally, it did allow us to look at the instrumentation measurement techniques and evaluate them. So now I'll then turn the mike over to Abe Ramirez.

MR. RAMIREZ: Good morning. My name is Abelardo Ramirez and I want to tell you about the fuel tests that were done at the G-tunnel.

At the present moment, we will give you a snapshot of our interpretations but we reserve our right to change our interpretations as time progresses and we continue to uncover more interesting information.

I would like to say that what I'll do is give you a brief introduction of what we did during that test, and lead right into the summary, guiding you through the key observations that we have made. And then as time allows, show you some of the supporting evidence that drives those conclusions as you'll see.

The purpose of the G-tunnel test primarily was to

evaluate our ability to characterize the near-field environment. This kind of testing has not been done before, so one of our concerns was do we have the tools and understanding needed to properly characterize the near field environment.

So that was the main objective of prototype testing was to give us the ability to decide whether we are ready or not to do this kind of testing.

We were concerned about whether we had the right kind of measurement techniques available to us. We also wanted to provide in situ data that would allow us to evaluate the applicability of our conceptual models and also provide data that would challenge the predictions from the numerical code, the code that Tom described earlier.

We also wanted the opportunity to develop what quality assurance procedures and try them under the realistic conditions, see if we can have workable procedures and control the work in the field. Next slide.

The rock was perturbed by a heating and cooling cycle. What you see on the left here is the heater power schedule used. You see the vertical axis here, power shown in kilowatts. And on the horizontal axis, you see time and date from start of heating.

You see the figure of 128 days. We heated the rock at maximum power and the maximum power applied was 3.3 kilowatts. Now the heat loading rate for this test was about two and a half to three times that of an actual spent fuel waste package. The reason for the overdriving condition is that we have a limited amount of time in which to conduct the test.

We wanted to create a regional boiling that incorporates several practices within that period of time, and the only way to do that was to provide the rock with a lot more heat than what a true waste package could actually provide.

We were shooting to create a boiling region diameter of approximately 1.4 meters so that it would include within this boiling region several fractures. The fractures facing in this location is about .3 meters.

The heater was on for 195 days and the cooling ramp, after we heated at maximum power, we then started decreasing the power gradually over a period of 68 days to try to simulate a cooldown condition somewhat akin to a true cooldown situation in the repository, of course. In the repository, the cooldown will occur over a period of centuries and here we are making it happen in 68 days, so really accelerated. Next slide.

This is the plan view of the test region. Here we show the heater. The heater was about 3 meters long. It was emplacing a 12-inch borehole. You can see that we have 12 boreholes which monitors the response of the rock during the test. We had boreholes that came in at about right angles to the heater and were concentrated near the center of the heater. We also had boreholes that came in axially to the heater. Next slide.

These are the kinds of measurements that were made during the test. We measured temperature throughout the rock which was approximately 120 thermocouples used. We measured changes in moisture content using a couple of physical methods, dielectic, neutron logging and gamma density logging.

We monitored steam pressure during the test, matrix pore pressures using psychrometers, microwave resonant circuits, capacitance sensors. We used rock permeability testing of the heater borehole before we started heating, completed the heating cycle and then we turned to the heater wall and we checked the measurements to see if the rock permeability would change so that we could decide whether microfracturing occurred which would enhance permeability.

We also did fracture mapping and also atmospheric pressure. Okay.

I'm going now into the summaries of the key points.

We think that the primary contribution of the test is that it has allowed us to evaluate our conceptual models and point out areas where we have problems and point out areas where we are doing all right.

We know that if this cartoon is intended to illustrate the situation that we had around the heater, this is the heater inside a 12-inch borehole. We know that we created a dry region around the heater and the drying increases toward the heater inwards as we expected.

We know that we created a saturation ""halo"" immediately adjacent to the dry region and that this saturation "halo" as time progresses moves out basically as the dry region forced it out and increased.

The radius of the dry region matched the prediction of about .6 or .7 meters, or in other words, 1.4 meters diameter as I initially indicated. Fractures have a measurable effect on the drying condensation front. In the drying front, we know that the drying front tends to extend more where the fractures are present. We think our data shows that. The rewetting process when the temperature starts dropping and when water, therefore, can start coming back into this dry region, apparently happened primarily where the fractures were, so the fractures had a dominant control on the rewetting processes.

The measured temperatures were close to our

predicted temperatures, and we observed that the fractures had a slight cooling effect on the temperatures in the regions where boiling occurred. Next slide.

We also had some surprises. The rock below the heater dried faster as the temperatures increased. We think the reason for this is that we had some gravity-driven flow. And when the scoping calculations tests were performed, the problem was performed in such a way so that gravity was not a factor, so the predictions did not account for gravity-driven flow.

We also had increased fractures below the heater and as compared to the region above the heater. And it is also probably drying faster.

The rock above the heater rewetted faster as the temperatures started dropping. Again, we think that this is an indication of gravity-driven flow of the saturation "halo" overlying the heater region contributing water, similar to the dry zone above the heater, so that that route of flow would tend to move the water away from the dry region below the heater.

The "halo" of increased saturation differs from predictions. That is, this saturation "halo". We think that there are two likely reasons for this descrepency. No. 1, we know that the TOUGH Code that Tom used only had the drying factors incorporated into it. It did not have the wetting correctors incorporated into it because this data is not available at the moment . That would tend to overpredict the magnitude of the saturation level.

We also know that when we started the test, the ambient conditions for the rock were very close to full saturation, so there was only a very small delta to be filled in the bore space with water. And, of course, you cannot go beyond 100 percent saturation, so we probably reached 100 percent saturation very early in the test.

And once we reached that point, the water started draining away and we could not build-up much of the saturation level.

We also saw slight increases in the rock permeability as measured by the gas permeability measurements.

In terms of the instrumentation, I can give you a brief overview. We did find that we had several problems. We found that some of the instruments corroded badly. We also found that some of the calibration processes that we thought were adequate were indeed inadequate and needed to be revised and we had some of the instrumentation giving inconsistent results and this is still not understood.

Next slide.

Now I would like to show you some of the changes of moisture content measured during the test. I would like to describe to you some of the results. We have the heater going into the page. We have three container boreholes that cross the heater at about its midpoint, and I would like to include all the data from all three boreholes into one plot. The way to do that was to calculate the radial distances to each measurement point for all the measured values and then show them together.

Next slide.

This is the changes in moisture content measuring functional radial distance 70 days after the start of heating. This is about midway through the maximum power phase of heating of the test. The white access shows changes in moisture content. The zero change is shown by this dark line, meaning that the rock is still ambient, near ambient conditions plus along the line.

We can see something quite interesting; that is, that the rock below the heater shown in the little circles is drying at a much faster rate than the rock above the heater shown by the triangles within the boiling region, which at this point in time extends maybe .6 or .7 meters into the rock mass.

As I said before, we think that this moving flow is moving moisture away from the bottom portion of the heater at a faster rate than for the rock above.

If we take some of the same data tests and blow it

up so that you can see some of the detail or some of the information shown here, we see we also see evidence formation of a saturation "halo," in that some of the changes in the moisture content are above ambient conditions as shown by the positive change here. So that it shows that indeed a saturation "halo" formed. Next slide.

Now, this is a snap showing time of moisture content changes the last day that we took data. So we've gone through the full heating and cooling cycle. We have allowed the rock to recover for 100 days after the heater was turned off, and now we want to see how the rock rewetted, where the rock was gaining water.

We see that the rock is gaining water primarily above the heater compared to the rock below the heater. The rock below the heater, in fact, has gained relatively little water, where the rock above the heater shown in the triangle is rebounding at a faster rate. And as I said during the completion slide, that this is probably evidence for a gravity flow bringing water from a saturation "halo" above the heat. Next slide.

We've also seen evidence that fractures control the rate of rewetting. Again, the heater is going into the page. The same three boreholes that we have been talking about are shown in here. I've added the fractures map

along each of the boreholes shown by these dark lines. You can see the dark lines in here also. The objective of this line is to show that the rewetting is occurring primarily where the fractures are.

The graph that I'm showing, I show the changes in saturation along this borehole, in NE-2A, and the changes are calculated relative to the velocity of heating which should have been the maximum drying that occurred.

So we want to see where water is returning in the rock relative to the last day of heating. What we see in the heating is that the changes, the increase of saturation, are occurring only where the fractures are.

Next slide.

We think that the explanation for this is that the fractures are helping the rewetting process in one or more ways. No. 1, it allows humid air that is in equilibrium with water to force the water to move freely. You'll come into the dry region and then because of the suction pressures in the rock, the rock can pull humidity out of the air and condense it and make it part of the pore water.

It also allows for a possibility of gripping off of saturated regions above the heater to occur and bring some of those drips into the region and again, that would also again be brought inside of the formation.

The other possible mechanism to be playing a role here is that the humid air along the fracture can move -- the humidity in the air can move into the formation by a process of binary diffusion quite easily. Once there is a strip of increased saturation, you still can have binary diffusion move past that strip of saturation and move into drier portions of the rock and help increase its moisture content. Next slide.

The predicted and measured radial profiles are different. Here is the predicted moisture content profile. Here is the measured moisture content profile for the same time during the test. We see that in terms of radial distance of the dried region, the predicted and measured values are quite close to each other.

Where the difference comes is in the magnitude of the saturation level "halo." Here we have the saturation "halo" is overly predicted compared to the measured values. And as I said before, we think that this has to do with the fact that, No. 1, we started the test at almost full saturation or very close to it. And No. 2, we also know that the code would not predict the results anyway because we have only drying factors occuring incorporated into the code. Next slide.

I would like to show you now some of the temperature results that we have. The little symbols

shown in space are the location of the thermocouples. The heater is going into the page again as before. This is a flow of temperatures showing all of the measured temperatures two and a half months after we started heating, so this is about halfway through the full-powered heating cycle.

We are plotting the temperatures at a natural log of radial distance and what we find is that over in this region, most of the temperatures are plotting along a straight line. And what that means is that the heat transfer mechanism is conducting as we would expect.

We also see that as we move into the region above the boiling point isotherms, this is significantly be formed relative to a straight line. If you were just to project that straight line, you would see there are several points that plot below a stright line projection. After examining the data, you see that that area corresponded to regions where fractures were. And what we think was happening in this situation is that the fractures are allowing the steam to escape easier from the matrix.

In doing so, it removes some of the energy available to the system of those locations and takes it away and that creates a cooling effect. Just in the order of a few degrees, however.

We also see that one of the boreholes, borehole TC2, plotted quite a bit cooler than all the other boreholes, indicating that there is substantial thermal heterogeneities of thermal conductivity within the rock mass. Next slide.

Now I would like to show you some temperature information for thermocouples 87, 88 and 89 that you can see are below and to the side of the heater. Thermocouple 87 shown in the square is shown by this upper profile and this is a typical time temperature history.

We see that the temperatures increase fast up as the heater is turned on. As we are seeing the boiling point isotherm, we see a little dip, as predicted by the calculations and continue to increase. And then we start the rundown as they decrease. In fact, most of the thermocouples that were monitored had this kind of response. Two of them, however, had a very different response and these are thermocouples 88 and 89.

Notice that the temperature increased quickly here and then began to rollover. Then all of a sudden the rate of temperature increase picked up again similarily over here and began to rollover. And then the rate increased again and then they pegged both at about 97 degrees C, which is the boiling point of water for this elevation.

We think, referring to the next slide, that the

conceptual mode that we explained today will be as follows: We have the heater going into the page. We know we created a region of dry rot. Within this region we have rigorous boiling. We created high gas pore pressures. We drove some of those gas pore pressures outwards and that steam was condensed when the temperatures were cooled, raising the saturation "halo."

Now, the the predictories are going to be different, however. For example, for steam traveling upwards and condensing, if the rock was fully saturated, then gravity-driven flow would tend to move the condensed water back into the boiling region and create some sort of a reflux mechanism that would tend to stablize the push of the driving upward.

If you go to this side, you would see that the steam would flow outwards from there. For the region of a full saturation then, rather steam flow would tend to move the water tangiential to the margins of the dry region. And for the region below the heater, we see that the steam flow would bring water to this location and then condense it and then rather the steam flow would take that water away.

We think that for the two thermocouples that I showed you earlier, they were located in this region, but what we were seeing was the shedding of condensate water

just below boiling reaching that condition at some point during the test. Next slide.

The predicted and measured temperatures are very close and here we can see the predicted temperatures are shown in the close where that compared with P2 and P3 temperatures that they are within a few degrees of each other. Next slide.

We also saw that the heating changed the air permeability near the heater wall. The preheat values, this is gas permeability. Note the logorythmic scale, and this is, therefore, on the heater borehole. The location of the heater element is shown by this dark line here.

The preheat values are shown by the open squares in here. The post heat values are shown by the closed diamonds. I've also plotted fractures mapped along the heater borehole by this dark line.

What we see is that where the rock got hottest, that is near the middle of the heater, we saw the largest percent increase in gas permeability as compared to other regions away that only saw very small percent increase in permeability.

Now, we think that while this is interesting and important to note, in terms of the performance of the hydrologic system at this location, this is really a relatively small change because really what will dominate saturated flow conditions in this case would be this region of high permeability here. And even though this increase is still two or three orders of magnitudes below some of the higher regions of naturally-occurring permeability.

I believe that that is the last one. Any questions?

Thank you very much.

DR. PRICE: We are running just a little bit late here.

MR. WILDER: I think they are talking about the schedule outside right now.

(Whereupon the hearing was concluded at 12:30 p.m. and continued to January 19, 1990)

JANUARY 19, 1990 9:00 A.M.

CONTINUED PROCEEDINGS

DR. VERINK: Good morning. I'd like to make a few comments about the laboratory tour yesterday.

The members of the Nuclear Waste Technical Review Board, and particularly the panels on Containers and Transportation greatly appreciate the courtesy extended to us during the tour of Lawrence Livermore yesterday.

This certainly is a national resource in both equipment and personnel. As a corosionist, I particularly licked my chops over the state of the art corosion research equipment and staff.

With regard to other impressions of the visit, several of the laboratories, the QA bibles were displayed in such prominent positions it would be easy to conclude that this was job one.

Considering the immense and expensive effort required, one wonders if it might be possible to capture at least some of the effort on behalf of other laboratories concerned with QA programs.

Perhaps QA approved procedures could be published in some suitable form along with a list of any appropriate applicable literature references. Possible publishers might be ASTM or NACE, for example.

In support of this idea, it was noted that the availability of an ASTM standard for conducting potentiodenemic polarization tests greatly simplified the obtaining of QA approval for corrosion test procedures using the potentiostatic equipment. And that could be a nice piece of economy to capture some of that cost.

Apparently the major activity to date has involved review of the literature and QA. By comparison, relatively little actual laboratory research has been recorded at least.

In the meantime, an outstanding array of equipment has been assembled, much of which now has been qualified through QA programs and greatly increased research productivity is obviously anticipated.

It's hoped that today's speakers will include in their comments a few extra little goodies here. Some indication of when the program being presented had its start and what it's history's been, as well as the present status of the laboratory research programs including any milestones for work that has been planned.

Secondly, we'd like to have some indication of when some sort of bottom line conclusions may be expected concerning materials for the waste package container.

Then if possible in addition, we'd appreciate any comments from the speakers with regards to the questions posed yesterday in the introductory remarks, namely would shifting additional responsibility to the waste package from the host rock for containment and isolation of the waste be an advantageous alternative.

And secondly, would in placing the waste under conditions that avoid the near field temperature rising above the boiling point of water have a beneficial effect on containment life.

That's a big order, but if any of these things can be put into the situation I think it would be very helpful.

Yesterday we transferred forward the last speaker from the program. Will, are you going to be in charge of this? Suppose I turn it over to you and let you pick it up from there. We'll hear from the first speaker.

MR. CLARKE: Okay. The first speaker then will be the carryover from yesterday. This is Richard Van Konynenburg, and he is going to be discussing radiation chemical effects, the effects on the environment.

This is work that has been done under my technical area. Rich actually is in our technical area so it's not inappropriate that he start our material selection today.

The work that he is going to report on basically was done under subcontract to Argonne National Laboratory. Rich will go over the people involved in that effort.

It is a very interesting piece of work that is being done and it is very vital for us to understand the environment as it relates to the corrosion mechanisms and processes that we'll be discussing throughout the day.

So with that, Rich, would you like to come up, please.

DR. VAN KONYNENBURG: It is my pleasure to talk to you today about the chemical effects of radiation that we expect in the near field of the waste packages.

Could I have the next slide.

In response to your question about the history of the project, I'd like to just say that there have been several people involved at three different laboratories. And some of the radiation chemistry work has been done in cooperation with other parts of the project, for example the waste form dissolution. The work done by John Bates and his group at Argonne, the bottom three folks here, was involved with that.

And we did radiation chemical studies at the same time he was doing leaching of the glass under gamma radiation.

Wayne Yunker at Westinghouse has done some work on corrosion of copper alloys in gamma radiation environment. And that was part of our copper feasibility study that was held a few years ago in response to the request to consider copper as a candidate.

And then Bob Glass and George Overturf at our laboratory did some work also some years ago, looking primarily at the effects on corrosion potential. And Joe Farmer will be showing some of that data later on in his talk.

The material I'll be presenting today has primarily been done by Don Reed and the next two gentlemen there at Argonne National Laboratory.

Okay. Can we have the next one.

And just to recap from yesterday, we talked about the various environments that we're talking about, both expected and bounding. And for the corrosion work, because of the range of environments that could be possible in Yucca Mountain, we're considering corrosion in considerable environments.

And particularly in this talk I'll be talking about the radiation chemistry in these environments. And it includes water vapor, mixtures of air and water vapor, liquid water solutions as you can see here, and then two-phase where there is both air and water in contact.

Okay.

This is sort of a summary of the talk. I'll start out and talk about the types of radiation that we expect and dose rates. Then I'll talk about the elemental composition that we are radiating.

And then the effects on the environment, and these are essentially the same environments that I showed you except it also has now dry air as kind of a limiting case which is helpful to understand radiation chemistry.

And then finally I'll talk about some effects of these two products on corrosion of candidate material. I'll be primarily talking about moist air. We have done some of this work. And as I said, Joe Farmer will present some of that data later.

Okay. But first of all, what kind of radiation do we have? Well, fission products and actinides, which are the components of high level waste and spent fuel, produce the whole gamut of types of radiation.

Okay. Could we have the next one.

Now, the important thing here is that as long as we have intact cans, a lot of these types of radiation will not be able to penetrate the wall and get to the environment outside the waste package.

So I have here sort of a nominal container wall thickness and I put down 10 millimeters -- this is subject to some change as design proceeds and so on -- but this is roughly the range we've been talking about.

You can see then that the types of radiation that are higher on the chart here have shorter ranges, would not be able to penetrate the wall. On the other hand, these below would be able to.

We can reject neutrinos right away. They have a range so long that they won't do any observable damage to the environment right around the can. Neutrinos can go through the sun after all.

Neutrons we can also get rid of. The dose rate due to neutrons is something like five orders of magnitude lower than that due to gamma rays in spent fuel. So again we can rule that one out.

We're left then with gamma radiation.

Can we have the next one?

Now, this is a calculation done by Don Reed and a coworker of the dose rates of gamma rays expected. Now this is a calculation done with a tough environment around the waste package. In this case it was a copper waste package with various wall thicknesses.

And there are two plots here. One is the dose rate in rads per hour as a function of distance from the container.

The other is dose rate in rads per hour as a function of time located at various distances.

Now, the important things are, first of all, that the dose rate dropped very rapidly with distance. Rock is a good shield for gamma ray.

So in a few tens of centimeters, you can see orders of magnitude decrease in the dose rate. The imporance of this is that the radiation chemical effects that are going to be significant will be right near the packages. Once you get out into the rock the dose rates will be too small to have serious effect.

The other thing is that we have a dropoff in time which is something like a 30-year half life. That's because CZ137 is a dominant gamma emitter during the containment period. It has about a 30-year half life.

And then the final thing I'd like you to notice is the size of the dose rate. And the maximum values we have are in the range of 10 to the fourth rads per hour, something times 10 to the fourth. And that's for spent fuel.

Now for glass waste forms we expect something like an order of magnitude less in terms of maximum dose rate. And then it depends on the exact design of the package, the age of the waste in the package and so on. But these are roughly the numbers we're talking about. Okay.

Now, the other important thing is what are we radiating with these gamma rays? And we are radiating -- the expected environment here is moist air. And we've got a couple of variables. One is temperature and the other is humidity. So I show plots for two temperatures here, 90 and 120, and range of relative humidity.

Now, the point that I want to make is it really makes a difference when you vary relative humidity, for example, what species you're actually radiating.

You could, for example, if you had relatively dry air be radiating primarily nitrogen if your relative humidity were down here, because air is essentially 80 percent nitrogen, 20 percent oxygen.

On the other hand, if you were at say a higher temperature and you had a high humidity, you could be essentially radiating pure steam, pure water vapor.

Now, the chemistry that you get out of this depends on what you're radiating, because the gamma rays come in, they interact with the electrons primarily by cause and effect, and then they cause ionization of the species that the electrons were found in.

And from there on, those ionized species undergo chemical reaction and what reactions you get depends on what species those are. Okay.

Okay. So here I'd just like to summarize. This information is primarily from the literature. However, we've done some of our own experiments as well and confirmed a lot of these results.

The literature data is primarily at room temperature. We of course are interested not only in ambient temperature but also elevated temperatures up to something like 200 or 250 as we talked about yesterday.

So we've been doing studies to see if the effects change very much when we go to high temperatures and also high humidities. A lot of the moist air work, for example, is done at fairly low humidity in the literature. We're interested even in higher humidities.

So first of all, I have put down here some limiting cases for what happens to these various environments when they're radiated with gamma rays.

First of all, pure water vapor, what one finds is small steady-state concentrations of hydrogen gas, oxygen and hydrogen peroxide. Now, there are back reactions that cause these to reform water and that's the reason they don't keep building up. You just reach a steady-state small concentration.

Now, if there are catalytic materials present, and a couple examples are copper, which is the basis for some of our candidate materials, or manganese dioxide which has been reported in the tuff environment, if those are present those serve as good catalysts for the decomposition of hydrogen peroxide. And so if that occurs, you come back to water and oxygen.

Now, a living case here is dry air. We don't expect dry air in the repository, but suppose we consider that first because it makes it easier to understand the moist air case.

If dry air is radiated, one obtains nitrous oxide, otherwise known as laughing gas, ozone and N_2O_5.

And then a very short time later, the ozone decomposes, converts the nitrogen pentoxide to NO_2. This is the brown gas we're all familiar with in smog.

And then the long-term products then are the N_2O and the NO_2.

Now, N20 is fairly inert, it's chemically stable so we don't have to worry about that from a corrosion standpoint. On the other hand, NO_2 is a reactive gas and particularly with copper. Okay.

Now, suppose we have some moisture in the air and we radiate it, things are somewhat similar at room temperature and low humidity, which is what's in the literature primarily. Again we get the N_2O, again we get ozone.

But now we get nitric acid in the gaseous state. At high humidity, and this is our own work, this is relatively new work, we found at high humidity we can observe a small amount of ammonia. That was a surprise.

In the literature, whenever oxygen has been present in any sizable amount, the reactions have gone toward the nitrogen oxide side. Here we're seeing it going toward the reduced side, ammonia to a small extent.

The reason I bring that up is because some copper alloys have been known to be sensitive to ammonia in terms of stress corrosion cracking. So it's an important thing for us.

Now, for liquid water, if one has pure water in a closed system, again one sees small steady-state concentrations of these species just as we saw in pure steam.

If solutes are present in water, which they always

are in real groundwater or vadose water, or if the system is open so that these gases could escape, then one could see net radiolysis to hydrogen and oxygen. And as time progresses the water breaks down into hydrogen oxygen, much like an electrolysis process only this is radiolysis.

The reason for that is the back reactions now no longer occur. The back reactions that would switch these back to water don't work because the solutes are scavenging up the free radicals.

Okay. Then in the two-phase system where we have moist air in contact with liquid water, for example in the pores of the rock, we find that nitrogen from the air is fixed as nitrite and nitrate ions in the water.

So we start with nitrogen gas, radiate that, and that then dissolves in the water and comes out in the form of nitrite and nitrate in the water.

We also produced hydrogen ions in equal amounts. So the result of that is we've got nitrous acid and nitric acid in the water.

Now the pH, of course, then drops because it's becoming acid, unless a buffer is present. Now, in our situation bicarbonate is a main anion and that's a good buffer.

So we found in experiments at Argonne that in this kind of a system we dropped the pH to about six and a half and that's where it held because we didn't radiate long enough to overwhelm the bicarbonate buffer. So that's an important factor.

Radiolysis of water to hydrogen and oxygen also occurs, particularly if solutes are present. Again for the same reason we talked about. Okay.

Now, I'm going to switch and talk about the second part, which is, what are the effects of these various regulated species, these products, what are the effects on corrosion.

Now, as you'll be hearing later on today, we've got several candidates and three of those are copper based alloys. And so we looked in the literature for effects of radiated air on copper and copper alloys.

And there is some data going back to the later '50s indicating that at room temperature under some conditions you get a species known as dicopper trihydroxide nitrate or basic copper nitrate, which is sort of a blue-green material forming on copper. And this has been reported by several groups over the years.

So we were concerned about that. We didn't know if that was a protective species. What would the corrosion rate be. Would they be parabolic tapering off, would they be linear.

We had to know what the corrosion product was to understand the mechanism to get down to what is the corrosion rate versus time, which is what we need to check for the future.

So in order to try to get an idea about this, we considered where could we find an environment that had been subject to large amounts of radiation for a long time in moist air with copper alloys present.

Well, it turned out that down the street we have an electron linear accelerator and this facility was about 15 years old. This is a zero degree cave, which is a room underground straight off the end of the accelerator. So that's where the beam dump is and that room has been subject to bremsstrahlung and X radiation for a long time at dose rates that are the highest at our laboratory and I don't have a good number for what they are.

So we can't get really quantitative about production rate here, but we can sort of look at this as an analog for this kind of situation.

Well, you'll notice that there is a lot of blue-green color in this picture. Most of that color is found on copper cooling water pipes. For example, these pipes you see here, those are all ordinary copper cooling water pipes running down along the floor. There's also copper tubing here.

And you'll notice that they jibe pretty well with the blue-green color here on the color chart.

Now, just because copper looks blue-green does not mean you have basic copper nitrate. There are lots of other corrosion products of copper, for example the basic chloride, the basic sulfate which you see on the Statue of Liberty, the basic carbonates and so on. Same sort of color.

So we did X-ray to fraction analysis and we did identify that this is the nitrate. Now that's unusual, normally you don't see the nitrate. Nitrogen is a fairly stable gas, and to break the triple bond of nitrogen you need to do something drastic.

So here's an example of how radiation corrosion really does show up. Okay.

This is the end of the beam dump. This is stainless steel here. This is a copper jacket and it was covered, of course, with this basic copper nitrate. You can see I scraped it off and that's where we did the analysis. It was quite thick.

Okay. So then we proceeded to do some corrosion experiments of our own. These were done by Don Reed and his group at Argonne Laboratory.

And you see here that we did put in our three candidate copper based alloys. We also put in Alloy 825 more or less as a control.

These are preliminary experiments, so we have only one coupon of each material. The statistics is not great. We are in process now of doing experiments with larger number of coupons for a longer time.

We did do a range of relative humidities and the total pressure was floating. In other words, the pressure of air was constant of dry air. But then on top of that is whatever the vapor pressure of water is for these relative humidities and whatever temperature we ran.

Okay. So the total pressure floats depending on what the water partial pressure is. But the amount of air in there is constant.

We ran for one month and this dose rate, as you can see, is in the range that we were talking about, ten to the fourth rads per hour range.

We looked at the sample by weight loss and gain, x-ray to fraction, scanning electron microscope, the analysis of the corrosion product.

Now, there's a picture -- you can't always tell a lot from pictures, but let's look at the pictures and then I'll talk about the analysis.

The first thing to notice here -- again, these are all done at that same gamma ray dose rate. It's moist air environment. And these coupons are about one centimeter by two centimeters, that's the scale.

And these are the relative humidities. First we have dry air here on copper at 90 centigrade, and then we have 100 percent relative humidity.

Now, you can see the tremendous effect of humidity.

And we found our largest dose rate, our largest corrosion rates at the 100 percent relative humidity.

Here's the aluminium-bronze. Again you can see for dry air it didn't do too much. And we have quite a bit of oxidation at the high end.

This is a higher temperature on copper and three relative humidities. And you can again see the big effect of relative humidity. It might be hard to see here, but we have some flaking of the corrosion process here.

Here's the copper-nickel. And again you can see the effect of humidity, sort of mottled appearance here at the high humidity.

And here's aluminum-bronze. And again, this is a little bit harder to see, it didn't show up. But these were very thin corrosion layers here and this one again was .6.

Could you rotate that and -- okay.

Now, what we have here is you can see the scale, one centimeter, this is a copper-nickel specimen that was irradiated at a little bit higher dose rate, two times 10 to the fifth. Now, the others were 7 x 10 to the fourth.

This was one 2 x 10 to the fifth, the temperature was 120 centigrade.

And you can see here that this corrosion that you can observe, but also the feature I'd like you to look at is this large feature here. And we have a blowup in the next shot of that. That's one millimeter. So you can see it's not exactly a pit, it's more like a crater.

But the important thing is we saw something like three of those on this specimen that was a centimeter by two centimeters. We only have the one specimen in this condition and we are repeating these results.

So I think it's important to say that we need to confirm this and find out is this repeatable.

But we did analyze this, gave good data for X-ray to fraction and it is the basic copper nitrate that I'm talking about. So we do see this forming.

And an important thing is that it really is a function of humidity. Earlier performance done by Wayne Yunker at Hanford did not show this, and we now believe that the result that he saw happened because the humidity was much higher there.

In our case when we run up to the high humidities, we do not see the nitrate. We just see the oxides, cuprous and cupric oxide. But when we run to low or intermediate humidities, we do see it. Okay.

So then this is a result of the X-ray to fraction in the SEM work to see what we actually had on the surface. And for the pure copper I've listed here the three -- they're the same in each one.

On the pure copper we always have cuprous oxide as the major product. And this is -- this is what one finds when you don't eradiate, that's very common.

We also found cupric oxide when our humidity was high. And the nitrate phase then was present at low relative humidity.

For the copper-nickel, again we saw a lot of cuprous oxide and we saw some cupric oxide at the high humidity and the nitrate phase was the major product at low relative humidity, the blue-green color.

Aluminum-bronze, again cuprous oxide, some cupric, and at low relative humidity we saw nitrate as a major one.

Alloy 825 we didn't observe anything. It looked like a mirror going in, it looked like a mirror coming out. There was no weight change within our precision of measurement, we couldn't see anything with the microscope.

Okay. To summarize then, gamma radiation will be present and dose rates will be as high as somewhere in the 10 to the fourth range rads per hour.

Radiation chemical effects will depend very strongly on this humidity, since we have a range of humidities to deal with we need to understand that propensity.

These are the species that can form, depending on environmental conditions, and those are all implicated in various types of corrosion, they're reactive chemicals.

And particularly on copper based materials, we have seen some pitting and we have seen the formation of the nitrate.

We didn't see any radiation chemical effects on Alloy 825 in the length of time that we ran, which again was only one month for these occurring. And we do need to do longer term tests and those are underway now.

Okay. Are there any questions?

DR. PRICE: When did the program begin and has it been a continuous program, the radiation chemistry?

DR. VAN KONYNENBURG: The radiation chemistry has sort of been, I would say an auxiliary to other things that have been going on. That's the way it was initially. As I said, it was done as part of the glass leaching analysis.

And then we had a small effort in electric chemical measurement which Joe will talk about, and I think that was done in the early '80s.

And then we started -- because we were concerned

about the behavior of copper in a radiated environment when we had the copper feasibility studies, which were I guess in the mid '80s -- is that right? '85, '86 range -- then we had this worked done by Wayne Yunker at Hanford to see whether copper would have a problem in radiation.

At that time we decided that we couldn't say that it did, because the corrosion rates, while they were somewhat elevated, and we'll see this data later today, they still weren't huge. But we also did not see the nitrate phase.

And now we have continued to do this work at Argonne lab, and that's been going on now for, I would say two or three years. And we now think we understand how to interpret the rest of the data that we have.

It's a matter now of trying to get quantitative and to see what these rates really are.

DR. PRICE: Would you say you actually started in the '70s?

DR. KONYNENBURG: No. This project didn't start up till '82.

Now, a lot of the literature data that I described for you was gained on other projects back into the '50s.

DR. VERINK: Was there anything about Carbon 14 in any of these experiments?

DR. KONYNENBURG: No, not in these experiments. That's a separate pursuit.

DR. VERINK: Is there any background information on that that we could see or hear?

DR. KONYNENBURG: Yes, there is, and I guess I would refer that to Mike Clonniger.

MR. CLONNIGER: Dr. Verink, we're looking at Carbon 14 release particularly from the cladding material itself and the hardware and crud.

It can be released as carbon dioxide, as you know, and through a breached container be released into the air space, into the rock, and subject to transport as a gas. And we're looking at how big a problem that might be, what that might imply in terms of allowable container failure rates post closure and any engineered solutions that may be necessary.

DR. VERINK: Are there any reports for anything like this that we could have to look over?

MR. CLONNIGER: We have one draft report submitted and under review now. Rich wrote it. I understand that we can turn over draft material to the Board.

DR. VERINK: Then we'd like very much to have that --

MR. CLONNIGER: Sure.

MR. VERINK: -- if we may.

MR. CLONNIGER: May I ask a clarifying

question,

Dr. Price?

Rich, you said that neutron radiation wasn't a problem.

A VOICE: I don't think your mike is on.

MR. CLONNIGER: Am I coming through at all? How about if I talk right here?

A VOICE: That's better.

MR. CLONNIGER: Okay. Rich, you said that the neutron radiation was not a problem. And I misunderstood the reasons, because the flux was so long?

DR. VAN KONYNENBURG: That's right. The flux from neutrons initially is something like 10 to fourth neutrons per square centimeters per second.

If you're familiar with reactors, you know that in an operating reactor core where, for example, the spent fuel came from, you're dealing with something like 10 to twelfth or 10 to thirteenth neutrons per square centimeter per second.

So many orders of magnitude down from what it would be in a reactor core, and then if you look at the actual dose rate in terms of ionization of the moist air or dry air or whatever it is, around the package, it's about five orders of magnitude lower ionization rate than that due to the gamma radiation.

So from the standpoint of radiation chemistry

neutrons aren't important. From the standpoint of activation of species, the amount of radionucleus produced by neutron capture reaction are again many orders of magnitude below what you already have there from the nuclear waste because, of course that was in the reactor at much higher fluxes.

So, yes, neutrons can be dismissed from the standpoint of effects that we were interested in here.

DR. PRICE: For the sake of clarity on the record, I think we'd like to make an action item and request that draft report on Carbon 14.

DR. CARTER: I have a couple of questions, Rich, I might ask you.

One, in the work you've done to date, the implication is that the concentrations might be sufficient for some of these species to give you problems as far as corrosion. I think that was just an implication.

Do you foresee that concentrations would be large enough to cause significant problems as far as increased corrosion rates?

DR. VAN KONYNENBURG: I think the key question is what will the envioronment actually be. If the environment is moist air, or essentially a gaseous environment, then the only liquid water we should have would be something like physisorbed layers on the surface of the metal. We're talking about less than ten molecular layers of water.

Well, even though the production of radiolytic species is small in terms of total number of mols, if you dissolve those in a water layer that thin, yeah, then the concentrations can go up.

And additionally if there's no buffer, if that water has no bicarbonate in it, let's say it appeared there by condensation from gaseous water, then there's no buffer, you can drop pH in a situation like that.

And we have seen the effects of peroxide on the corrosion potential and we'll see that in later talks.

DR. CARTER: So there's a potential there it could be a significant problem.

DR. VAN KONYNENBURG: Yes, I think there is.

DR. CARTER: Now, I presume if you gain a better understanding of the situation as far as the quantitative side of it, that if need be for corrosion control, you could basically control or engineer the environment to minimize those sorts of problems.

I'm leading you a considerable amount, I suppose, but --

DR. VAN KONYNENBURG: Do you have in mind putting some --

DR. CARTER: Yes.

DR. VAN KONYNENBURG: -- species there to buffer or to --

DR. CARTER: Yes.

DR. VAN KONYNENBURG: -- counteract together the species you're forming --

DR. CARTER: Yes.

DR. VAN KONYNENBURG: -- or something like that?

Things like that could sure be considered.

Okay. Thank you.

MR. CLARKE: Okay. What I want to do is set the stage for the remainder of the day. This as you were just shown is now called the Container Materials Modeling and Testing Technical Area, of which I am the techical area leader.

The objectives in our authorized reference path that I will be discussing, that the speakers the rest of the day will be discussing and work that they are doing, is to meet the objectives that are shown on the board. And that is to select a material and fabrication porcess for the waste package containers.

And then also to identify the most likely modes of container failure after emplacement, to develop models for prediction of container lifetime. To perform testing that is required to develop and confirm those models. And then to provide the models and the supporting data in a form usable for performance assessment.

Now, Les showed you this diagram yesterday. I want to just repeat because this is the container configuration that we are currently doing research to select the material for.

As you know, it is a thin wall container somewhere between one and three centimeters in thickness depending upon the material that is finally selected.

And alloy like 825 would most likely be somewhere about one centimeter in thickness, whereas if pure copper was selected our current thinking is that it would be somewhere like three centimeters in thickness.

The size of the container as you have seen, the spent fuel containers are about 15 and a half feet high and the waste glass containers are about 10 and three-quarters feet high.

The talks today or the work that we are going to be reporting on is really the strategy for the container material selection, and this gives a breakdown as how the talks are organized.

We will start off with the selection criteria by Bill Halsey. This criteria is a process that he will be describing that is now being done under the new QA Level 1 effort. And that, of course, is one of the inputs into the total selection process that Bill will be combining in the talk, the selection criteria and the selection process.

Then after Bill has gone over that part of the talk, Dan McCright will be discussing the degradation mode survey.

I will point out that those are surveys of existing literature, not data that we have developed here. However, they have indicated areas where testing is needed.

Then Joe Farmer will be discussing our materials testing and modeling effort in support of the selection process and also beyond the selection process.

I just want to in the introduction say a few words about other parametric studies. We are not prepared to discuss those today since in many instances work has not started or is not sufficient at this point to give you data on. But we will be using information from the process side of other parametric studies that will feed into the selection process.

The output of this then will be the material for the advanced studies as we lead into licensing.

Now, to break each one of those boxes down and just give a very brief overview, you're going to get annotated histories from these people and also you will learn more in detail.

The selection criteria is a material independent exercise. The criteria was derived from functional requirements on the container. These come from the SCP and strategy documents.

They will be discussing how a relative weighting of criteria topics was established.

First each candidate must meet the minimum requirements of a pass/fail and then they are assigned a quantitative score or a figure of merit to allow comparison of the various candidates.

The selection criteria that you will be hearing about this morning has been formally peer reviewed, and that report is forthcoming.

Dan then will be discussing the degradation mode surveys. This is a very extensive effort as you are going to hear. Their first -- the staff in total sat down and determined the various modes that we thought were important after emplacement.

There was a survey of the literature by the entire staff over the last about year and a half, two years.

They established the mechanisms that were important, they compiled the data, analyzed that data. That's what you're going to be hearing about in great detail this morning.

And then also that indicated needs for additional data that was required, and that is directly some of our test efforts.

Joe then in the material testing and modeling area, those people that were on the tour yesterday saw a great deal of this and talked to Joe and learned a lot about what he is doing already.

However, in selecting models he had to write the degradation mode surveys, he had to survey the literature, find those models that are presently existing, those models that maybe we can use but we may have to modify, and those areas where models are lacking and we will eventually have to develop.

So after his evaluation and selecting, he is now establishing our model needs and then we are entering into the testing phase.

You saw one of the types of accelerated testing and the critical fitting potentials that Joe was doing yesterday.

Also I think Greg Gdowski indicated some of the long term tests that we have started and we have others to begin.

The model confirmation type of testing is like what was shown yesterday in the reversing DC area, and that is crack growth behavior that we can try to make predictions on for long term behavior and much lower crack growth rates.

Just an overview of some of the test environments. You've already heard from Rich on the ANl radiation tests, and this gives you an indication of the type of testing that's being done at that facility.

These are ongoing tests at Argonne by the way. These tests will at least be carried through to the end of fiscal year '90.

We're also at ANL doing slow strain rate tests. Most of these tests are over and a report is in preparation by Argonne. These were done in simulated well

J-13 water, both at the 1x and the 20x concentration. Also was done in aqueous and in the gaseous environments at 90 degrees centigrade.

The reversing DC tests that you saw yesterday have not started although the specimens are in place and we are in the process now of putting together the documentation to start these tests.

The initial test will be done in simulated J-13 water in the aqueous environment, and the first test will be done at 150 degrees centigrade.

The polarization test that Joe is doing is also in simulated J-13 water. He is currently varying the pH and the chloride contents but eventually will be adding other species to the water. This is aqueous test below boiling.

And then the long term corrosion tests that Greg mentioned yesterday, again in simulated J-13 water in the gaseous phase initially and at the various temperatures 50 to 200 degrees centigrade.

Now, Dan is going to be discussing in great detail many of these parameters for the various candidate materials, but basically he will be discussing the mechanistic aspects of corrosion, phase analysis and that sort of thing.

There are other parametric parameters that have to feed into the selection criteria involving the processes.

Weldability. Is the material weldable and what are the difficulties encountered. Phase stabiity as related to weldability. Mechanical properties is one of the major inputs. Fabricability and closure of the material.

One that we consider extremely important that we have yet to start our effort on is microbiological corrosion. MIC can occur either from microbes that are present in the unsaturated tuff or also can occur man-made, brought in during drilling operations and other mechanisms.

And then, finally, cost is considered extremely important.

Okay. At this point I'm going to turn it over to Bill unless there is any questions.

Okay. Bill Halsey will start then with the selection process.

DR. HALSEY: Good morning. My name is Bill Halsey and I'm going to be discussing some of the material selection issues for the containers.

You just saw this chart. I'll start out with it and you'll see it again. It sort of outlines everything you're going to be hearing today.

I will be discussing some of the selection criteria, some of the history of the candidate material list and a little bit about the selection process.

We have also conducted an independent peer review of the draft criteria. And we intend to conduct a peer review of the selection when it is clear. I will discuss that.

Dan McCright and Joe Farmer will discuss information gathering activities which support the selection process.

The testing and modeling is a different shape than the degradation mode survey, because this is an ongoing activity which supports the selection process but also iterates with the design and all the way out through container licensing or repository licensing.

Parametric studies are also a set of ongoing studies that will just describe but are not discussed in

detail.

The goal of all of this is a material for advanced study.

A brief outline of the process. We began by developing a list of candidate materials. I will give a brief history of how that came about.

We have recently established selection criteria. And we are approaching the point of selecting material for advanced studies based on performance requirements and what is currently known about material performance and predicted container service environment.

Then there will be a continuing process of developing performance models, performing parametric testing. This will be conducted in parallel with the ongoing site characterizations which will gain additional information about the service environment.

There will be design activities to the detail, the container designs. There will also be design analysis and performance assessments and these will be conducted over a period of years.

And then prior to the final design phase which is referred to here as the license application design, we hope to confirm the material selection. There may be a decision to revise it at that point based on site data and the predicted performance of the container.

And the long term testing will continue on all the way out through the license application phase.

The question was asked when was there going to be a bottom line on the material selection for the materials. And there are competing desires in that.

One, we would like to make a decision on material as soon as possible so you can focus your research and your efforts on the testing and using that in the design activities.

But on the other hand, the actual service environment will not be completely detailed for many years. That brings out a complex process that turns into an iteration. We're picking, selecting material early on based on what we know, and later on confirming or revising that based on the final information inputs later in the program.

The process started many years ago. This is a viewgraph summary of the document which is referenced on the last page of this section.

A brief history of the container material candidate selection. Back in the early days of the project, the repository at Yucca Mountain, repository horizons both above and below the water table were being considered.

And if you're below the water table, then for accommodating the hydrostatic loads you would look at thick container sections. The containers are partially or completely self shielded, and you need a material which has a corrosion allowance for a long time period. So there was a lot of emphasis on carbon steel, cast iron.

Once the decision was made to locate the repository in the unsaturated zone, then the emphasis shifts to thinner section containers. They're lighter. For the same size container you get more capacity, internal capacity. And you can work with corrosion resistant materials.

The list of reference materials or candidate materials has been historically tied to the preparation of the site characterization plan. So that during an early draft of that it was proposed to use the bare pour canisters from the defense high level waste as the disposal container.

The decision had already been made at that time to use 304L stainless steel for the pour canister for the defense high level waste glass at Savannah River. So at that point with a desire to have a reference material as a starting point and to compare against, 304L was selected as a reference material.

Although you know that that's not necessarily the best material, it's a reasonable engineering choice at that point. So there was a continuing evaluation of additional materials.

There was a survey of various engineering metals and alloys. About 31 materials were surveyed including steels, copper.

Base materials, nickel base materials, titanium, at least one of the zirconium alloys. And it was narrowed down to 17 materials for a little more detailed study.

And then four equal selection criteria were applied, the mechanical performance, the corrosion performance, the cost and the weldability.

To try again to narrow the focus of the additional reseach as much as possible, four materials were picked out of one alloy family and that's the austenitic material. 304L was still a reference material, and then three higher performance alloys, 316L stainless steel, 321 stainless steel and Alloy 825, which is a nickel base alloy but it's still in the iron nickel austenite series. There is a reference report on that selection.

In 1984 the program was asked by Congress to reevaluate the copper alloys. And the program worked with the copper industry looking at five alloys. It was then narrowed down to three, CDA 102, 613 and 715.

And about this time it was also apparent that som of the thermal processing in forming the glass into the 304L was bringing up the possibility of sensitization, a

nucleation of sensitized microstructures.

And it was not a good idea and a viable concept to use that as a disposable container.

At that point we decided that whatever disposal container was used for spent fuel would be used as an overpack for the defense high level waste.

In the '84, '85, '86 time frame copper was studied in laboratory testing, surveys of existing information. And some feasibility reports were prepared. They did not show any real show stoppers.

About this time there was a material review board convened to examine the waste package container materials. They made a number of comments and those have been folded into the program and have helped direct it.

More recently as the site characterization plan was finalized the candidate list was narrowed to six, three austenitic materials. Alloy 321 was dropped because it was -- its performance overlapped 316L and Alloy 825. And three copper base materials. These were technically reviewed as part of the site characterization plan, technical review. And that list exists now.

This whole history is described more fully in a document which I believe the Board has been provided, the annotated history of the waste package container by McCright.

If there aren't any questions on that part, here are the austenitic candidate materials, 304L stainless, 316L stainless, and Alloy 825, and all of the iron, chrome, nickel austenitic material series.

Some of the properties of these Dan McCright will be discussing in his talk. Their mechanical properties in an annealed or hot roll condition are shown down here. They all have a reasonably good strength and ductility shown by the elongation number.

The copper based alloys, CDA 102 is unalloyed copper.

CDA 613 is a normally seven percent aluminum bronze with three and half percent, a few percent iron in it.

And CDA 715 is a 70/30 copper-nickel.

The mechanical properties for these, the tensile strength or the yield strength is a little lower, particularly in the case of the unalloyed copper. And that's one of the reasons why Will Clarke mentioned if we were going to use that material we'd probably use a thicker wall section.

For a point of reference, the conceptual design container analysis that was conducted a number of years ago in the conceptual design for retrieval loads, the greatest loads on the container would be during a retrieval where there had been rock fall so you're having

to pull this thing out against some restraint.

And the highest wall stresses in that analysis came out around 10,000 psi. So that number is in the ballpark with the largest stresses calculated for the current conceptual design.

I'm now going to discuss some of the selection criteria and the selection process. This is the same viewgraph shown by Will Clarke.

The criteria are material independent down to a certain level of detail. I'll point that out when we get into the details of the criteria.

At some point when you're measuring quantitative parameters on materials, that measurement becomes material dependent. One example of this is you use different measures of ductility for ceramic materials and metals alloys, for example.

But in the topic areas, the way the criteria are developed, they are material independent. They are derived from functional requirements on the container.

There is relative weighting factors for the different topic areas that have been established. There are a set of minimum requirements and these are intended to be points below which the container material would not be adequate for meeting the performance requirements of the waste package.

And there is an attempt to establish a quantitative score to allow comparison of candidate materials and this has been subjected to a formal peer review.

At this point let me say a few words about the selection process because the structure that you see here in the criteria is dependent upon the process.

There's a number of ways of trying to make a decision like on selected material and a number of different ways have been considered.

And the different criteria that you will be seeing, some of them are much more -- something you would -- property of material that you would like to have, and the more of it that it has the better such as corrosion resistance. And others are things that you really have to have, that have to be sufficiently strong to meet the handling requirements.

So we have decided to try a combined pass/fail and quantitative figure of merit. So those areas where there's an obvious passing minimum below which the material is not acceptable establish that and assess all of the materials against that minimum requirement.

And in those areas where there is a material property which beyond a minimum requirement gives you additional advantage, such as corrosion resistance, put a quantitative figure of merit on that to allow comparison

with the materials which are adequate.

DR. CARTER: Bill, I wonder if during the process this might not be the appropriate time, you might want to do it a little bit later, but I'd be quite interested if you would discuss with us the Material Review Board.

I'm particularly interested in its composition, the interactions with the working folks, procedures, expected longevity of the board, its responsibilities and perhaps its authorities.

DR. HALSEY: The Material Review Board was convened and had a finite lifetime and it issued a report which -- I don't have the reference with me. But it was not an ongoing, long term -- Bill -- Dan would you like to address that? Dan was --

DR. McCRIGHT: I'm trying to recall it. The Material Review Board was composed of, I believe, all university professors and I don't remember all of the --

DR. HALSEY: Use the mike, okay?

DR. McCRIGHT: The Materials Review Board, as best I recall, was convened to review not only the NNWSI project but also the SALT project at that time and the SALT project. And also to look at what we call the materials characterizations center.

I believe the composition was all university professors -- later I could give you the names -- their charter was to loook at the activities that were going on in each project to determine if they thought that the success of what they were doing would lead to a real demonstration of the regulation requirements that were in effect at that time.

They gave reports in 1985. We met with them again in 1986. The report indicated that they thought there were some serious deficiencies in the program, part of which was aimed at the materials that were being looked at and part of the approach that was being taken at that time.

I believe our meetings with them in '86 we rectified some of the points of disagreement we had with them. They did issue reports that are available, and I don't know if the Technical Review Board received that or not --

MR. CLONNIGER: Excuse me, Dan. Are we talking about the Ad Hoc Corrosion Panel?

DR. McCRIGHT: Yes.

MR. CLONNIGER: Not the Materials Review Board necessarily.

DR. McCRIGHT: That's part of the Materials Review Board, isn't it?

MR. CLONNIGER: Well, they were established by the board but they were a short term group and they were

called the Ad Hoc Corrosion Panel.

DR. CARTER: Well, I was particularly interested in the board as far as its history and, like I say, these points that I mentioned.

I guess the other question, sounds like to me that that board has gone out of business. Is that correct?

And I presume then the program does not feel a need for an independent material review board or someone in that -- or is there something else that's taken over this function?

DR. HALSEY: Within the program at Livermore, the independent peer review panel serves a similar function. We ask their opinion at certain milestone points, where, for example, the criteria are established, we ask their opinion when the selection is performed.

The Yucca Mountain project has the prerogative of conducting its own independent peer review and its own technical assessment of those activities at that point also.

DR. CARTER: I'm thinking particularly now of canister materials, if we can restrict it to that.

Is the peer review that you have, is that an ongoing group or is it set up on an ad hoc basis as needed?

DR. HALSEY: It's set up on an ongoing but we really only ask their opinion typically at decision points to review a block of work that has been completed and ask their opinion on that.

DR. CARTER: Okay. So that's initiated by the project itself, in essence?

DR. HALSEY: Yes.

In establishing criteria, again referring to the history of this, while the candidate material list was being developed, so was a list of criteria, what's important.

And I would refer the board to Section 8.3.5.9.2.1.1. of the site characterization plan where there's a discussion of preliminary selection criteria.

These bullets are just the section headings. There is additional discussion under each of these, it runs on for several pages, and it gives philosophical points that have been set down as the criteria for selecting material.

These were reviewed by the Material Review Board and there was quite a bit of discussion. They were actively involved in reviewing the program as these were being developed.

Briefly, what material will meet the

performance allocated to the container?

Can the performance of the material under repository conditions be adequately predicted?

Will the container material interact favorably with other components of the waste package and the near field environment such as the waste form, borehole liners, the rock?

Chemical material. Can a container be made of this material, the fabricabilities?

Are the container materials and process for fabricating it practicable and similar?

And how can additional confidence in the selection be gained?

You can see from this list that these are points of philosophy, not quantitative parameters.

So what we've tried to do is take these and other engineering requirements on the container material and establish detailed criteria which can be used for determining adequacy of the material and quantitative figure of merit for comparison of materials.

There is currently a draft of a selection criteria, not a final version, which was prepared at Livermore iterative process discussing with staff, consultants and contract technical personnel, and has been subjected to the peer review.

There are currently 34 separate criteria and they're divided into seven topic areas. I will be showing you the structure of the criteria in the next few

viewgraphs.

The criteria address the engineering, performance and regulatory requirements on the waste package and the waste package material.

Each criterion has a relative weighting factor versus all of the others.

And in most cases, the criteria have both a pass/fail and quantitative score. Some of the criteria, it's not really appropriate to have one or the other. There's either no true minimum requirement so the pass/fail judgment is not applicable, or once you have met the minimum requirements there's no additional benefit to be gained from a higher score, so you don't need a quantitative scoring.

We divided into seven topic areas, four of them performance related and three of them nonperformance related.

The first part is the mechanical performance.

The second part is the chemical performance which is actually the corrosion issues.

C is predictability of performance.

D is compatibility with other materials.

And then the nonperformance issues are fabrication, cost and previous engineering experience.

The current weighting factor division between these seven topic areas:

The corrosion performance is the largest one at 30 percent, and then the ability to predict the performance, fabrication and the chemical performance at 16 percent, 20 percent and 14 percent.

Compatibility at 10 percent. Cost and previous experience at five percent.

I will show now an example of how each of these areas is broken down into subtopics.

The corrosion resistance is broken down into topics of general corrosion which is the oxidation and general aqueous corrosion.

We get some pitting, crevice and localized attack.

Resistance to environmentally accelerated cracking such as stress corrosion cracking of various types and hydrogen embrittlement effects. And resistance to microbiologically influenced corrosion.

Now, some of these are then broken down further into individual criteria. For example, that 10 percent which was resistance to environmentally accelerated cracking is further broken down into individual criteria as listed here.

Stress intensity, susceptible phases, threshold potentials if they can be established, possibilities for smooth specimen stress corrosion cracking, concentration

of chemical species in the environment which will promote cracking.

And the same things for hydrogen, sufficient hydrogen concentration and formation of phases which are susceptible to hydrogen cracking.

So this is where you end up with 34 separate criteria. This the 10 percent which was environmentally accelerated cracking of the chemical performance portion.

I'm not going to try and go through all of the draft criteria. One, it gets long winded, and two, they are still draft and they are not finalized. But I thought the board might like to see an example of what the criteria looked like.

A fairly straightforward one is strength. We are using the yield strength as the parameter. And as I said from the conceptual design analysis, currently the passing score looks like around 10,000 psi.

And the goal is to assure adequate strength for both static and handling loads.

Other than in the most extreme retrieval scenarios, typical loads in the conceptual design are in the one, two or three thousand psi range, as seen here.

Another example would be resistance to general corrosion, both oxidation and aqueous corrosion. And at the moment we're looking at lumping all of these together

because the effects tend to interrelate.

And if you had separate criteria for oxidation and aqueous corrosion and at various times you have a wetted or nonwetted surface you now have two criteria which are strongly interdependent and that makes the selection process difficult. So they are currently lumped together.

And the parameter is our estimate of the time averaged oxidation rate with a passing score of one micron per year. That relates to on the average. And that relates to a one millimeter wall thinning during a thousand years.

And with conceptual designs of about one centimeter thick this is a 10 percent reduction in the wall due to the general oxidation.

One final example would be for a subcriteria in the resistance to environmentall accelerated cracking, a subcriterion to that is the threshold stress intensity for corrosion cracking with a parameter such as K1 over K1scc.

We've posed a passing score and then a scale so the further away from the threshold stress intensity for stress corrosion cracking that you get, the more likely you are to resist that phenomenon.

This goes on through at the moment 34 separate criteria.

DR. VERINK: Just for curiosity, how did you arrive at the weighting factors?

DR. HALSEY: Iteration through opinions of a variety of people, myself, other staff.

This has been through other people within the program at other organizations. We've had technical engineering consultants and contract support look at it and give us their opinion. And these numbers have varied.

The ones that you see here reflect some of the opinions of the independent peer review. I'll get to that in a minute. They suggested a modification of the number of the weighting factors, but not by very large amounts.

So this is an evolved consensus. Not everybody agrees.

DR. PRICE: Did you use a formal technique like a Delphi process?

MR. HALSEY: No.

This is as good a place as any to dicuss. Because things like the weighting factors and the scales, how you scale the quantitative weighting factors are subjective.

They are an opinion derived from discussions of a lot of people. Because of that and because in applying these criteria -- if you look at these criteria carefully, we are not going to have hard and fast data to nail down every one of these parameters, not at this point in the

program and not with the amount of testing that has been completed.

We will be able to quantify a number of them. And others we will be able to get pretty good numbers from existing literature that you will be hearing about, testing that we are doing from predicted models, from the current estimates of the expected environment and bonding environment.

But since the exploratory shaft and the site data have not been available yet and will not be for some time, some of these will be subjective opinions.

We think that this will be adequate. We think that the environment within these bonding ranges that you saw yesterday, the bounding range is what we're currently using for design purposes, this material will perform within the minimum pressure criteria.

DR. CARTER: By the way, as a matter of curiosity, how do you quality assure subjective matters?

DR. HALSEY: I'm getting to that. That's what

I'm leading up to.

DR. CARTER: Good.

S DR. HALSEY: And there is a process.

DR. PRICE: Before you get to that, I understand there's a draft of selection criteria document; is that correct?

DR. HALSEY: Yes, there is.

DR. PRICE: As an action item we'd like to request that.

DR. HALSEY: The way you assure the quality of something like that is through a process of independent peer review where you say what your opinion of the weighting factors is and why and you subject it to the examination of a board of independent technical experts and get their opinion also. And that's what we've done and will continue to do as part of the selection process.

The next viewgraph shows -- you may ask where is all the information going to go, or come from to go into these criteria. Here's just a breakdown of the seven topic areas and to the primary information source.

Some of it is from our own degradation mode surveys which you will hear about. Some of it is from the general body of engineering data. Things like mechanical properties of these alloys are available.

And, for example, engineering experience and industrial experience in fabrication, we have both fabrication studies and existing engineering data.

We are conducting cost studies, our own parametric testing which you will hear about this afternoon, and our own modeling studies which you also will hear about this afternoon.

One of the things that I want to indicate with this is that there is in most cases not a single piece of information that will go into one of these topic areas, but that you're going to use whatever information is available. Both information generated by us and information generated by us and information that we can gather from the existing engineering information.

The peer review process that we have gone through is select a chairman and members of the panel, to provide advance information, hold a meeting where we give them a briefing on the status of the program.

We train them to those portions of our quality assurance program which are pertinent to the peer review, show them the selection criteria, for example, and then allow them to discuss it.

And then we provided a support contractor to the chairman to assist in preparing a report. It took them a period of time, actually quite short, about six weeks or so. They prepared a report iterating between the members, and they did reach concurrent opinion, at least in the case of the selection criteria.

So we're right at this point now, there has been a draft report given to us by the peer review, and we are currently revising the draft criteria taking into consideration the panel comments.

So you asked about the draft of the criteria. They are currently being changed in response to the panel comments.

And the peer review panel asked if they would be allowed to view the changes that we make and comment upon them again, and the panel responding to the revisions of that has not happened at this point.

So both the criteria and the peer review report itself had one more step to go to completion.

DR. CARTER: Are these always consensus reports or do you have provisions from minority opinion?

DR. HALSEY: There is provision for minority opinion as provided in our quality assurance program.

DR. CARTER: And this is a formal process.

DR. HALSEY: It is a formal process. There are written requirements on peer review. We have a written procedure. We have tried to conduct our peer review -- well, it is conducted under our QA program and we attempted to make that program coincide with the NRC position on peer reviews for this program. And we think we've succeeded, although we haven't had their opinion on that.

The peer review panel that was convened for the selection criteria occurred in 1988. Revision of the criteria is in progress.

You had asked us to indicate where there were delays attributable to the quality assurance program. And in the process of revising the criteria in response to the peer review is when the new quality assurance program in the economic project was put into place.

So we suspended that work because it is QA Level 1 until the new QA program was in place and was qualified. Then subjected this to readiness review for the completion. And we now have authorization to complete the process. So there was a delay.

The members in the peer review panel, we were looking for expertise in materials, material corrosion, predictive modeling, fabrication, performance assessment, failure analysis and general nuclear engineering experience.

And we tried to get a viewpoint from more than one location. Academic, industrial, the utilities, people with a variety of backgrounds.

The composition of the peer review panel is shown here. Dr. Robin Jones was and is the chairman and he is at the Electric Power Research Institute and has been involved for many years in nuclear industry materials issues.

Dr. Geoff Egan at Aptech Engineer, again is an engineering consulting firm to the nuclear power industry on engineering and materials issues.

Martin Prager is chairman of Materials Properties Council.

Dr. Robert Long is vice president of GPU Nuclear Utility for financing and safety.

Dr. Rich Gangloff is professor of material science at University of Virginia.

And Dr. Roger Staehle is a consultant in corrosion and material performance. And after the peer review report was concluded, and he had concurred to it, Dr. Staehle resigned.

Their report again has not been finalized. The final step in their report has not been taken. And it is fairly lengthy so I'm trying to summarize their opinion. This is my summary of their response to four questions that were asked of them.

They didn't have to answer these questions. They were convened to give us their opinion. But these four questions were asked and they responded to them.

One: Is this type of commparison of materials and this type of selection process a reasonable thing to attempt?

And the summary response: Yes, you have to be careful, as in any kind of selection like this on complex criteria.

Two: Are the criteria topics and the parameters reasonable?

And for the most part they agreed yes. They recommended a few changes in some of the parameters.

Three: Have we left anything important out?

And they suggested a few things be added in. The criteria are being revised to consider those.

Four: And are the weighting factors and quantitative scales reasonable? That's a leading question.

Of course, they had a slightly different opinion on those issues. But it turned out they weren't widely different. They suggested a number of small changes in both the weighting factors and the quantitative scales.

The peer review panel continues. We will show them the final version of the criteria. And we also intend to use this panel, perhaps with some additional personnel, in the final selection when we make it.

So that's some discussion of the selection criteria, discussion of the selection process that we will be conducting. And what we are trying to come up with is part of the material for advanced study.

Are there any questions?

The very last viewgraph is a breakdown of the selection criteria showing all 34 points, and I don't

think I'll try and go through this, it's a little hard to read. It's included in the package so that you can see how they are currently structured.

DR. CARTER: You mentioned earlier a figure of merit. How do you go about composing that or constructing it?

DR. HALSEY: Okay.

DR. CARTER: And how do you use them?

DR. HALSEY: Well, I said the selection process really has two parts. One is on those parameters where we can identify a minimum passing level, the materials will be given a pass/fail score. And any material that fails any of the minimum passing scores will be eliminated. That means it's not adequate for use as a container material.

All the materials which pass all of the minimum requirements will then be compared using quantitative figure of merit. And that is obtained by multiplying the quantitative score on each parameter for that material times the weighting factor for that parameter to come up with a numerical figure of merit for that parameter.

Adding all those together for the quantified criteria will give the figure of merit for that material. And then it's just simply a matter of which one has the highest figure of merit.

DR. CARTER: Okay. So that number is essentially the sum of a series of products.

DR. HALSEY: Yes.

DR. CARTER: Okay.

(Recess Taken.)

MR. CLARKE: Okay. To carry on this morning's session on the container materials, one thing that I want to mention before we pass over too far that I neglected to say in the opening, and that is in relation to the QA control.

Argonne is conducting the program to a RQAP, they're under the umbrella of RQAP. But they also have their own plan in place that we have audited and it does comply with the '88-'89 Reg 2 requirements. So that work is being done and is being upgraded to Level 2.

Also the selection process, as you just heard, was a Level 1 activity. The modeling and testing that you will hear this afternoon is a Level 2. And for our purposes we do not distinguish between a Level 1 and a Level 2 in the way that we conduct our business. They are the same.

The surveys that you are now about to hear from Dan McCright were conducted previously under the previous QA plan and that was a Level 3 activity. Okay.

With that I'd like to give you Dan McCright.

DR. McCRIGHT: Thanks very much, Will.

I'm going to be talking about the performance of candidate materials and put the emphasis on the degradation mode surveys.

I'm a metallurgical engineer and I'm also serving as a task leader for the metal barrier selection and testing task.

Okay. If I can have the first slide please.

What I'm going to try to do is to break my talk into three parts. And in the first part I'm going to talk about the background of the degradation mode surveys, why we did them and how we did them. I'd like to take about 20 minutes on that one.

In Part C which is jumping ahead a bit, the comparison and analysis, I'd like to take about 15 minutes there.

The part in between which is the highlights of the degradation mode surveys, to go through, walk you through, volume by volume, I'd like to be very flexible on the time and really it's the board's wish how much detail you'd like me to go into.

I'm planning to spend about 40 minutes there, but I certainly don't want to talk into lunch. So if it gets too long, I would be glad to terminate it or certainly to compress it.

So if I could go on to Part A on the background.

Okay. Just from the regulations in 10CFR60 there is a section that says that the design shall include but not be limited to consideration of the following factors, and then the factors are listed.

And again, the emphasis of the talk today and the emphasis on the long term performance of the candidate materials is going to be obviously the corrosion behavior of them. But virtually every other item that's listed on there influences the corrosion behavior of the material. Oxidation/reduction reaction, solubility of species, hydriding, hydrogen that's produced by electrochemical production, by radiolytic production, gas generation -- Rich talked some about that earlier -- thermal effects, mechanical properties, radiolysis and so forth.

And you've heard some before and you'll hear some later today. But all these factors are important and they all interact with one another.

Next slide, please.

Generally, we wrote the Yucca Mountain project and site characterization plan Information Needs 1.4.2 which is talking about the materials properties, and it's right from there we talked about the degradation mode surveys and why we would do that.

The historical development of them was that the first volume was started in 1987, early part of 1987. And we did the draft of the phase stability, hydrogen effects and oxidation and general corrosion in the earlier part of 1987. And they were completed at the end of that year.

Then the volumes on localized corrosion, stress corrosion, the lengthier volumes I might add, were begun in late 1987.

And then it became apparent with the concomitant surveys being performed by Babcock and Wilcox on fabrication and welding processes that there was so much interaction between the welding processes and ultimate performance concerns of the welding regions that we thought that that area alone deserved some special treatment. So we added volumes to deal with the welding effects in the spring of 1988.

Drafts of all the volumes were completed in June of 1988, and because of the length and the breadth of the subject an overview was written to go along with that.

Drafts were made available for the selection criteria peer review panel that Bill Halsey described in his -- in his talk, so that they could see when they met with us in September of 1988 where we stood on the degradation mode surveys and analyses.

And during the rest of 1988 and 1989 these reports underwent a very thorough review. All of our reports undergo a review of this caliber, but because of the length and the subject matter and the importance of these, the review was very extensive and it did take quite a bit of time.

They underwent technical review, policy review and QA review both at Livermore and at the project office in Las Vegas.

And finally, the eight-volume set was approved in November of 1989, and advance copies were sent to the technical review board panel members.

Okay. If I could have the next slide, please.

What I would like to do is to walk people around, just assume we've got a vertically in place container in Yucca Mountain, and just to look at some of the degradation features, degradation modes that could happen over the long term containment period and beyond.

And for the purposes of this illustration I made some assumptions on how the container might be fabricated, but there are many other processes that were being considered.

I've also made some assumptions on the configuration as far as how it would be in place with respect to shield plugs and borehole liners and other items that would be in and around the waste package. And it's a schematic, it's not drawn to scale.

I'd like to point out I'm doing this more from a pedagogical point of view so I have a lot of material to talk from. But certainly from an engineering point of view, by proper design of the waste package and the waste package peripherals and the choice of materials, we would try to minimize many of these problems, either try to eliminate them or certainly to mitigate those problems.

So if we could have the first slide, what I'd like to do is consider primarily the metallurgical and mechanical considerations.

And first of all, let's assume that this waste package container was made by a very conventional process from rolled and welded plate. And that would be the body of the container.

And then the two end pieces with the bottom and top might be fabricated, say, from a forging.

From these materials there would be a seam weld and a circumferential weld, to build what I would call the assembly weld to get this into a body. And then at the facility that would be located at Yucca Mountain, this would be filled with the, say, spent fuel rods, or if it were a glass waste package with the pour canisters containing the glass would be then placed inside.

Then a closure weld would be performed at that surface facility and then the waste package taken from that facility and placed in the borehole in the proper horizon in Yucca Mountain.

But during this processing, because we would have had different study materials for the body and for the end pieces, there may be differences in composition and the microstructure.

Also during the welding process, it's ordinary, it's assumed -- again we've done this with a fusion process and there's been a filler metal, and that there would be compositional differences between the filler metal, possible compositional difference between the filler metal and the base metal.

Also there would be in effect in the microstructural in the heat affected zone around the welds. And perhaps some of the welds haven't been made quite properly and there's undercutting at the potential crevice site.

During the handling operation we may have dents and scratches and other surface blemishes that were imparted into the container.

For instance, in making the closure weld, perhaps it's not possible to fully stress relieve that and so some of the welds would be left at the yield stress.

After the package has been placed into the repository during the long thermal period with moderately

elevated temperatures, there could be long term aging effect and possible phase transformation occurring in the metal.

Also, perhaps, there's an upset condition that the waste package, which is initially surrounded with an air gap separated from the the other metalic, which is the borehole liner, that perhaps during the course of events in the repository, that there's some shifting and there's another contact between those metals and if they are dissimilar metals there can be a galvanic corrosion problem there.

Let's move on to the next slide which is dealing with the chemical and environmental considerations.

For instance, we have a shield plug here for radiation protection amd a borehole liner. These will be cooler objects than the container, so that moisture from the moist air environment around them may condense on these surfaces and then as it condenses and forms droplets then these can drop onto the body of the container.

Well, in so doing they may contain a very dilute concentration of electrolyte but, repeated droppings of this kind can cause a buildup of electrolyte on the container surface causing a concentration of the electrolytes.

Also, because this water may have been in contact

with other elements around the waste package, there could be some chemical modification of that because of the differences of materials, which were talked about yesterday, there's quite a range of possible chemical environments that could take place.

We could also have perhaps something happens to borehole walls it was nice and smooth at the beginning but rock slides occur at a very local level and we have rock fall against a container creating again, now, another crevice site and again a loss of the air gap and with a possible collection of moisture in that area.

As we talked yesterday in some of the geological presentations about fracture-matrix flow we had a condition of fracture flow, water coming to the near vicinity of the waste package. And then if the drainage is inadequate there can get an accumulation of water there with temperatures such that they are below the boiling point of water.

Over a long period of time, for instance, there may be microbiological activity, as was mentioned earlier, which could be the inherent microbiological action that's present at Yucca Mountain, or maybe more probably that which would be introduced during the drilling and operational activities, again and intervention into the repository environment.

We also have with us container rest against some sort of support plate, there would be another location of a crevice site.

We also, as Rich talked earlier, we have gamma radiation from the decaying waste which causes changes in the chemical environment.

One other item is that for the most part we consider the environment and corrosion effects on the outside. It's also possible that there may be some effects from the side.

There is some spent fuel because of its reactor history which is waterlogged, and if this waterlogged spent fuel is put inside the container it creates a humid internal environmental, there of course would be gamma radiation plus chemical changes in that, and because this may be an inert gaseous environment there would be a different kind of oxidation behavior inside than there would be outside.

So, in other words, this container material is going to be exposed or it has the potential of being exposed to a very wide variety of physical, chemical, mechanical and metallurgical conditions. And that's the point I would like to make with these two slides.

And that's the basis that we tried to conduct our degradation mode survey, that we would try to survey as

much as possible that wide range of possible conditions.

Can we go on to the next side, please.

Again the methodology that we used, we used computer searches where they were available on data bases, particularly the Metadex and the Chem Abstracts data bases.

We found review articles on many subjects. For instance, NACE has produced a number of volumes on specific corrosion topics like localized corrosion, stress corrosion and so on. And those are very, very good sources to begin a literature survey.

And if one obtains a review article, that in turn references earlier work and so we obtain those references.

Then there are ongoing corrosion conferences. There's the annual NACE conference among others.

ASTM from time to time puts on corrosion conferences usually related to very specific subjects.

The annual NACE conference covers a wide variety of corrosion topics.

But anyway, there are publications that result from these conferences and special topics, ASTM Special Technical Publication series.

Then we review the current literature, such as journals, these appear monthly or quarterly, and I've listed here some of the journals that we particularly consulted.

And in some cases, of course, these are so new that they're not in the data bases yet. On the other hand, there are some general articles that were published before the 1960s when the computerized indexes became available. And so we have to go back with them often using manual methods to search for articles of interest in selected journals.

Then in a few cases we have some work that was sponsored by NNWSI, work that was done previous to 1986. Because it's published we often use that in our degradation mode survey.

Next.

And again we review this published technical literature, compile the existing data on the candidate material of interest, and we try to interpret this data in light of the conditions that we thought would be expected and the bounding case that has a wide variety of possible conditions in Yucca Mountain.

This resulted in an eight-volume publication and an overview. And then finally when it was typset it was 400 typeset pages. During the earlier draft it was a lot longer than that because this is a very condensed way of doing it. But there were more than 1,200 references that were cited in this.

I might also add from the QA point of view in their archives they want all 1200 references, hard copies of this, so that was no small task to get them all together.

The surveys will be used, of course, as Bill talked out, in material selection. As Joe will be talking about later, when we do the degradation mode surveys along with that came a lot of information relating to the mechanisms and model development and so on. And it was largely on that basis that he began a lot of his modeling activity.

And so in surveying the literature, of course, we found out what things we don't know, the pieces of information we'd like to know about the candidate material, and particularly to apply them to Yucca Mountain.

So this became the basis for a lot of our planning activities on what kind of experiments we should run to generate what we call the parametric studies. In other words, what range of chlorides, what range of pH, what range of different chemical, physical parameters did we need to do. And particularly how do we compare alloys from different families. That isn't often so evident from the literature.

Next slide, please, Liz.

And again, here the listing of the eight volumes. The way we went about organizing this was again dependent

a little bit on the way the information was found in the literature.

Some combinations were just natural. For instance, many of the mechanisms for localized corrosion were also applicable to stress corrosion. According to some researchers, the localized attack precedes stress corrosion cracking. So in those cases it was logical to group those kinds of modes.

In some volumes we just talked about one family of alloys. In others, for instance in Volume 1 and 2 we -- volumes, they deal with both alloy systems, usually in Part A, say the copper based materials, and Part B would be the austenitic materials.

And now I'm ready to go into Part B of my talk, and the way I'd like to do this is I'd like to dwell a bit on the phase stability, because the phase stability is very important in all the welding effects and also it affects some of the corrosion behavior, and spend some time on localized and stress corrosion, because those are the kinds of corrosion that are generally more performance limiting than the other forms.

Also, it's often a difficult job to try to present a large body of information like this in sort of capsulized form. What I'm going to try to do is whenever possible, is to choose examples where there's a comparison between the different alloys.

And again I will try my best to explain as I go along -- we tend to go from one analysis into another, from one chemical medium to another. But again, if there's something unclear please stop me and I will try my best to explain the details of it.

We want to go, Volume 1, phase stability and I'll begin with the copper.

First of all, why we would be interested in the phase stability and its analysis and to identify the phase transformations and then to, after we've identified them, what would be the effects of those phase transformations on the mechanical properties and on the corrosion properties of the alloys.

For the copper-based materials. For the oxygen-free high conductivity copper, it's a very simple material, single phase, pure element, essentially, fcc metal.

There is in a way a disadvantage because it is oxygen free or there's been such an effort done in the processing of this metal to take all the oxygen out, you might say that the material is very hungry for oxygen.

And so during high temperature processing, and particularly in welding, if there's any oxygen present, even very small amounts, this material will absorb oxygen,

and internal oxidation could be a long term physical metallurgy property problem with this material.

And because of this, this is why we recommended that we also look at some of the different deoxidized grades of copper, particularly phosphorus-deoxidized alloy, that's perhaps the most widely used and we have the most information on it.

The other copper-based candidate material, the aluminum bronze is a single phase material but with iron precipitates in it. In other words, the bulk phase is single, it's bulk material with single phase.

The iron precipitates help to harden the alloy system. There's a very small tin addition on the order of like .2 to .5 percent, and this improves a lot of the intergranular behavior of this material, a very important addition.

So because you have such a wide galvanic difference between copper and aluminum, dealloying is always a concern with this material.

With the copper-nickel alloy, for an alloy this is probably one of the simplest alloy systems possible, it's a solid solution of nickel and copper.

At the nickel rich end there is the possibility of having miscibility gaps, but to the best of my knowledge this is not really a limiting case either from a corrosion

point of view or from a mechanical point of view.

On the next slide I do have a diagram of the copper-nickel phase diagram. And as you can it is a single phase in the solid region. The alloy that is of interest to us is the approximately 70 copper/30 nickel alloy because here it is all single phase. And it is a little bit hard to say if that is going to be the case right down to room temperature.

What often happens is, of course, it's very difficult to have equilibrium at very low temperatures because of the diffusion rates are so slow, so small, so one tends to retain the higher temperature structure to lower temperatures.

When we get into the alternate discussion a little bit later, where there is an alloy Monel 400 that may be of interest, it's over in this part of the diagram.

Next slide shows the copper aluminum phase diagram. This is a bit more complex than the one you saw earlier. And the alloy range of interest here is approximately seven to nine percent aluminum.

So in some cases there is a very high temperature beta phase present, and depending on the particular process and often during the welding operation there would be some retention of the beta phase at lower temperatures.

And the significance of this is that the beta phase will be much richer in aluminum than the alpha phase. So there is a built-in, what you might say a built-in galvanic cell in that case, because you're getting the widely electrochemical difference between copper and aluminum.

Now, I'd like to go on to the austenite materials. These are iron-nickel-chromium alloys.

And just as background material, iron undergoes a phase transformation. At room temperature it's a body center cubic material which we call the alpha phase, ferrite, and at approximately 900 degrees centigrade it transforms to austenite, which is signified usually with a gamma in the phase diagram.

And this is a face center cubic material. And at even high temperatures of 1,400 degrees or so it transforms back to ferrite, it's often signified by a delta ferrite at the high temperature part.

But, anyway, in the metallurgy of stainless steel, one is always trying to balance the ferrite forming elements with the austenite forming elements.

Chromium is added. And of course, chromium is the element that gives stainless steel the stainless character. Chromium is also a body center cubic, so it tends to stay with the ferrite, the body center cubic phase.

Nickel is added to make the material austenitic, in other words to bring the gamma field down to room temperature stability. And again, nickel in conjunction with the chromium even improves the overall corrosion resistance all the more.

So in other words, there's quite a balancing act which goes on in making stainless steel without having the proportionate body center cubic and face center cubic alloy additions.

As it turns out, the 304L and 316L, these are metastable alloys. They would have a tendency to transform some of the austenite back to ferrite and to a another metastable phase called martensite at lower temperatures.

Because of the alloy composition of 825 it has an exceedingly large amount of nickel in it and the austenite is stable at all temperatures.

These materials, stainless steel and stainless alloys have carbon in them, the carbon again results from the initial formation of iron from pig iron, from the ore, and so much of steel making is the control and to take out a lot of the carbon.

But the carbon is there, and it is a useful element and it does give some strength, but it can also cause a number of corrosion problems.

And that is because the carbon would like to combine with particularly the chromium and precipitate out usually at grain boundary. In other words to take the chromium out of the alloy it takes -- the local chromium is temporarily depleted and we have a sensitized structure in that area.

And then because we have the bulk of the alloy so rich in chromium with grain boundaries poor in chromium we could have most of the corrosion occurring at the grain boundaries and this can result in intergranular attack and with stresses there, and also the intergranular stress corrosion cracking.

So the composition is very important in whether a material sensitizes or not. The thermal history is very important. Generally the lower the carbon the more resistant the material is to sensitization.

And there is also a problem of perhaps it may not sensitize during the initial cooldown from say the processing operation and from the welding operation nuclei form over a long period of time it may sensitize due to the low temperature sensitization.

There are also, and we'll talk about it a little bit later, there are some intermetallic phases that can form in austenitic materials and these can degrade the mechanical and corrosion properties.

In other words, the austenitic materials from a physical metallurgic point of view are a more complicated material.

This is a ternary phase diagram. And again, one has to take some liberties and some simplification when trying to show an alloy on a relatively simple plot like this, because commercial alloys have many, many components and you just would run out of dimensions to try to show them all.

But anyway, this is an attempt to give you somewhat of a map of where the different alloys of interest lie.

825 is in the gamma field, the austenite region, and it's significant in that it's well removed from all the boundaries.

304L and 316L and all the other candidates are near the boundaries.

This was an isothermal section at 650 degrees centigrade. If we went to the lower temperatures we would find that these are indeed located over in the two-phase region.

Again, the reason why we tend to use this is because the phenomenon I indicated earlier, because of the cooling down operation. One tends to keep the higher temperature phases to lower temperatures because when one gets to very temperatures the diffusion is so slow, for all intents and purposes, this is the structure which will prevail.

This also shows an alloy 625 that's often used as a weld filler material for 825. And again it's significant to point out that it's in a region of where there would be some phase transformation possible.

Then this is a quality phase diagram taken at room temperature. And I'd like to point out to you, again coming back to the alloy 825, which is approximately 40 percent nickel and 20 percent chromium, that we were indeed in the nature of stable austenite at room temperature.

But for the 304L and 316L material, which would fall roughly in here, with eight to 12 percent nickel and 16 to 20 percent chromium, that we would be in this area right here where indeed some of the austenite would have a tendency to transform to some martensite or to ferrite.

And the significance of that is that the ferrite phase is generally more prone to different kinds of corrosion, it's more prone toward hydrogen effect.

And martensite is the same way.

This also just an idea that sometimes these things aren't necessarily bad but they're more complex and there's more analysis to be done with these kinds of materials.

There are also some intermetallic phases in the austenitic alloys. There, sigma phases forms at approximately 50 percent chromium in an iron-chromium simple back binary.

Other elements that are added to stainless steel, molybdenum in particular, are also favored sigma phase.

The significance of sigma phase is it is a very brittle phase, something like two percent sigma in a structure reduces the factor toughness by more than a factor of two.

In fact, this is one of the technological limiting cases. Chromium is very beneficial from a corrosion point of view. But we don't have any alloys of commercial significance above about 35 percent chromium because of sigma phase formation.

Also it can cause intergranular corrosion because it's got the difference in the electrochemical behavior of the sigma phase versus the parent phase.

And some other element also influential in promoting sigma phase.

Molybdenum is very important in preventing sigma phase. And sigma phase has been identified in some pieces of 316L stainless steel. Its presence has been speculated in some pieces of 825, but from the results of our survey it's not conclusive whether sigma phase could form in 825.

There's also a chi phase that is in many respects similar to the sigma phase. It's again favored by high chromium, high molybdenum contents in the alloy can also cause intergranular embrittlement.

There's a laves phase which is again favored by molybdenum addition, and that can cause an intergranular embrittlement.

As I said earlier, carbon is a necessary ingredient in austenitic materials. It can form as carbides on grain boundaries.

And because the carbide is rich in M which in most cases is chromium rich, a lot of chromium is taken out of the solid solution, the chromium then imparts to the *stainless character to the material. So in those areas we have an area that's more prone to corrosion cracks particularly along grain boundaries where the carbide is most likely to form.

This is very dependent on the alloy composition and the history, it primarily occurs in the 500 to 800 degree centigrade range. So it's real important often in weld cooldown of the time that is spent in that temperature range.

Generally the low carbon grades are susceptible. It's also possible to add other elements, what we call stabilizing elements. This is usually also a very strong

carbide forming element, forming other kind of carbide and you need some of the chromium from combining with carbide so it stays in solution.

And again, if one were to discuss the relative resistances to intergranular attack and intergranular stress corrosion cracking, 825 is the least susceptible, it's the most resistant and is in the order in which they would fall.

As we said earlier, there's a possibility of low temperature sensitization, but again in very low carbon materials, this appears that by some annalysis that has been done, both in the nuclear power industry and elsewhere, that it just doesn't seem to occur, at least at the temperature ranges that we would be interested in and even at very long periods of time.

It's also possible because of the initial chromium depletion given the long soak times that we have at modestly elevated temperatures so we can have chromium redifusion back into those deplete regions, in other words you have a natural healing effect.

Next slide.

This shows the microstructures of Alloy 825, again what one might call the optimum microstructure an alloy given the stabilization treatment at possibility of 900 degrees centigrade for many hours. I don't recall exactly how many.

But this in other words, was to favor the titanium carbide precipitation. Alloy 825 has an addition of approximately one percent titanium. And that forms carbide.

It does form some chromium carbide if one does the appropriate ASTM steps to try to reveal if there's a susceptibility of intergranular attack, but it appears that this alloy is very, very highly resistant.

Now, I'd like to go into the general corrosion and oxidation of the two candidate alloy systems. And I'll begin again with the copper based material.

This is some long term atmospheric corrosion data taken from different sites in the world, different kinds of atmosphere.

As we talked earlier, particularly in Rich's presentation about the protective natures of different copper corrosion products, for instance with a marine humid atmosphere, we have a protective copper, basic copper chloride.

In an industrial atmosphere, Pennsylvania, we would likely have a basic copper sulfate material that was formed.

These tend to form protective films on the copper, again the corrosion rates of copper are relatively high, the general corrosion rate, compared to some of the other candidate material.

But if one were to use this, this certainly is a benchmark point here. If these conditions were to prevail over the whole full thousand years this would be a penetration that would result in a millimeter of material lost, so it would be a tenth of the wall thickness if we were assuming a one centimeter thick container.

What we said earlier was that particularly in a radiation environment, one of the key analogs is whether the basic copper nitrate that forms under certain radiolyte conditions, whether that would have some of the same protective qualities that these other kind of basic copper corrosion products have.

This is some material, some information that we obtained under NNWSI auspices some years ago with the work that was done both at Livermore and at Westinghouse Hanford.

And this gives some comparison, comparative as possible between irradiated and nonirradiated conditions. So if we group the two together, for instance here's the J-13 immersion and J-13 water, again one can see that in this case the irradiation is not too much different from the normal nonirradiated condition.

But in the theory where we have condensed steam

that forms on the surface, where again we have the greatest possibility of having some of the NOx's plus hydrogen peroxide and other materials, other species that are very, very oxidizing toward copper, we see that the irradiated substance does produce quite striking increases in the corrosion rate. And particularly for the copper-nickel alloy which in many cases is some of the more resistant in the nonirradiated condition.

The comparable results from the austenitic materials, again first of all, the index is much, much lower here. This would be a hundredth of the wall thickness loss if these conditions were to prevail over the full thousand year period.

But, as I think most people know, stainless types of materials are very, very resistant to oxidation, general corrosion, and even to radiation induced corrosion -- if you remember the 825 coupon which showed that in an intense gamma radiation environment at Argonne National Laboratory.

Again the rates are very, very small. And radiation has hardly any effect on the general corrosion rates or oxidation rates.

I'd like to switch now into the localized corrosion and stress corrosion cracking of the austenitic alloys.

In some respects this slide says it all with regard

to the localized corrosion attack of these materials in an aggressive environment.

This environment is ferric chloride. Ferric chloride is the medium that's often used in some standardized ASTM tests to show the differences in these families of materials.

It's very aggressive, high chloride content. It gives acidic hydrolysis, ferric iron is, of course, very oxidizing so it's quite aggressive.

But this shows that the molybdenum conditions of the alloy is very important when determining what the resistance to localized corrosion attacks will be.

The Alloy 825 has some resistance to localized corrosion but not complete. This is again a very aggressive environment. Alloys with much higher molybdenum content are much more resistant.

However, there are some disadvantages with these alloys that I'll try to point out later.

When you were visiting the laboratory yesterday you saw our work on generating polarization curves. And one of the uses of polarization curves is to predict the so-called breakdown potential where the passive film has been protective of the material you have in here to give us a low corrosion rate. Then something breaks down and bare metal is exposed to the solution and then the

corrosion rate goes up considerably.

And, again, if one were to arrange these, one finds that the Type 304 stainless steel starts to break down at much lower potential than the more highly alloyed material.

And, in general, the more highly alloyed offering material as with nickel molybdenum, the greater the resistance is to localized corrosion attack, both pitting corrosion and crevice corrosion.

Next slide, please.

Again, this shows the same kind of data but in a different way. If one were to compare the corrosion potentials and then the breakdown potentials of the two materials, and then what's most significant is the differences between the critical potential and the corrosion potential for Alloy 825 under these conditions in sea water, low temperature, is much more resistant than the 316 stainless steel.

Okay. We go on.

This shows, again, the same kinds of indexing comparing critical potential to the corrosion potential, and the greater that distance the more resistant the material is.

The significance here, the SO subscript 2 saturated sea water is again, we're getting to an acid chloride

solution which is very harsh for most metallic materials.

SO subscript 2 may be introduced into the repository environment by some of the operations that would be going on during the drilling operation over parts of the repository, also some of the microbiological entities could produce SO subscript 2 because many of the metabolism products of microbiological activity deals with the various oxidation states of sulfur in reducing and oxidizing sulfur. So often SO subscript 2 is the metabolism product that would be formed. But again, under acidic chloride conditions, again one sees with the 825 it's more resistant than the stainless steels and, of course, the 625 is often used as the weld material for joining 825.

This is a curve that many corrosion people have called one of the so-called classic curves. This was developed by Copson at Internation Nickel, and I believe starting in the 1940s.

But one of the great technological limitations of austenitic stainless steels is their susceptibility to chloride induced stress corrosion cracking, usually results in a transgranular propagation of the crack.

And it's often because of this susceptibility, the susceptibility is greater, the curve is the lowest, it just at the range that most of the 300 stainless steels of

composition nickel lie.

One could solve that problem in two ways. One can either have an alloy with no nickel in it or one can have an alloy with a lot of nickel in it.

On the basis of this curve, this has many of the alloys like 825 or above, particularly with 42 percent nickel or so. But this is the region where it's -- I hate to use the word immune, but it's certainly very highly resistant to cracking and as well as magnesium chloride.

Magnesium chloride, just like the FECL subscript 3 that I talked about earlier, is again one of these ASTM tests that have been developed over a long period of time as a basis for comparing different kinds of stainless steels, different stainless alloys.

And so for instance, when a new stainless steel is developed, one of the first things one does is to test it in boiling magnesium chloride to see how it fares compared to some of the older grades of material.

I'd like to go on to the next slide, please.

This shows the same kind of data that's now collected for various alloys.

A point I'd like to make on this slide, it came from the lab discussion yesterday, and again, it leads a lot in the modeling and even into some philosophical discussion.

It's very common for one in the corrosion engineering point of view to consider different thresholds. And I'm only using this because one must argue that, well, if we keep the stress down low enough the material will never crack.

Well, this is the kind of question that has to be examined in a project like this, particularly where we're trying to make these long term projections, because what if thresholds do indeed really exist.

And Joe is going to say a few words about that later, about the modeling that have been developed where essentially they just predict a longer and longer and longer incubation times.

Because one can only see a little bit from the flavor of this curve whether that becomes really, really straight or whether there's still some slope to that curve that even after a very, very, very long time that we would -- at very low stress values we would intercept that curve. And even under those conditions we would have failure.

By comparison, 825 isn't shown here because again sometimes 825 cracks and sometimes it doesn't. It depends very, very markedly on small variations in the compositional differences and how it's processed. But it would presumably be way up there somewhat off the curve.

And just to show that magnesium chloride as we said earlier is somewhat the chosen test review because it is so agressive. This shows the relative corrosivity of other chlorides. Magnesium is more corrosive than calcium chloride.

And again, one can interpret this from the point of view that magnesium chloride generally gives an acid hydrolysis more than calcium chloride.

Another bad actor is ferric chloride as we mentioned earlier in promoting local corrosion and also it promotes stress corrosion cracking, and again because it is an aggressive medium.

The significance in the repository is that if one were to build the borehole liner and the shield plug and so on out of carbon steel, and corrosion products that form on the carbon steel and the chlorides are present, if one can perhaps generate scenarios of getting ferric chloride to the surface of the container.

This is a comparative study, again really quite short term data with the candidates of interest. And again, it's an acid chloride medium. And again, one sees no cracking of the stainless steel -- of any of the materials in 10 days, but then under the 30 days all the stainless steels cracked and the 825 did not crack.

A relationship that's been developed over many, many years, and which seems to occur over repeatedly sometimes at different levels here, is the relationship between chloride and oxygen.

And this one shows that almost in another extreme case it goes back to work done in the 1950s on boiler water with an alkaline phosphate treatment, which shows that somewhat at very low concentrations of chloride or oxygen that there's a synergistic relationship between those materials that fail under stress corrosion cracking and when there is no failure.

So what you see is the threshold in these cases is quite low. And in our case where we do have a chloride content with J-13 water in this region, and then as we talked earlier in some of the clear water it can be somewhat higher.

But we do have oxidizing conditions prevailing, either oxygen present in the atmosphere or there's nitrate present in the water which is an oxidizing species of its own, some of the radiation effects can create even additional oxidizing species.

So in other words, one would predict, and we have done some work here and at Pacific Northwest Laboratory, when one takes a 304L, a 304 or 304L stainless steel for that matter, and puts it into J-13 water in a highly irradiated environment, it does undergo stress corrosion cracking and has transgranular end morphology and that suggests that it's a chloride induced kind of failure.

This is work that was done by the Japanese again with 304L stainless steel. And where there's no irradiation present, in other words the oxygen content or the oxidizing power would be lower, then again when irradiation is introduced the oxidizing power goes way up, the material becomes much more sensitive, much more susceptible to stress corrosion cracking.

This again is work that was done in support of the nuclear power industry for 304 piping, work done at General Electric in Schenectady, New York. But again, this is sort of a map of different degradation modes.

But when the material is more highly sensitized, the failure tends to be intergranular. If the material is not heavily sensitized, then the material tends to be transgranular. And in between we have a granulated, a mixed mode, there's some transgranular and some intergranular effect.

Also you'll notice the significance of the strain rate as you saw on the lab tour earlier, we are doing or planning to do some slow strain rate tests of the material.

And these are just some of the relatively low strain rates -- if one wants the high strain rates, they have a ductile failure which is not usually related to anything of other corrosion significance -- this is the low strain rates, which again we would expect with a very statically loaded item that we had, and that would be this area of significance.

Now, I believe I'm going to go on to stress corrosion cracking in copper base alloys.

And as we alluded to yesterday, it's the chloride in the austenitic materials which is probably the number one causitive, number one stress corroding positive species. It's ammonia that is the major culprit that would affect copper base materials, and in some cases other nitrogen bearing species can also be induced to stress corrosion cracking.

This shows a section of a potential pH diagram. This indicates that ammoniated copper complex becomes stable at 1 pH range and approximately seven, also at a higher pH range.

But the one that is of interest to us is probably this one over here. When the complex becomes stable, this is at the expense of what would have been the protective cuprous oxide film. And in that case when you have an unstable pass of film on the metal surface that's a condition that often leads to stress corrosion cracking.

And indeed, many copper alloys, and I'd almost say that virtually all of them are susceptible to stress corrosion cracking in ammoniated environments, some more, some do have some degree of resistance, but it is a very devastating species to the copper based material.

Some work that was done to support the Swedish nuclear program, work that was done at the University of Newcastle-upon-Tyne in England. It shows that the other nitrogen bearing species may be significant.

And these are some slow rate, slow strain rate test specimens of high purity copper to nitrite solutions of varying concentrations shown here. Again nitrate and nitrite ion concentration increases the susceptibility also increases.

Again the significance of nitrite would be the J-13 water contains nitrate, and almost invariably if one introduces a metal into a nitrate environment there will be redox reactions occurring on the metal surface to produce nitrite.

And as Rich talked earlier, there are certain radiolysis conditions that can lead to nitrite formation in an aqueous environment.

And according to some people, the nitrite itself isn't so much the causative agent but nitrite during the action again of reducing agent like copper or other metals would produce ammonia. And so this is again another manifestation of ammonia induced stress corrosion cracking in copper base materials.

Now I'd like to talk about localized corrosion of the copper base materials.

When we performed the degradation mode surveys we found relatively few quantitative data for the localized corrosion resistance of copper base materials.

And as Joe will talk later on this as one of the important things that would need to be done if indeed our interest in the copper based materials continues.

There's been some information on exposure in sea water and that's where most of these ratings have been obtained, in sea water and other kinds of saline environments.

And pitting corrosion, if one were to examine pitting corrosion, we find that generally the high purity copper and the 70/30 copper are the better performing ones, aluminum bronze is the worst.

When dealing with dealloying effects, naturally the pure metal is the best, and particularly the aluminum-bronze is the worst.

Crevice corrosion, in this case the two alloys are very good but the high purity copper is not so good.

In biofouling, the significance of biofouling is not so much that we would expect that in Yucca Mountain except this might be indicative in a very general way of what microbiological organisms would do.

One of the reasons why copper is used so much in marine applications is because the copper corrosion products are toxic to marine life and so it's very resistant to biofouling. As one alloy of copper one reasons that effect because the concentration of copper corrosion products becomes less.

Now I'll talk a bit about hydrogen effects on these two alloy systems.

In the copper base materials, as we said earlier, one of the deficiencies or one of the weak points of the high purity copper was that it would tend to take up oxygen during rolling operations or other high temperature operations where oxygen is present in the atmosphere.

When this material is then subsequently exposed to a hydrogen generating environment, which could occur during radiolytic processes or even doing long term electrochemical processes, then the copper oxide which is present in the material is transformed into little water blisters that are inside the material, inside the copper, and this has been called a hydrogen sickness phenomenon.

If you want to be real authentic, it was discovered in Germany as the WasserstoffeKrankheit as it's called in the literature.

Because of this there's a severe degradation of mechanical properties when hydrogen sickness is there. There appears that there may be some mechanical property degradation in the bronzes because of the absorbed hydrogen.

We found very little information on any

detrimental effects of hydrogen in cupronickel. And again on these philosophical points, when one doesn't find any information, one is asked to conclude it's not a problem.

Again, particularly if one of these materials have been used in environments that should generate the cause of the agents and the cause of the kinds of conditions where comparable materials are affected, that that is usually an indication of good resistance.

And also the fact that there hasn't been an incentive to do work in that area or there's not that much published information maybe should indicate that at least up to date there's that that's been an observed technical limitation on those materials.

But I think one must always take that kind of statement to heart when one finds no or little information on what the repercussions of the interpretation of that would be.

Again the next slide just shows an example of where we have this line of little water vapor bubbles that form

and what that would look like on the fractured surface of a simple hydrogen sick copper.

Copper austenitic materials. Again absorbed hydrogen in degraded mechanical properties. And these materials are really quite resistant to hydrogen effects.

And what we have discerned in the literature, to give some comparison, the 304L stainless would be the more vulnerable to hydrogen emrittlement than the 316L.

Again we found very little information on the hydrogen effects in Alloy 825, which again really tends to lead one to the conclusion that it's not a problem.

I might point out that 825 is used a lot in sulfuric acid production and for containment of sulfuric acid. In other words, because of any corrosion in a very strongly acidic environment like that would produce hydrogen.

So one does feel some comfort in the fact that it survives in a sulfuric acid environment with no indication of detrimental mechanical effects, that it is indeed highly resistant to hydrogen embrittlement.

Now I'll talk a bit about the welding effects on the two alloy systems.

For the copper based materials, as we just said previous, is the possibility of the hydrogen sickness phenomena, and is an alternative is the use of the

oxidized graded copper because in that case we formed a more stable oxide such as with phosphorous than we would with water vapors.

With Alloy 613, the aluminum-bronze, we have as far as the fusion weld process what we would think would be the greatest difficulty, and particularly in the performance of the material that would result from the welded material.

Again one can introduce the particles. As I said earlier when we were talking about the phase diagrams where we had more tendency toward dealloying and localized corrosion if we have high aluminum fillers.

And the way this material's processed, that we have that aluminum rich base prevails at room temperature

Also there's some loss of ductility in the mid temperature range, and again it would be a question whether that loss of ductility maintained down to the temperatures of interest to us.

With the 715, it appears that this would have the fewest performance related problems due to welding.

Again it's because it's a fairly simple alloy system so we expect that the microsegregation effects can be resumable but they could be easily overcome.

You saw earlier in the lab tour yesterday that we had some material that had been inertial welded. And again, intertial welding as many of you probably gathered is one approach to get away from a lot of these fusion weld introduced problems.

Again it's very -- I'm just getting a little bit off -- I'll say the result of a study, you're trying to do an inertia weld with 102 copper, which is a little bit difficult, in fact it wasn't obtainable, it was because it was such a soft material, and you try to force those two materials together, they just gum together, they don't make a solid weld.

Next is the austenitic materials. Again, the principal concern, when you talk about the physical metallurgy discussion, would be the desensitization issues and the stress corrosion cracking that would result from that, weld cracking and the brittle intermetallics that can be formed.

And, again, primarily because of the metallurgical instability of the 304L, 316L, we have the greatest potential for detrimental welding effects.

Again, for a number of reasons but particularly to prevent any weld cracking during the actual welding operations, there's a tendency to have a balanced composition such that one does have some retained delta ferrite at high temperature phase of iron to maintain that down to room temperature.

And when that's done, that introduces quite a bit of complexity in what the microstructure and the various interactions between those quite compositionally different microstructures would be over a very long time period.

The 825 appears to be particularly simple in that it's a single phase. However, the complication that's introduced there, and again a philosophy subject to tracking during the actual welding operation, is to use this much richer nickel, much richer molybdenum containing 625 material.

From a galvanic corrosion point of view it's an ideal arrangement, but it's much more noble than the Alloy 825. So in any kind of preferential corrosion attack it would be the base material that would be corroded and that corrosion would be distributed over a much wider area so that the filler material would not be preferentially attacked.

I believe now we're ready to go on to the last part of the talk.

And what we have done in this slide is to summarize and put into one table the results from the degradation mode surveys compared by the different kinds of degradation for the three austenitic alloys.

And as one can see, the 825 in every category is the most resistant material, and in general the 304L is

the least resistant material.

I should say that these last two items that are information here is not very thorough and there's a lot more work that need to be done there. So we have information on radiation effects and microbial effects, but this is just a little bit of information we have gathered.

These are discussed not as separate volumes but in microbial for instance because it often affects the localized stress corrosion behavior in those volumes. And radiation behavior is discussed in the general corrosion oxidation volume.

The next slide shows a similar analysis with the copper based material. But in this case there is no one candidate that comes out as being the best in all categories. There's really quite a mixture and interplay of which one seems to be the better performing.

Our conclusion or our analysis is that the 715, perhaps all things considered, is the more resistant material, but the 613 also has some merit.

The high purity copper, as I indicated from some of the processing and welding points of view, has quite a number of disadvantages we feel.

Now, we say somewhat the bottom line of all this, we feel that the Alloy 825 is the most corrosion resistant

among the austenitic materials.

The 715 is probably the copper base material that's the most resistant, all things considered. But there are some cases where the 613 merits more consideration.

But I really want to make this point emphatic, that this is just the analysis based on the degradation mode surveys and there are other factors that have to be included in making a selection as Bill Halsey talks about.

Those would be some parametric study results, particularly when we try to compare these two families of materials and compare them under more representative Yucca Mountain repository conditions.

We have yet to evaluate the fabrication and closure process evaluations and results from them on the relative behaviors and relative rankings of the candidate materials.

And then on -- and Joe will get into this much later on -- on model mechanisms of degradation behavior, and particularly as we would judge our success in predicting different kinds of corrosion based on what we already know or based on what we think we can find out in the next few years. And those are all the very important factors that can go into the selection criteria.

And then the next slide, what I'm trying to do -- and again, it should lead very much into Joe's

discussion -- is trying to compare Alloy 825 with the copper base materials.

And, again, there are very few direct comparisons of these materials. And much of our planned parametric work is aimed at making those comparisons.

And to give some of the pluses and minuses of each one, the 825 is more sensitive to halide ions and copper base materials, as a rule, is less sensitive to halide ions. The copper base materials are more sensitive to nitrogen bearing species.

Well, in our environment we've got both, so that's the kinds of decisions that we have to make and the significance of them are that they very much affect the localized and stress corrosion effects.

As we talked earlier in Rich's talk we knew from the outset that the copper base materials would be more sensitive to radiation induced corrosion oxidation.

What we have to determine in the near future is whether that is indeed going to be performance limiting.

And particularly to establish that the corroding products are protective.

Often copper materials, when they corrode they take a long time to reach steady stage. It takes really years of exposure to get to that condition.

So sometimes we have short term data where the

rates are very high at the beginning and then just levels out with time. And what we have to do is make sure if indeed such a threshold exists when we get to that point.

And then on the other hand the 825 and austenitic materials in general are more complex physical metallurgy consideration. The 715 is a much simpler material, and there is a lot to be said for simplicity.

And often the result of that is that we would have fewer welding effects, fewer physical metallurgical, long range physical metallurgical considerations for the simple material.

And that concludes my presentation.

DR. VERINK: Dan, I'm sorry for not bringing this up earlier, but recognizing that there may be configurational things that influence the design, has any attention been given to pressure welding as an alternative to friction welding and others?

As you may know, if you can reduce the thickness by 50 percent you can get 100 percent efficient welds nominally at room temperature, not counting the heating that you get from deformation on combinations of materials.

DR. McCRIGHT: The best source on that, Ellis, would be the fabrication and closure reports of Babcock and Wilcox Developing.

They looked through a wide variety of processes and then they recommended ones for additional consideration, and inertial welding was one of those that they did.

And I don't recall -- apparently the pressure welding wasn't one that they recommended, because we didn't pursue that one.

DR. VERINK: Well, one of the things that is complicated about it what do you do about a tube, because you have to first either bend it out so it has flanged, do the pressure welding, and bend it back. But it's been a very successful kind of process in many kinds of industrial uses.

DR. McCRIGHT: Yes.

DR. CLARKE: Okay. It's 11:30. Did you want to start into Joe's or wait until just after lunch? The gentleman needs a break on his hands here for a moment. After lunch? Start an early lunch and return a little bit earlier?

DR. VERINK: I think that sounds like a workable scheme.

DR. CLARKE: Then we can report back at -- what time do you have on your agenda?

DR. CLARKE: 1:00 o'clock?

DR. VERINK: Come back at 1:00?

DR. CLARKE: Yes.

DR. VERINK: Okay. Let's shoot for 1:00.

(Lunch break taken.)

DR. CLARKE: Okay. We would like to continue with the container materials program this morning. As you see by the flow chart, the square boxes are basically activities that are completed.

The selection criteria as you heard from Mr. Halsey still is going through its final approval stage but the actual criterion work has been developed.

The degradation mode surveys obviously are done, and as Dan indicated, have been published.

You are now going to hear from Joe Farmer relative to our material testing and modeling.

Those of you that were on the tour yesterday probably got some of the advanced information that will be presented today. That is an ongoing activity, as is the other parametric studies.

All of this information is input to the selection process, as Bill indicated, and will lead to our material selection.

With that, Joe.

DR. FARMER: As Bill said, I'll be discussing with you the corrosion modeling and testing that we're doing to support the waste container.

In this presentation I'd like to try to follow this

outline to the best I can.

I'll begin by telling you about conditions outside of one specific spent fuel container. I wanted to do this so that you'll be reminded and have an appreciation for the types of environment that we're talking about as we discuss the corrosion processes.

Then I'll tell you about the documentation that we have of exitsing models to support our modeling activities.

The next two portions of the presentation deal with modeling and testing.

First I'll tell you about the measurements that we made of uniform rates of oxidation. This particular experimental activity has been to support modeling work for vapor phase corrosion.

I'll then move on and discuss modeling and testing to support the aqueous phase environment condition. Trying to tie this back to Dale's talk yesterday, the vapor phase condition is an expected condition, the aqueous environment is the bounding case, the bounding condition.

When we get into the discussion of the modeling and testing for the aqueous stage environment I'm going to illustrate this activity by discussing in detail models for pit initiation and propagation and models for stress

corrosion crack initiation and propagation, and also tell you about the type of experimental activities that we're undertaking to support these two modeling efforts.

I'll end the presentation by giving you some flavor about the status of the corrosion research that's being done here at Lawrence Livermore National Laboratory.

As I wrap up the presentation and tell you about the status of our experimental work, I will first tell you about measurements that we have made and are making on the corrosion and pitting potential.

Then I'll go on and tell you about some work that we have done in the past to develop in situ diagnostic techniques for identification of corrosion products. We believe that this could be applied in the future to cases that Rich discussed with you this morning, perhaps the in situ detection of the basic copper nitrate species.

And, finally, I'll end up by telling you a little bit about a new technique that we worked on that allows us to detect the initiation of stress corrosion cracks very, very early.

And I'll finish with a few summary statements.

First, the conditions outside of one specific spent fuel container. And I emphasize one specific spent fuel container because every container will see a slightly different environment. And this is one typical container

that I want to discuss with you.

In the time immediately following emplacement we know that the temperature of this container will remain above the boiling point for a considerable length of time.

We also know from some of the work that Rich discussed with you that we're going to have the radiolytic formation of nitrogen oxides in the case of dry air, and we'll have the radiolytic formation of nitric acid and ammonia in moist air.

Of course, we have to have moisture so that we can have the hydrogen contributive to the formation of these species.

We also have the possible formation, in this initial emplacement period, the possible formation of a salt crust on the container surface.

As you recall, the container is very, very hot. It can be up to 250 degrees C. As we have water possibly falling on the container surface it can reflux and evaporate. This will result in the formation of a salt crust.

After very long periods of time the temperature is expected to drop below the boiling point. This is due to the decay of the radionuclides inside the waste container.

After the temperature does drop below the boiling point we have the possible formation of a concentrate

electrolyte. Water that contacts the salt crust can form a concentrated electrolyte.

And of course, this is a very bad condition for corrosion. This is not an expecting condition but a bounding condition, something that is possible.

You also know from previous discussions in Rich's talk that we have the possibility for the radiolytic formation of hydrogen peroxide. And I'll discuss with you in a few minutes as I did with some of you yesterday in the laboratory, some of the effects of hydrogen peroxide on corrosion potential and pitting potential.

And finally, even at very long periods of time after we're below the boiling point on the surface of the container, we have the radiolytic formation of nitric acid.

Many of you have seen this curve before, but I just wanted to put it up to give you a firmer feel for the type of condition that this one typical spent fuel container will see.

You can see that the wall temperature starts out about 250 degrees C. And after some period of time, in this particular case on the order of a thousand years, it drops down to a level that begins to approach the boiling point.

Now, again this is the calculation for just one specific waste container. So other containers can be slightly warmer or cooler.

As we've already discussed with you, we've gone to great lengths to document the models that we're using to support our modeling effort. We're also using these theoretical models and mechanistic models to dry some of our experimental work.

We've done the best job that we can to document these models in four different places. The most recent documentation was at the SMiRT-10 conference held recently in Los Angeles. And I won't read each of these for you because I know you have them in your handouts.

I now want to discuss with you an expected condition. Containers exposed to vapor-phase environments. This is the type of environment that the containers will see for the first on the order of 300 to a thousand years.

This diagram represents the type of modeling effort we have to have in order to predict the type of failure that these containers might undergo in a repository and again, in a vapor-phase environment.

We had three information inputs into the model. These are along the top of the viewgraph.

We had environmental information that's been put into the model, materials properties input, and mechanical

force inputs.

When we considered the environment we had several things to be concerned about. The temperature, the partial pressure of both water vapor and radioloysis products, as well as the possibility of microbial growth. And we're doing our best to take these things into account.

When we consider materials properties, we have to know mechanical properties of the materials as Bill Halsey has already discussed with you. We have to know metallurgical properties, things related to the face of these alloys, and we also have to know about their chemical properties. How resistant are they to the chemical types of attack that you see in corrosive environments?

We have to have some knowledge about the mechanical forces acting on these containers. We expect that we could have four different types of mechanical loads imposed on the containers.

Force due to residual stress near the weld. This would be expected to be somewhere near the yield stress of the material.

We could have the possibility of unexpected lithostatic stresses operating on the containers. Again this is not expected, it's an unexpected condition, but it's something we need to at least give some thought to.

We could have the possibility of building up internal gas pressure in the container. This would be due to a scenario where the container would be subjected to perhaps a transportation fire and the heating of the gas inside the container would result in significant pressure so the container has to be capable of withstanding this internal pressure.

And then it has to be able to withstand structural loads due to movement and emplacement.

These three types of input were fed into models in the case of vapor-phase corrosion for uniform oxidation, stress corrosion cracking and mechanical failure.

In the case of uniform oxidation we have two basic types of corrosion models that we concern ourselves with, a simple linear growth of oxide or corrosion product on the surface and a slightly more complicated but not very complicated situation where we have parabolic growth of the oxide or corrosion product on the surface. The parabolic growth, of course, is due to the diffusion limitations of this growth process.

In the case of stress corrosion cracking whether it's in an aqueous phase or a vapor-phase, we have to concern ourselves with microstructural changes which occur in some of the alloys such as the austenites that Dan has

already discussed with you.

Some of the microstructural changes which are of special interest to us include sensitization. After we have sensitization, of course, we can have intergranular stress corrosion cracking occur.

When we think about modeling the stress corrosion cracking process, we divide the modeling effort into two types, models dealing with the initiation step for stress corrosion cracking and models dealing with the propagation of stress corrosion crack through the container wall.

And finally we have classical mechanical failure. I'm not going to discuss this today.

When we take our overall modeling effort, the end goal is to predict the time required for complete penetration of the container wall and a failure of this container.

In this viewgraph I want to summarize for you what we've done to date and what we think in terms of modeling the oxidation process in a vapor-phase environment.

Again we have two basic types of models for vapor-phase oxidation, parabolic models and linear models.

The parabolic growth law is usually cases where we have adherent protective oxide films. This growth process, some people refer to this as Bogger's law, but it's a diffusion limited process and that's why you have the parabolic growth.

We have a second scenario where we can have linear growth of this corrosion produce on the surface. In this particular case we don't have the same diffusion limitation. The reason is because the oxide film falls off the surface and the corrosion product does not form a protective coating over the metal underneath.

We're doing various types of tests to support this model. For instance, we're doing coupon exposures to steam and water. In the next few slides I'll show you some of the data that we have collected from this type of activity.

When we do these coupon tests we make periodic measurements of the gain in weight of the coupons due to the formation of this corrosion product on the surface.

We've been performing experiments at temperatures ranging from 50 to 150 degrees C. And as Rich Van Konynenburg discussed with you earlier, we've also done what we can to determine the effects of gamma irradiation on the oxidation process.

We've looked at the effect of gamma irradiation both on rates as well as the type of corrosion product that we form. And we've gone to great lengths to try to positively identify all corrosion products using X-ray diffraction.

These are data taken in our own laboratory and illustrate the corrosion rates that you would expect to observe in a repository light environment.

Here we have data for some of the austenitic alloys exposed to both water and saturated steam. The primary conclusion that you draw from looking at this type of data is that the container life will not be limited by uniform oxidation provided we build the container out of an austenitic type of material.

You'll notice here we have a marker, .1 millimeters and 1,000 years. You'll recall that we're talking about a container wall thickness that will be somewhere between one centimeter and three centimeters thick.

Well, clearly in a thousand years, if this particular corrosion rate would give us only .1 millimeter of erosion, we're talking about losing less than one percent of the container wall thickness in a thousand years.

So, again, we know from our work that container life will not be limited by uniform rates of oxidation and corrosion. And this -- pointing out, this is data that was collected in our own laboratory.

Next slide, please.

From similar type experiments with the copper based alloys we have learned that container life may very well be limited in the case of the copper base alloys.

Here we have data at three different temperature levels representing both saturated steam and saturated liquid.

Here you'll notice another marker. In the previous slide -- I had intended to change this but haven't had the opportunity yet -- but in the previous slide you saw a marker for .1 millimeter per year, one percent of the wall thickness. This marker represents 10 percent of the wall thickness.

So this particular observed corrosion rate would indicate to us that 10 percent of the container wall thickness would be oxidized away after a thousand years.

Well, clearly, some of these rates that are being observed are much higher. So in the case of the CDA 715 we have high enough rates of oxidation so that we would expect up to 60 percent of the wall to be oxidized away.

DR. VERNIK: Did I understand from earlier comments that copper base would be more likely three centimeters rather than one?

DR. FARMER: Probably so. I --

DR. VERNIK: So three percent.

DR. FARMER: Yes. I think considerations like this we went with the copper base alloy would drive us to a thicker wall as well as their ductility and some other

considerations.

But nonetheless even if you consider 60 percent one centimeter or reduce that by a third, that's still very significant compared to the types of effects you'd see with the austenite.

Okay. I'll just show you this again. I mentioned in the outline that we've done considerable work to try to identify corrosion products as we find them.

This slide was shown to you before by Rich, and we have gone to great lengths to dry to identify corrosion products that we see in various places such as some of our particle accelerators.

These are the X-ray diffraction data that we've taken to identify these corrosion products. And if you compare our observed X-ray diffraction pattern with that of a known basic copper nitrate diffraction pattern you can see that there's quite good agreement. If you look at the major piece here you see that you have quite good agreement.

Now I'd like to go on and discuss with you some of the modeling that we're doing and testing to support that modeling for aqueous phase environments.

These aqueous phase environments again are a bounding condition, not an expected condition. I'm going to spend considerable time discussing this with you though because from a theoretical point of view, from a testing point of view, it's a much more complicated case than simple oxidation. So it requires more explanation.

Again we have three types of inputs into the model. We have environmental inputs, we have materials properties inputs, and we have mechanical force input, very similar to what you saw in the case of vapor-phase oxidation.

In this particular modeling effort we picked up an additional mode of failure, localized attack. You'll recall in the previous slide we were concerned with uniform oxidation, in the case of aqueous corrosion, I call it uniform oxidation.

We had the uniform attack mode, we had stress corrosion cracking and mechnical failure. But now we pick up an additional mode of localized attack.

In the case of aqueous corrosion we have to worry both about passivation and dissolution processes. In modeling our localized attack we have to take into account crevice corrosion as well as pitting. Pitting is very much like stress corrosion cracking.

We have models that explain the initiation step and models that explain the propagation step. Stress corrosion cracking, again we have to worry about sensitization and formation of M23C6 carbides. We have to worry about the initiation of these stress corrosion

cracks and we have to worry about the progation of the stress corrosion cracks through the container wall.

And finally, we have to also worry about mechanical failure. The end goal, of course, is to predict the time of failure.

In the remainder of the discussion I'm going to concentrate on the pitting corrosion and stress corrosion cracking models. More specifically, I'm going to illustrate for you one model in each of these two categories, pitting initiation model and a pitting propagation model, stress corrosion cracking initiation model and a stress corrosion cracking propagation model. With the end objective being to predict time of failure.

From the documentation we have gathered on all of these models, we have come to the conclusion that these are the basic initiation and propagation models for pitting.

We have six primary types of models for the initiation of pits on the surface of austenitic alloys, and I emphasize austenitic alloys. The halide nuclei theory developed by Professor Okada of Japan; point defect model developed by Digby McDonald and his coworkers at SOI; critical suppression of pH model developed by Galvele in '76; an electrostriction model developed by Sato; an inclusion model, which is more a conceptual model than mathematical model, developed by Manning; and the applications of stochastic probability theory by Shibata and Takeyama in '77.

When we think about the propagation of pits in austenitic alloys we have two primary types of models to concern ourselves with when we go through the review of accepted literature.

The first model is a quasi-steady-state mass transport model. And this basically means that you let the time derivative be zero and assume that you've always come to some steady state in solving the differential equations for the transport processes.

This model was first developed by Pickering and Frankenthal in '72. And it was later modified by Galvele to take into account the hydrolysis reaction at the base of the pit.

Galvele's contribution to this modeling is important because it gave us some appreciation for the importance of pH suppression in pits and gave us an understanding of why the pH suppression drives the pit propagation process.

The second generic type of model we find for pit propagation is a transient mass transport model. One of the first developed very thoroughly in the literature was presented by Beck and Alkire in 1978. This was in the Journal of the Electrochemical Society.

Now, you'll notice here I've emphasized that these models are applicable to austenitic alloys. The theoretical work that is documented in the literature is for austenitic alloys.

We've concluded from going through all of that work that predicted models for pitting of copper based alloys do not exist. We cannot quantitatively predict rates of initiation on pit propagation in the case of copper base alloy.

This is a need that we've identified and one that will have to be addressed. And again, time and resources will be required for the development of this type of model.

Now, first, as I discussed with some of you in the laboratory yesterday, I would like to illustrate for you some of the modeling and testing effort to support a pit initiation model, specifically the model of Professor Okada, the halide nuclei theory.

So the primary thing that I wanted to impress upon you as I discuss this particular pit initiation model is first of all two important parameters are derived by Okada, the critical pitting potential and the incubation time for pitting, how long it takes for a pit to initiate on the surface of the container.

You'll notice that both of these expressions allow one to calculate the effect of chloride on the pitting potential and the effect of chloride on the incubation time. There are actually more recent derivations which also allow us to take into account pH.

But the important thing to appreciate as you look at these expressions is that they are logarithmically linear in chloride concentration. And this gives us an important experimental handle as we try to treat our data.

The other important thing that I would like to impress upon you about the type of work that we find in the literature, especially that of researchers such as Professor Okada, is that he's used two independent theoretical approaches to derive these expressions.

He has used nonsteady state thermodynamics as one approach and he's come up with this functionality. And he's also applied perturbation theory and he's come up with these two expressions.

So two independent theoretical starting

places, but it brought him to the same functionality.

Now that gives us quite a bit of confidence in the generality of those expressions as we go and try to treat our data in using those expressions.

We also developed confidence, because as we look at the derivations and work from other researchers such as Cal and McDonald at SRI, we find that even though they use a point defect model as their starting place, they also derive the same functional expressions.

Similar equations have been developed by Galvele and have been verified empirically by Matamala. And this in fact is his correlation. And you can see, of course, that we had this logarithmic dependence on chloride concentration and linear dependence on pH and temperature.

You'll also notice the appearance of these two factor interactions in this empirical expression.

Okay. So we have models that are applicable to the pit initiation process. We now have to go and do experiments to support those types of models. These are the types of experiments that we're doing to support those pit initiation models.

We're making measurements of the critical pitting potential using potentiodynamic linear sweep polarization as you saw yesterday in the laboratory.

We're also making measurements of the incubation time by doing potentiostatic polarization of single and multiple samples. When we do polarization of multiple samples we can again apply the stochastic probability theory to the analysis of the results, and that is the statement that you see here.

The thing that I want to show you in the next few

viewgraphs has to do with the use of factorial designs to minimize the number of experiments that are required to determine the dependence of pitting potential and incubation time on facts such as chloride, pH and temperature.

Actually before I get to the discussion of the factorial design, I show you this viewgraph again as a reminder of the type of experiment that we're doing to measure the pitting potential.

As Dan discussed with you before, the pitting potential is measured as the point where we have breakdown of the passive film and begin to pass substantial amounts of current while doing the polarization curve. And again, some alloys are better than others.

To minimize the number of experiments that we have to do to determine the dependence of pitting potential and incubation time on temperature, chloride and pH, we use this particular type of strategy.

The factorial design allows us not only to pick up the important two factor interactions such as the interaction between pH and chloride that you see here, but it also allows us to pick up potentially important three factor interactions.

And, again, even though this equation is relatively complicated and takes into account numerous effects, we can fit this equation using only eight experiments. And if we employ factorial designed experiments we can also determine the confidence intervals on these parameters.

So it not only allows us to fit the parameters, it also gives us statistics with only eight experiments.

This is the factorial design that we're using for a typical pitting potential series.

Here we have eight experiments. The plus represents a high level of the variable that we're interested in. The minus represents a low level of the variable.

And here you see the plus and minus levels for both the independent variables as well as the two factor interactions and the three factor interactions.

Now, I'm not going -- because of the time that it requires, I'm not going to discuss in detail with you the pit propagation models, but I wanted to tell you how we have gone about quantifying pit depth and quantifying things having to do with pit propagation in the past and how we plan to do these things in the future.

The first thing that we do in the pit propagation measurements is we want to determine pit depth as a function of time. And we normally follow this procedure in order to do this. We first expose our coupons to an environment of interest, be it J-13 well water or some

modified J-13 well water.

We then use optical microscopy to determine the distance from the surface of the coupon to the base of the pit by measuring the focal distance at the base of the pit. This is a fairly standard practice and I'm sure some of you probably have done it yourselves.

We can also use optical microscopy of metallographic cross-sections where the pits are large enough.

We have plans but we haven't implemented them yet to use optical microscopy with a video camera and digital image processing to actually quantify the fractional coverage of these surfaces by pits. And I think this is a relatively new technology that we're anxious to apply to this problem.

And, finally, we always make measurements of weight gain or weight loss.

As I said in the previous slide, there aren't any models to quantitatively predict the rate of pit initiation or the rate of pit propagation in the case of copper based alloys. And I wanted to show you this slide so that you would have an appreciation for why those models do not exist.

The pitting process on a stainless steel is relatively simple. You have the formation of a halide

crystallite on the surface of the oxide film, a single crystallite.

And you can see that is in dramatic contrast to the type of processes that you have occurring in the pitting of copper. Here you can see that you have multiple diffusion barriers and very complicated chemistry.

So to solve the differential equations for this it's challenging to say the least. And this is the reason why these quantitative models do not exist in the case of copper based alloys.

I'd now like to go on and discuss with you what we know about the modeling of stress corrosion cracking and then elaborate further to tell you about the types of experiments that we're doing to support this stress corrosion cracking modeling effort.

Again, we have two phases in the stress corrosion cracking process that we have to be concerned with, the initiation step and the propagation step.

These are the three initiation models that we've concerned ourselves with primarily. We have a linear-elastic fracture mechanics model that was developed by Hagn in 1983.

We have a crack-tip-opening displacement model for the initiation of stress corrosion cracks at pits that was developed by Buck and Ranjan in 1984. Perhaps it was developed earlier and published in 1984.

And, finally, we have the concept of spontaneous initiation which is an initiation process that is proposed by Andresen and Ford of the GD Research Center.

When we consider the propagation of stress corrosion cracks, we have three primary models that we're considering.

We have one model which assumes that the crack is propagated purely by anodic dissolution of the crack tip. This concept was proposed initially by Turnbull and Thomas. Their paper was published in 1982.

We have another model which is more widely accepted that was proposed by Andresen and Ford. Their papers were published between the years of 1982 and 1988.

This particular model is based upon the

periodic fracture of a passive film at the crack tip. Every time you rupture the passive film at the tip of the crack you have an elongation of the crack.

And finally we have a film-induced cleavage model that was proposed and developed by Paskin and some of his coworkers. However, this is a very controversial model and we haven't put much emphasis on it.

Most of our emphasis in terms of propagation models is on the periodic fracture model and most of our effort in the initiation area is using the crack-tip-opening displacement model initially proposed by Buck and Ranjan.

These are some of the underlying assumptions of our stress corrosion initiation model.

First of all, it's assumed that stress corrosion cracking initiates at pits. You can't have stress corrosion cracks initiate on the surface unless you have some sort of preexisting flaw such as a pit.

Secondly, there were assumptions that were made about the electrochemistry that occurs inside or around the pit.

First it's assumed that the base of the pit is active, and secondly it's assumed that the pit depth varies linearly with time. This is inherent in the assumption of Butler-Volmer kinetics.

We have to assume that we have sufficient displacement at the mouth of the pit due to an applied stress so that we don't have blunting of the crack tip.

And finally, we assume that the propagation rate of microcracks at the base of the pits are proportional to this opening displacement at the mouth of the pit.

The crack tip opening displacement model by Buck and Ranjan allows us to predict both the conditions and time required for the initiation of stress corrosion cracking.

In order to get at the conditions required for stress corrosion cracking they give us two criteria. First of all, the displacement at the mouth of the pit has to be greater than some critical value, Delta sub naught plus Delta sub c.

And secondly, the applied stress sigma has to be greater -- this is an end quality so you should take this stress intensity factor over to the right-hand side of the end quality margin. But at any rate, this quantity has to be larger than this quantity.

So that we have two criteria that have to be satisfied before we can have initiation of stress corrosion crack at site of pitting.

Starting with these criteria, these two investigators have developed an expression that allows us to predict the time required for initiation of a stress corrosion crack.

The expression that they had developed for this initiation time has two factors in it. The first factor allows us to take into account mechanical effects. The second parameter allows us to take into account environmental effects through electrochemical corrosion.

The reason I showed you that slide is I thought that it was important for you to have an appreciation for why we're having to run some of the experiments that we are. These are experiments that we're doing to support

initiation models.

We're making measurements of the threshold stress intensity factor for stress corrosion cracking K1SCC, using modified wedge-opening-loading fracture specimen, WOL specimen.

We're also trying to make measurements of the incubation time required for the initiation of stress corrosion. And this describes in brief the procedure we're going through to make these measurements of incubation time.

We are loading specimens in a screw-driven tensile machine. This is the same procedure that Buck and Ranjan have employed in the past.

We're then subjecting the loaded specimen to various environments and we're varying the environmental chemistry, the electrochemical polarization and the temperature.

We're then measuring the time required for reduction in stress at constant displacement. For instance, when you initiate a stress corrosion crack you see the signal from your load cell begin to diminish. So that is our technique for measuring the initiation time required for stress corrosion crack, one of the techniques.

I now want to go on and discuss with you the type of models that we're employing and the types of experiments that we're doing to address the problem of the propagation of stress corrosion cracks through a container wall.

The primary model that we're relying on is one developed by Andresen and Ford and it involves the periodic fracture of passive film at the tip of the crack.

This is the expression that they have that allows us to predict the rate of crack propagation as a function of the crack tip strain rate, the film fracture strain and the thickness of the passive film at the crack tip. The thickness of the passive film at the crack tip is represented by this electrochemical grounder.

We have molecular weight and Q sub F. The Q sub F is the amount of anodic charge involved in repassivation of the crack tip after it fractures.

Z is the number of electrons involved in the repassivation process, rho is density of the oxide film, and F, of course, is ferrous constant.

At any rate, this expression gives you an appreciation for the types of phenomena that are involved in the propagation of stress corrosion cracks.

We have an electrochemical process occurring and we have a mechanical fracture of the passive film at the crack tip.

In order to quantify Q sub F, the amount of anodic charge involved in the passive film at the crack tip, we have to perform transient electrochemical experiments.

As you saw yesterday, we have a number of potentiostats that are doing some transient electrochemistry experiments. We're looking at current transients as a function of time to determine these kinetics.

And, of course, the end result is that we want to develop some functionality that allows us to predict the propagation rate as a function of environment. The environmental effects can be taken into account in the lumped parameter of N.

You'll recall another important part of that Andresen-Ford model is the crack tip strain rate. You actually can derive an expression for the crack tip strain rate theoretically.

Unfortunately when you do, you find that the dependence of the crack tip strain rate on the stress intensity factor is one where it is expected to vary as the square of K. In reality we observed that it varies to the fourth power of K.

And I wanted to show you this to emphasize to you that theoretical models are good and we use them where we can, but there are situations where the theoretical models don't agree with the data and we have to rely upon semi-empirical correlations and mirrors between empiricism and mechanistic models to make things work. And this is one of those situations.

You saw in the laboratory yesterday the type of instrumentation that we're using to make measurements on the crack propagation rate. This is our reverse DC instrument. It was built by General Electric.

The reason we decided to use the reverse DC instrumentation for measurement of crack propagation rates is because it is being used in several BWRs around the world and has -- and a great deal of credibility has been built around this particular instrumentation.

In the machine you saw yesterday there are two autoclaves. Each autoclave has three testing stations. We have three testing stations in this autoclave for copper based alloys, three testing stations in this particular autoclave for the austenitic materials.

The electronics in the center rack is responsible for applying the reversing DC pulse of current which is about five amps in amplitude and about one hertz frequency.

And we basically measure the potential drop in this instrument across the crack and correlate that potential drop with the length of the crack. But it's fundamentally a very simple measurement even though the instrumentation is large.

And we're using this technique to measure the parameter of D80T. And then we're fitting D80T as a function of K to determine, whether it's K to the fourth or fifth or whatever.

Okay. Now I want to move on and tell you about some of the experiments that we've done in the past. A lot of the work that I've discussed up to now has to do with modeling, the documentation of those models, what we plan to do with modeling, and the experiments that we plan to use to quantify the parameters of those models.

I now want to tell you about the experiments that we've actually conducted in the past. One of the first experiments that I wanted to tell you about has to do with some electrochemical measurements that we have made in a gamma pit on site at the laboratory.

This is the electrochemical cell that was involved in those gamma irradiolysis experiments. It's very much like the electrochemical cell that we used to do the standard polarization test in except the geometry has been compressed a bit so that we could fit it down into the gamma pit.

And you'll notice that it has the standard electrochemical cell configuration. We have working

electrode, reference electrode, Luggin probe, counter electrode, so on and so forth, and the ability to access the electrolyte in the cell.

This whole cell is lowered down into the gamma field when we do an experiment.

These are data that were gathered several years ago before one of our stop work orders so that we can -- we did these experiments so that we could develop an understanding of the effects of gamma irradiation on corrosion potential.

These markers off and on represent conditions where we modulated the gamma field around the electrochemical cell. We modulated the gamma field by lowering the electrochemical cell down into and moving it from the gamma bed.

And you can see that when we turn the gamma field off the corrosion potential drops, it moves in cathodic direction. When we turn the gamma field on we have a positive deviation or excursion in the corrosion potential.

We did other experiments and finally came to the conclusion that these modulations of corrosion potential were due to the formation of hydrogen peroxide.

Hydrogen peroxide, when it is reacted on the surface of the electrode, it moves the mix potential in the anodic direction anywhere from 100 to 200 millivolts.

The previous data was from one of our austenitic candidates. This data was taken for OFHC copper. Here you can see the same general effect.

When we first expose the sample to the gamma field we see a positive deviation in the corrosion potential by about 100 millivolts, then the potential decays exponentially until we turn the gamma source off. And at that point it drops to background levels.

We did not observe this rapid exponential decay in the case of the austenitic materials. This particular decay is due to the electrochemical reduction of the hydrogen peroxide on the copper surface. Copper turns out to be a very good reduction catalyst.

Again, I apologize. I know I've shown most of you these slides twice. But just bear with me and I'll get on to something new in a bit.

We've also investigated the effects of gamma irradiation on the pitting potential of 316L in sodium chloride solutionn. Again we did this in the same electrochemical cell that you saw in the slide previously.

We modulated the gamma field by lowering the cell into a region close to a cobalt source, and then removing it from that area.

You can see that the corrosion potential has shifted very substantially by the presence of gamma irradiation. The starred values represent values measured in the presence of the gamma field.

The shift here is on the order of 200 millivolts which is comparable to the types of deviation that you saw in the previous viewgraphs.

In contrast, if you look at the pitting potential you can see that there's very little effect of the radiolysis on the pitting potential. And if anything, it may be a bit better -- or I'm sorry, not a bit better -- it's only a bit worse in the presence of the gamma irradiation.

And again we quantify the resistance of a particular alloy to localized attack by taking the difference between the corrosion potential and the pitting potential.

Since implementation of our new QA plan we've been trying to determine quantitatively the effects of chloride concentration on the pitting potential and the incubation time. This is one example of some of the data that we're gathering for our 316L.

And along the X axis we put markers that represent various concentration levels so you'll have a visual understanding of exactly what these concentration levels mean.

For instance, we know that the concentration of chloride in J-13 well water varies between five and ten parts per million. So we're down to this extreme end of the range in the case of J-13.

We know that the vadose water is somewhere around 100 ppm. More accurately, I think it varies between 35 and 170 ppm.

The sea water -- or actually, this is not really a sea water concentration but close to a three and a half weight percent salt. But this is the vicinity where we would expect to see a response in sea water.

And finally, this is the saturation of a solution with chloride.

So these are the four levels in extreme that one might expect to see. And we have been taking measurements in the last month, working our way to both ends of the chloride concentration scale and trying to determine a critical chloride level.

This is the level that we begin to induce localized attack of this particular candidate material, 316L.

And we so far have determined that the critical chloride concentration lies somewhere between a thousand and 5,000 parts per millimeter. And as we speak, we're gaining additional beta points to fill on this curve.

Rich discussed with you experiments that he has done and some investigations that he has done in order to identify corrosion products in a gamma field.

One problem that we have in looking at corrosion products and trying to identify them is the in situ identification of these corrosion products. And this is especially true in cases where you have gamma fields.

In the past we've developed in situ diagnosic techniques that allow us to determine and identify corrosion products on services.

Next slide. One of these techniques is laser Raman the technique that I just showed you.

These are spectra that we have collected in an experiment with copper in a solution of approximately pH

12 to 13. In this particular experiment what we've done is perform a linear polarization experiment while we simultaneously observe the surface using the laser Raman.

And you can see that by doing this we can experimentally map Pourbaix diagrams. And we have developed good techniques to do this.

Here we see initially the formation of of cuprous oxide as you would expect to see from the Pourbaix diagram with the ultimate formation of cupric oxide in an intermediate region and cupric hydroxide ultimately.

The reason we don't see strong bands for the cupric oxide in this particular case is because it's very weak

Raman scatter.

We've gone on and extended diagnostic techniques such as these to the investigation of model alloys. This is a copper silver alloy. We looked at this alloy because all of the species which we expected to form from a Pourbaix diagram had very strong Raman scattering cross-sections.

To our surprise, however, all these species occurred at potentials that were very much different than we expected from Pourbaix type calculations. So there is some need for in situ determination of stability of these compounds.

And we've even identified some products which are not identifiable. For instance, you will notice that these bands have no weight on them, that's because they can't be identified based upon any known standard.

We've also been working hard to develop new techniques, not only for the identification of corrosion products in situ, but also for the early detection of stress corrosion cracking.

And as I show you this data, I purposely tried to show you data pretty much as we collected it because I wanted you to appreciate the fact that it is indeed data and it's not terribly polished.

But this is an instrument that we put together in our laboratory that's equipped with acoustic emission detection. And in this particular apparatus we put acoustic emission sensors on both sides of a tensile specimen. As that tensile specimen begins to crack it generates acoustic emissions.

Next slide.

This shows acoustic emissions that we

collected from that electrochemical cell. Here we see acoustic emissions as a function of gauge length along the tensile specimen and acoustic emissions as a function of time.

Time is along the Z axis, the number of acoustic emissions is along the Y axis, and here the number of -- I'm sorry -- distance lies along the X axis. This is the center of the gauge length and the two extreme ends of the gauge length.

And this turned out to be a very tricky measurement. We had to use four sensors simultaneously to image these stress corrosion crack initation events, if you will.

We've had to use sensors on both ends of the gauge length, the actual detection sensors as well as two guard sensors to discriminate against spurious acoustic signals.

As we discussed with you yesterday, we've gone to great lengths to implement a workable QA plan. We've now identified required activities by subactivity numbers. In

this particular column we see a description of the subactivities.

We have corrosion modeling which is now referred to as Subactivity E-20-16a, linear polarization measurements E-20-18a, so on and so forth. You can see that all of our planning documents, as we showed you yesterday in the laboratory, have been approved.

Thus far, the only two activities that are really underway under the new QA plan are the radiation effects experiments that are being done in collaboration between our lab and Argonne National Laboratory, and the linear polarization measurements that are being done principally in our laboratory.

In summary, what I had planned to do for you is to begin by reminding you about the types of conditions we expect to see outside of one typical spent fuel container.

I showed you a predicted temperature profile to give you hopefully a better feel for the type of temperature history that some of these containers will see.

I also discussed with you the formation of various corrosion products on the surface by gamma radiolysis, and I had discussed with you the role of various ions on the localized attack of these container materials.

I then went on and reviewed for you models and

testing strategies that address uniform oxidation, initiation of pits and the propagation of the pits through the container wall, as well as models and testing for the initiation and propagation of stress corrosion cracks.

Overall I believe that we have found and documented models to predict the failure of container materials in the environments of interest.

The model that I discussed with you today include the halide nuclei theory, a point defect model, a crack tip opening displacement model and a film fracture model.

We have been implementing a QA approved experimental strategy to determine the various parameters of importance.

As I stated before, we're determining corrosion potential, pitting potential, incubation time, the threshold stress intensity factor for stress corrosion cracking, the incubation time and the crack propagation rate. We're determining parameters such as these experimentally.

And, finally, the work is being done in accordance with the quality assurance requirements of 10CFR60. This has required that we have spent significant time in writing detailed activity plans, effectively implementing procedures and individual software plans.

And I believe that I've also illustrated for you

how some of our past research, research done before implementation of our QA plan, has led to some insight into the types of corrosion chemistry that's important in the life of these containers.

If there are any questions I'll be happy to try and address them.

DR. PRICE: I have a couple. What is your comment on the similarities that you expect to see between the J-13 well water and the actual near field water having gone through evaporation and condensation and so forth, whatever it goes through?

DR. FARMER: Well, I guess first I would have to say, as it was stated yesterday, Well J-13 is a substantial distance from where we want to build the repository.

We believe that water is representative but we're not absolutely sure of that and we won't be until an exploratory shaft is drilled.

But my own opinion is that that is representative of the type of water that you might see coming into the borehole.

Does that answer your question?

DR. PRICE: How do you think the process of evaporation and condensation would affect that --

DR. FARMER: Well, some of the species have -- I mean they have different solubilities as a function of temperature and other parameters that you might vary.

So some of these species will precipitate out before others. For instance, you will be able to concentrate the chloride ion much more than you will be able to concentrate bicarbonate ion.

So as you concentrate these electrolytes you're not going to maintain the same ratio. In fact, you're very lucky to be able to get a 20X electrolyte. Under some situations we've seen precipitation from 20X electrolytes.

So as you concentrate these at some level some of the ions will be there at their saturation levels and others will continue to increase.

DR. PRICE: So it could be considerbly different, would you say?

DR. FARMER: Than J-13?

DR. PRICE: Yes.

DR. FARMER: I believe that, yes, that's correct. And we're trying to take that into account in our experiments, if that's the question you're asking.

DR. PRICE: Second question is a little more philosophical. What do you see as the relationship between your relatively short term corrosion tests and the long term of hundreds of years, maybe thousands of years?

DR. FARMER: Well, the goal of doing all of this work is to try to determine parameters in predicted models.

For instance, I think it would be naive on my part to say, for instance, to take this polarization data and to assume without verification that that will last for a thousand years.

But I believe once we, you know, collect the short term data in the laboratory and fit these models we will make every effort to go on for some period of time and try to verify those models and make sure that those impressions are correct.

Is that what you're asking?

DR. PRICE: Yes. But isn't it a very difficult problem?

DR. FARMER: Well, it is very difficult, there's no doubt about it.

In showing you these models I haven't tried to convey to you that, you know, we have absolute faith in these models and that we've done unshakeable predictions.

What I've tried to do is show you that we have gone through all the corrosion modeling literature -- that's very controversial literature, by the way -- and we tried to extract the best that we can, models that we feel will help us have some predictive capability in terms of predicting performance life.

We now have to take the experimental data that we're gathering and fit the parameters in those models. And it's yet to be determined how well those models will predict reality. That's something that yet has to be determined.

DR. PRICE: Well, for such a long term validation of the models it's going to be a challenge?

DR. FARMER: That's right.

Thank you very much.

MR. CLARKE: Okay. Before starting, let me mention, I'm sure you've already noticed there were a few of the slides missing from Joe's presentation in your handouts. That was strictly by accident and we will assemble those. And I've already discussed with Helen, we will mail those out to you.

Okay. I am going to, in the next few moments, discuss the alternates program that was at the laboratory a year ago, until about June of 1989, and discuss just a little bit the reasons that we felt at the time that the alternate materials program was needed.

The first bullet indicates that it does need a regulatory requirement. I won't go through that, but all of that is 10CFR60.21 plus all the subparts.

And because this is an important regulation, I took the liberty to bring it along to read it so that I could get the wording right. And that particular regulatory requirement indicates that a safety analysis report shall also include a comparative evaluation of alternatives to the major design features that are important to waste isolation, with particular attention to the alternatives that would provide longer radium nuclei containment and isolation.

Now, it's reasonable to conclude from that that a fully developed alternative design is not required to meet that evaluation.

However, we did strike off in a path about a year ago in that we have a reference design case that I went over this morning and also an alternate path design. And that's what I'm going to talk about is just the alternate path here for the next few moments.

Some of the reasons that we felt were very important, it protects against a different set of environmental circumstances. We do not have fully characterized site data at this time.

Once we do obtain that information it may turn out that we have more water. We may have more aggressive water chemistry and we also, for whatever reason, may have higher loads than is expected.

Also a concern is that somewhere down the road, for whatever reason, that the reference design or the

reference material will not meet the performance assurance. And that could be that the containment release requirements may not simply be met by the metal barrier.

Also we know from the nuclear power industry business that having a redundant design often provides licensing conservatives and it makes licensing easier.

Now, the container material selection process is, as you notice, very much the same as what we had intended for the metal barrier.

We started out with screening the concepts, we will develop criteria, we would do degradation mode surveys based on that criteria and the modes that were identified, there would be parametric testing done, and finally selection, performance testing and development of the models for performance assessment.

What I will be describing today is the screening of concepts portion of the program.

To give you a feel for the accomplishments, there was a scientific investigation plan written and it was approved by the project office in Las Vegas.

Then the scientific investigation plan was revised and the metal portions of that alternates program were taken out, that was looking at other alternate single metals and also looking at bimetals. And those were put into the metal barrier portion so that the scientific investigation plan that was revised to the 1988-89 Rev.2 QA plan contained basically the nonmetallic portions of the alternate plan.

There also was for that work, and basically this was involved in ceramics as you heard yesterday, there was an activity plan written and then there was QA level assignments assigned and graded. The work was graded under a Level 1 activity.

There were some ceramic studies initiated, a workshop was conducted. I think those of you that were on the tour yesterday heard a lot of this from Clarence Honig, and I'll just present it in a little bit of an overview fashion in a moment to put it into the record.

There was a trip to Sweden to review the container progress, their container progress.

There was a candidate manufacture survey completed.

There was a closure study started at the laboratory and there was a model report written which has not been completed.

There also was a graphite workshop conducted. And then there were preparations to reassign this task to the management operations organization. And so we prepared a turnover package and that was transmitted to you back in June of '89.

Now, just to give a few words about the concepts

that we were considering at the time, some we talked about a little more than others, some we actually did some work in. Ceramics was on top of the list. Graphites was one of our concepts. Bimetals, other single metals than the six that you've been hearing about this morning.

We also discussed briefly things like coatings and fillers. And then thicker wall metals, which I am not prepared to talk about today because there was no work in that area, but this would be very much like what is being done presently in Canada and Sweden, where I believe that their reference design is currently four-inch thick copper.

In terms of the ceramics, the primary candidates that were discussed, as you heard yesterday, was aluminum and titanium. Both have superior corrosion resistance to the metals and they are geologically very stable at Yucca Mountain.

The data that you see there is real data developed by the Swedes and given to us during the trip. Less than one millimeter over the 10,000 year period for aluminum. This was done in 30 degree centigrade in oxygen bearing siliceous water.

And another value, less than 10 to the minus 12 millimeter for 10,000 years where titanium is a real value. They did this using titanium 44 radioactive traces.

One of the biggest concerns -- and again, you had a very good presentation of this yesterday during the tour, the delay fracture due to defects we feel can be eliminated by minimization of the residual stress. This becomes fabrication concerns and closure concerns.

Our ceramics people at the laboratory feel that the fabrication technology, mass production of high quality alumina is well understood. Obviously work would have to be done in large shapes and lengths such as the containers.

Closure was always considered to be one of the major concerns but it is believed that containers from either alumina or titania can be closed satisfactorily.

The ceramic study was conducted at Livermore and it was on November 2nd of 1988. The two topics were alumina and titania.

Based upon that workshop there was a request for proposal issued by the laboratory to fabricate half scale demonstration containers, and all of the specifications and all of the drawings to procure those half scale containers was put into place.

The closure study was initiated and there were requisitions placed for parts and supplies. There also was a preliminary nondestructive examination study

initiated, and there the concern was how we could measure and identify residual stress, voids and defects, and those types of things that we considered would give us a problem during closure.

There also, as you heard yesterday, was preliminary HIP hot isostatic pressing study for closure initiated concerns over localized heating which would lead to nonuniform thermal stresses, but it was determined that with the use of compressive pressures up to about 30 psi that the ceramic staff feels that closures could be made satisfactory in these container ceramics alumina.

The closure study looked at high quality closure temperatures of less than 650 degrees C, and those are considered to be feasible.

One of the problems, as you heard yesterday, that the Swedes got into was trying to make closure at too high temperatures which had an impact on the fuel or would have an impact on the fuel and also on the residual stress that was produced on the half shells of the ceramic containers. These lower temperatures are necessary and we feel that there are ways that these can be done.

Also already mentioned, the 35 psi pressure. There also was work done on metal to ceramic closure using single phase bonding which was considered to be important. This was for the metal overpacks over -- up to I think about three-inch thick ceramic shells.

And, of course, the matching of thermal expansion was necessary. The work that was done at the laboratory last year did develop two closure techniques.

In the candidate ceramic manufacture survey there were six U.S. aluminum fabricators contacted. The listing is shown here. Favorable responses were received from those six. They agreed to fabricate half-size alumina or graphite containers.

And one of the requirements of that survey, which was initiated with a telephone call and followed up by letter, is that we would obtain commitment from these fabricators for long term participation, and we did obtain that from them.

The graphites workshop was also conducted at Livermore November the 17th, 1988. In this case there were 25 participants, 16 from outside of Livermore. And there were a whole number of issues considered. All of those that were discussed yesterday.

Some of the more important ones that is not characteristic of metals especially for the surface handling would be things like fire safety, resistance.

Things that we didn't know about would be irradiation effects, that work would have to be done.

Also the workshop discussed at length remote handling and closure processes and things like permeability to gases and liquid water.

However, the important part of that workshop were the conclusions that graphite should be considered and a study should be initiated.

In terms of the bimetals there were two types that were considered. One was to have a metal insert inside of another metal overpack. And another was double wall containers that would be fabricated by fusion bonding using standard techniques.

The concept here is that the outer shell of the container would be anodic and that would provide containment at the higher temperatures and the higher gamma dose rates.

In the event over some period of time that the outer shell would be breached and the inner shell would be cathodic to provide long term stability at the lower temperature and the lower gamma dose rates.

And there were a number of candidates that were discussed. Some of those that were considered more plausible were nickel and iron base alloys over copper and also mild or low alloy steel versus a nickel based alloy such as Alloy 825 on the inside.

Obviously we had problems with predicting galvanic attack and localized corrosion. And if it was going to be

a shell in a shell, then you have crevice conditions in between in the event that the outer shell was breached. It was, however, considered to be a very promising alternative concept.

Single metals were considered at the time because the metal barrier pass was working on the basic six materials that you saw earlier. However, we did not want to shut the door on looking at other materials. Some were called more high performance alloys.

There were materials that were not included in the basic list of six, because earlier criteria, as you heard Bill Halsey mention this morning, had a criteria of some 25 percent for cost, and some of these materials are quite costly.

However, the other material, there's interpretations of containment requirements may change over time. We wanted to have flexibility in some aspect of the program to be able to look at other newer materials.

There were other things like knowledge of degradation mode scenarios that were changing or new information that came along. Microbiological corrosion, the MIC shown there, is becoming a very important corrosion phenomenon in the country today and that may change some of the present selection.

And, also, there may eventually be problems with closure in some of the materials, and I've already mentioned that we wanted to keep the door open for technological advancements of materials.

There's a number of them that have been looked at in some degree when the program was first started that included monel and titanium. Other materials, hastelloys such as C22, I know is one of the materials that the NRC is interested in and is a material that we could consider studying.

Coatings. Very little beyond just the talking stage was done on this process, and these would be fairly thick protective corrosion resistant layers either applied on the inside and outside or on the outside wall of the container.

Some that was discussed was oxides or nitrides in the ceramic family, and metallics such as aluminum and nickel chrome aluminum in the metal area. Of course, they'd have to demonstrate close porosity, substrate adherence and possess crack and corrosion resistance. And that was about the extent of the thinking at that time.

Also with fillers, not much was done with those. Continuous or discontinuous solids that would fill the void spaces within a container, and also there was long-term protection against corrosion that had to be studied, radionuclide release in the continuous form was an issue.

Some of the candidates included the magnitide, the glass, aluminum, copper, lead and zinc were all considered. And obviously those must demonstrate compatibility, wetability and void detectability.

Well, that was the extent of the program at the time. Just to summarize, the container materials alternate concepts was established. We did provide a package for reassignment to the program to M&O.

And I might mention that those planning documents are in place. They are upgraded to Level 1 and they are available to conduct the program under the current QA plan.

DR. PRICE: I have a quick question. Who -- you might have said and I might have missed -- who attended the ceramics workshop?

MR. CLARKE: There were five individuals, I believe. There were four from the university or from the laboratory and a professor from the University of California.

Clarence, I don't remember his name. Do you?

MR. HONIG: Professor Joe Pass.

MR. CLARKE: Thank you.

Do you have any further questions?

MR. HALE: I would just like to take this opportunity to thank the Board for having the opportunity for the past couple of days to present these materials to you.

We certainly appreciate your comments and the questions that you've asked. And we will start work immediately toward preparing responses to your questions.

We had allocated time for questions for the board, and obviously we overestimated the amount of questions and some of the talks this afternoon. We're not complaining.

But if you have any additional questions, I'm sure you'll provide them to us.

Yes, sir.

DR. CARTER: Jack, I'd like to bring up one area for some discussion. I think this is probably generic rather than addressed to one individual.

But I mentioned yesterday during the discussion or the talk by Bill Glassley the business of spent fuel as well as halo waste and possible combinations of this. Of course, concern about the age of the material and obviously the heat load.

But I wanted to ask the question in a generic sense, are there other types of fuel or halo wastes that you're going to be concerned with and are there plans that are being made, or are you actually dealing with this?

I'm thinking, for example, about special cases, maybe the halo waste at West Valley, maybe the fuel from special reactors, the SNAP reactors or this sort of thing.

I wonder if someone would address that inquiry, because we've heard really primarily about spent fuel.

MR. HALE: Let me take that initially here and then I'd like to turn it over to Mike Clonniger to give you some details.

I know it does appear that we seem to be addressing a lot of our attention for the last couple of days on spent fuel.

I would like to stress, however, that we do very definitely have work underway in which we would be addressing the high level waste both from Savannah River at this time and also from West Valley. And eventually we'll be considering the fuels from Hanford.

We have a substantial effort and in characterizing the properties of all these high level wastes and there's active programs right now. Lawrence Livermore is participating in that, and we also have substantial activity going on at PNL, Pacific Northwest Labs, and then also the Savannah River Laboratories is doing some work and maybe some others.

So we do have this characterization activity underway and certainly we will be designing a waste package to accommodate not just the spent fuel but these high level wastes also.

DR. CARTER: Well, what about the special sorts of things? Now from what I know, which may be limited, I would just guess that there's some maybe 10 percent or so of the waste that may not fit into the standard categories of either spent fuel, taken as a general category, or high level waste processed material. Some that either they have a special cladding on. What about greater than Class C, for example?

There are a number of things I would envision that are quite different than these two general categories.

MR. HALE: Let me turn it over to Mike to give you some more of the details on that. We are developing generic waste acceptance criteria. And also with regard to Class C, I know that is under active consideration at this time, but a determination has not been made at this time that we will be putting the Class C into the repository.

If we do, of course, that is going to increase the job that we've been discussing for the past couple of days by a very substantial amount.

But let Mike say something more about the --

DR. CARTER: Well, this was the reason for the question, because, you know, if you just limit the discussion to what happens in the near field, some of these things I would think would behave quite differently.

MR. CLONNIGER: Oh, yeah. The -- can you hear me?

The greater Class C wastes are very cold wastes from a thermal standpoint. About all we know about them right now is that they will not go into a shallow land area.

That department has other options other than repository to dispose of them where they aren't restricted to disposing of them in a deep geological repository, and certainly not necessarily in this first one.

As far as --

A VOICE FROM AUDIENCE: We can't hear back here.

MR. CLONNIGER: As far as other fuels go, they make up a very minor portion of the inventory, I would guess way less than one percent.

There's some very exotic fuels out there, some of them ignite on exposure to air. Their chemistry is entirely different. Some are metal. Some are ceramic. Some are impregnated graphites.

I would guess that many will have to be reprocessed, particularly those that will ignite on exposure to air, will have to be reprocessed in some form. They'll be treated as special cases, different set of waste acceptance criteria developed for them. We are not addressing that at this time.

MR. HALE: Any more comments?

If you would like, we could go over the actions that have been identified as we have interpreted them and see if there's any misunderstanding there. Maybe if you have a little bit extra time you'd like to do that.

DR. VERINK: If you have the time, I think that would be very good, to give us a running list.

MR. HALE: Yes. Mike is going to use his list.

MR. CONNIGER: Okay. My understanding, we have three action items; is that it?

From Dr. North. Regarding the effects or impacts of man-made materials, you would like to know our plans for carrying out the man-made materials studies and recommend that the plan should address bounding type calculations.

We'd like the first cut at bounding answers in about six months after the start of the test, before rather than after turning out the research.

From Dr. Verink. You requested a copy of the draft of Dr. Van Konynenburg's paper on Carbon 14. Okay.

And Dr. Price would like a draft of the selection criteria. And that's all I have.

Did someone else record other actions?

DR. CARTER: Yeah. Well, someone, whether it got recorded or not, it may have been in a little bit different category, but I was interested in the report or the reports by the Material Review Board.

MR. CLONNIGER: The ad hoc corrosion panel?

DR. CARTER: Yeah.

MR. CLONNIGER: Okay. I think those were --

DR. CARTER: Well, I don't know if that's the same or not. What you say may be a part of the --

MR. CLONNIGER: Yes.

DR. CARTER: -- Board activity.

MR. CONNIGER: I was the secretary during those second series of meetings ad the proper name was the Ad Hoc Corrosion Panel --

DR. CARTER: Well, Ralph mentioned getting that material for us, so I would like to make that part of the record.

MR. CONNIGER: Easily done.

MR. HALE: Mike, if you don't have any further comments, I just wonder if Les Jardine would like to add any final conclusions for us.

DR. JARDINE: I'd just like to thank the panel on behalf of the laboratory and the technical staff who helped provid the information to you.

And we certainly will take both the comments that we recorded and questions -- more like questions, I guess, and also those that we learned during the tour yesterday. And we had interesting and direct exchanges among the technical staff and yourselves.

And you certainly have given me, and I'm sure the technical staff, some things that we need to be thinking about, how we can be aiming towards closure on some of these somewhat and sometimes open issues. And so we'll take that as a goal in order to learn from this experience. And we welcome future interactions with you and other members of the Board.

DR. VERINK: I would certainly like to express on behalf of the panel the appreciation of all of us for the considerable effort that you've gone to to make this a meaningful meeting and to respond to our concerns.

Thank you very much.

DR. PRICE: Thank you.

(Whereupon, the meeting concluded at 2:30 p.m.)

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) ss.
COUNTY OF CONTRA COSTA)

I, KATHRYN DAVIS, do hereby certify:

That I am a Certified Shorthand Reporter of the State of California; that I was duly appointed shorthand reporter to take down the proceedings in this transcript;

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IN WITNESS WHEREOF, I have hereunto set my hand this 31st of January, 1990.

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