UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

JOINT MEETING

PANEL ON HYDROGEOLOGY & GEOCHEMISTRY

AND

PANEL ON STRUCTURAL GEOLOGY & GEOENGINEERING

June 27, 1991

The Registry Hotel
3203 Quebec Street
Denver, Colorado 80207
(303) 321-3333

BOARD MEMBERS PRESENT

Dr. Don U. Deere, Chairman,
    Nuclear Waste Technical Review Board
Dr. Patrick Domenico, Chair,
    Hydrogeology & Geochemistry Panel
Dr. Clarence Allen, Chair,
    Structural Geology & Geoengineering

Also Present

Dr. William D. Barnard, Executive Director,
    Nuclear Waste Technical Review Board
Dr. Edward J. Cording, Consultant
Dr. Roy E. Williams, Consultant

I N D E X

PROCEEDINGS

8:30 a.m.

DR. DEERE: Good morning ladies and gentlemen. This is the third day of the joint meeting of the Panels on Hydrogeology and Geochemistry and Structural Geology an Geoengineering. The first two days, as you know dealt pretty much with the hydrology and geochemistry presentations. And today we are moving into the work of the structural geology or the interest of the Structural Geology and Geoengineering Panel.

Dr. Clarence Allen is the chairman of this panel. Dr. Edward Cording is consultant to us on rock mechanics and tunneling. So, I would like to turn it over to Dr. Allen for a few words.

DR. ALLEN: Well my few words are to simply to welcome you here and turn the meeting over to Dave Dobson.

DR. DOBSON: Thank you, Clarence. I am just going to very briefly introduce the people up here and then let Dr. Tom Blejwas who is from Sandia get us started.

With me at the table are Arch Girdley from the Yucca Mountain Project Office, Arch is the DOE person responsible for coordinating the underground testing program. Next to Arch is the chair of Dr. Frank Hansen from Sandia. Frank will be giving a presentation. Next to Frank, Dr. Larry Costin from Sandia and Dr. Tom Blejwas. Tom is the Technical Project officer for Sandia as well as a scientist who has worked considerably on the program over the last five years.

Tom.

DR. BLEJWAS: Thank you, Dave.

I'm going to just give a brief overview of what we call at Sandia our Rock Mechanics Program. It will predominately deal with the work that has been and hopefully will be done at Sandia in the future dealing with rock mechanics. And for those of you that are familiar with the Site Characterization Program, most of this work is contained in a single section of the Site Characterization Plan, called Thermal and Mechanical Rock Properties. That is a short title 8.3.1.15. And I am not going to go into detail for you of the way that is laid out, but I want you to remember that the work that I am going to be describing is predominately in this one place and it deals with a whole series of investigations that you are going to hear a little bit of detail about from the other speakers.

Instead of going into the detail of all those pieces, I am going to try to concentrate on the higher level view of this. What is our overall objective? What are we trying to do with our program in rock mechanics? The general statement would be that we are trying to characterize the thermal and mechanical properties of the rock units at Yucca Mountain. But that is using the term properties very loosely. As I'll show you we are really trying to deal with the way the mountain will behave in a thermal and mechanical perspective.

And even though several issues in the site characterization program point to requiring thermal and mechanical properties, the one problem that tends to be dominate in the thermal and mechanical area, is whether or not the openings, the underground openings will indeed remain stable during the preclosure period, and whether or not we'll have problems with those openings in some sense over the postclosure period. So here I have just kind of drawn up that what we are trying to look at is, whether blocks that re in the back or the sides of the drifts, whether blocks are going to befall in with time. So a lot of this work then is interrelated with design. I am not going to emphasize the design aspect of that today, but you can see that clearly you would use this in the design process.

Now you might say, what's the big deal? We design underground openings in mountains around the world and we don't have problems with most of them, we are able to handle that. Well, from our perspective, what makes this problem different and what leads to a lot of research and development work, is the fact that we have the waste generating heat. And what I've shown here is some analyses that were done with relatively simple approach, boundary element approach, and we are looking at the changes in the stresses with time. This shows over 100 years. And you can see at the time of excavation what this analysis would predict is that in the free field, you are generally going to have predominately vertical stresses with a small lateral component. And however, now as we heat up the entire repository, we see that the stress field, the principal stresses change not only in magnitude, but they also change direction. And so that by the time that you are into 35 or 100 years, what you see is that in many places the principal stresses are predominately horizontal with a smaller vertical component, and both of those tend to be larger than what we started with at the time of excavation.

Now this is for a case where we haven't really tried to reduce the heat load. So this may be thought of as somewhat of a worse case given the heating environment that we would anticipate for a repository. But in any case, you can see why this is not the kind of situation that you would typically have underground. Typically underground, once you've done the excavation, you are not going to do something to the rock that would so dramatically change the stress field.

DR. DEERE: Tom, if I may interrupt.

DR. BLEJWAS: Yes.

DR. DEERE: I made a comment on the first day about the stresses in the underground rock laboratory excavations up in Canada. And I mentioned that they had abnormally high horizontal stresses with respect to the vertical layer. And even though the granite at a depth of 400 meters is very, very good quality, the stresses have done just exactly as you have shown. They have peaked up so the horizontal stresses are causing slagging, just one slab after another, after another on the top. And there is some indication of a little crack at the horizontal, where the thing is almost entirely in tension at the present time. So the condition that we are dealing with now, and I made the comment, aren't you glad you are not going to develop the repository at this site, because the pillars and intersections, would have a lot of difficulty with that very high stress ratio. And that was in the granite.

After the heating up, your analysis shows that you will be getting also a very high horizontal stress with respect to the vertical.

DR. BLEJWAS: That's correct.

DR. DEERE: I think that it would be very good for your group to visit that mine, because that is what they had encountered.

DR. BLEJWAS: Well, I know I have been underground at that location, and I've seen just what you've described.

DR. DEERE: Did you get into the lower level?

DR. BLEJWAS: Yes, I did. Well we got into it enough to look at it. We weren't allowed to walk around at that particular time.

DR. DEERE: Would it be okay if we made a recommendation that you people go up there and take a look?

DR. COSTING: Not in the winter, however.

DR. ALLEN: Snow or the black flies.

DR. BLEJWAS: I wanted to try to give you a little bit of feel for how did we come up with our program in rock mechanics. And what I am going to describe here is really the process that is outlined in the site characterization plan, but I have to admit that it has been interpreted by me a little bit.

As you are aware, we started with the regulations and from that we've developed issues. We've developed in our performance allocation process, we developed issue resolution strategies, and then we went through the formal performance allocation where we deal with how are we going to satisfy these issues with experimental work and with the site characterization program. So there is some interplay that goes on here at performance allocation to what kind of experiments do you design and conduct? And in general what you are going to see in our program is that experiments fit into two categories, although some experiments fit into both.

Some of those experiments are designed

predominately to validate models and some of the experimental work is designed predominately to provide information for our reference information base. In other words, you need to have the data for performing analyses, the properties, but you also need to know that your models are good. And as you will see in some of the details, some of the experiments satisfy both to a degree.

Once you have both of those, then you can do design and performance analyses, you can decide whether or not your issues are resolved, and if they are not you can go back to the appropriate location in this strategy.

Well when you look at these issues and you kind of lump them all together in the rock mechanics area, what you find is that you have a lot of data needs, and they are very broad. You find that you need data for parameters for the design and analysis of the repository, which I mentioned. You need a data base for empirical design methods; something that you don't find in some of the other areas, because empirical methods are used a lot in the rock mechanics community for designing underground openings.

You would like to have, as I mentioned, experimental evidence for validating the advance analytical methods that we've developed. Another thing that really, I think a complexity that we don't often find, is that you need to have some criteria for the acceptability or failures of your openings. You are going to use these advanced analytical methods for coming up with stresses, strains, motions along joints, whatever the parameters are. But what does that mean? Does it mean that the opening is good, bad or is it somewhere in between? So, we need to consider what are the criteria for failure or acceptability of the openings.

And finally, we think that over the long run, it is very important that you have some experiments that really all of us can look at and they demonstrate to us that something is good or bad. So some of our experiments have that component to it, even though we are getting a lot of scientific information for these first four bullets, they also to a degree form a demonstration that openings will be all right.

Now in addition to having a broad group of data needs, we also have a broad group of data users. We have to satisfy to a degree the repository designers, the analysts for preclosure performance, the analysts for postclosure performance. We know that the State of Nevada, their people working in these areas will be doing investigations and the nuclear regulatory staff may choose to do analyses to verify or compare with our analysis.

So, the last thing is that well, what kind of analyses are we supporting? Well we are just supporting design analyses and modeling. And here what I've taken is a view graph from a report that we've written dealing with design methodology for drifts. And what this does is starts out with an elastic analyses and then leads you down to a variety of types of analyses that you might use to look at the stability of the openings. And you can see that the kinds of analyses we are dealing with start off on this side with the equivalent continuum elasto-plastic or elastic analyses, discontinuum, discrete joint modeling. We also will use some equivalent continuum models, sometimes referred to ubiquitous joint models, and finally we have the discontinuum models, the discrete block modeling.

Given all that, we think our job is pretty tough. We have got a rock mechanics experimental program where we are trying to collect data of sufficient breadth to support a wide range of anticipated and unanticipated analytical and empirical activities. So, you won't find in our program one for one correspondence with this person needs this parameter here; this is an experiment to give them that parameter. Or, you will, but only in very small areas. Most of it is designed to satisfy many users and several activities.

Let me go back and start for the rock mechanics program in the beginning, to about 1980 and give you a little bit of a history of what's gone on, because I think in order to understand our program, you need to look at the history.

Starting at about 1980, we started doing scoping experiments and we began doing some experiments in labs and also in G-tunnel. Most of this work was done by one principal investigator, Roger Zimmerman who is no longer with the program, but during the period from about '80 to '85, the work is dominated by Roger's activities.

In about 1982 the concepts for a suite of exploratory study facility tests was developed, and in about 1983 that suite of tests was included in the first exploratory shaft test plan and that first appeared in draft form in 1983. I might mention that the tests haven't changed a real lot since that time period. That is the reason I am going back to this history.

In about 1986, we went through our performance allocation activity. This was conducted, and we had internal review of the planned experiments. In about the '86 to '88 time frame the SCP was written, reviewed and issued. In 1987 there was an external review of the exploratory shaft test plan and then things just started unraveling a little bit.

We had significant additions first to the staff in the rock mechanics area. We went from having one principal investigator Roger Zimmerman to having four or five principal investigators, because we were getting ready to do a whole bunch of tests underground.

Then things turn around so that--well I'll get to the '90 -'91 in a minute. But throughout this time period starting in 1989, we established, we formed a peer review panel specifically for our rock mechanics program, and they performed reviews.

In somewhere around '89 to '90, the funding in this area changed significantly because of delays in the program. And because of that lack of funding, the field site, namely G-Tunnel was abandoned and hence, over the period from '90 to '91 most of the principal investigators in this area have gone onto other activities.

As a matter of fact, Larry Costin and I will be making some of the presentations. We both are doing some managing at Sandia. We would actually be the people to go out and do these experiments. Frank Hansen was a principal investigator, but he has transferred over to the WIPP program. Frank came back for this presentation because he is the most knowledgeable in this area. But right now we do not have any principal investigators working on the project in this area.

But we have, or Frank has, I should say, and some of the people that have worked with Frank, done some revisions of the tests to look at alternatives given the change in the exploratory studies facility.

Let me mention what I am going to go back to here. I mentioned this performance allocation activity. I wanted to dwell on that just a little bit more. I'll try to go through it quickly. I know after two days everyone is getting pretty tired.

In the larger slide I had earlier, we had issue resolution strategy. That leads into this. Well, what do you do once you have an issue resolution strategy? Well, that includes selecting systems that you are going to rely on. Then what you do once you've decided on what systems you are going to rely on? You determine what is the required performance of those systems. Once you know the required performance of those systems, you select parameters, the range of parameters that would allow you to show that the performance will be as you expected. Finally you determine goals and confidences for those parameters, and that leads you, and this is the hard part, I think, to designing site characterization experiments. In a good performance allocation process, there is a real lot of give and take that goes on between the people that are worried about the parameters or the model validation activities, and those experimentalists that actually conduct the experiments in terms of what do you really need and how can we do it?

We've spent a lot of time dealing with that in this particular program.

Now, let me just give you an example. It is a little artificial in that a lot more things would go into this. But, for example when we did the performance allocation we were NNWSI project and we had issue resolution 2.4, and that deals with is, will the repository preserve the option to retrieve the waste? Okay. Well the issue resolution strategy includes showing that the access to the waste can be maintained with normal maintenance, that is the drifts are generally stable while the repository is heated by the waste. Given that you are going to need to know what is a stress field around the openings. That would be one of many things that you would need to show that. But that is an example. And we know from our preliminary analyses that those stresses may be high. At that time we thought they might be as high as 50 megapascals. That is much larger now than we think they probably would be given different strategies in heating the repository. Probably a number like 30 or 40 would be more realistic.

Well you know from your analysis that one of the dominant parameters in determining what the stresses actually would be is the modulus of deformation for the rock. At least in your elastic analyses, elasto-plastic analyses the modulus of deformation is important. And you have to know that pretty well if you are going to be able to predict the stresses.

So what that led to in part was a complex approach to determining the modulus. And now that approach in the SCP includes laboratory testing of samples; it includes plate bearing tests; it includes analyses of jointed rock mass; and then a variety of validation experiments, both in the laboratory and the field. And these are the kinds of things that Frank and Larry are going to be talking about.

Now I should mention, I said that this was artificial, and the reason it is artificial is because many things in addition to modulus of deformation would have led to these experiments. That is just one of the things that we try to get out of these experiments.

And if you want to know more detail about these, as you already know you go back to the site characterization plan and then the study plans have more details. Some of the study plans in this area have been written and approved and one of them has been sent to the Nuclear Regulatory Commission, others are in various stages of completion.

But at Sandia, some of the former principal investigators may mention experiment procedures and technical procedures. This is our hierarchy of documents. We are not going to talk about these very much because this is where the details lie. Most of what we are going to present is captured in these first two.

I mentioned 8.3.1.15 of the SCP including essentially everything we are doing experimentally in rock mechanics program. Here is a list of the eight studies that we think of as making up our rock mechanics program. The first three of them, laboratory thermal properties, laboratory thermal expansion testing, laboratory determination of mechanical properties of intact rock, deal with experimental work.

I am going to just briefly mention the first three in a few slides, because what you were really interested in was the field program, but in order to understand the field program, you have to kind of understand the boundaries of our laboratory program. Generally what we have tried to do is to take a building block approach to the program in rock mechanics. I drew this up without anybody's assistance so blame me if it doesn't make sense.

I put complexity down here on the horizontal axis and then looked at experiments and analyses and tried to give you a feel for the general direction that complexity goes in the rock mechanics program. In the physical scale, you typically go from laboratory to canister to room to far-field. Size presents problems, although some of the laboratory experiments may be extremely complex. I didn't want to down play that.

In the loading area, you start out with in situ loading, but you know that when you excavate that changes the stress field and we have some field tests to look at those changes in performance. And then the thermal adds the additional complexity that we feel is the largest. I put seismic in parenthesis because in general our tests are not aimed at the seismic loading condition per se. We include them in our analyses, but we don't believe we need spatial tests to just look at the seismic.

Time is not necessarily the easiest to look at the instantaneous, but I just had an increasing scale. As you get up to years, the experiments clearly get more complex. And some of our tests are looking at periods of several years. Some of them are aimed in the long run to be performance confirmation tests, not just site characterization tests.

Finally, looking at the analyses we go from linear all the way up to discrete joints, and that is the general. We have experimental work generally dealing with all of this and we try to build on the earlier stages.

Now I mentioned the laboratory tests that I'll just give you a brief overview. The determination of mechanical properties of intact rock generally deals with compressive mechanical properties and then we use baseline conditions and then we change the environmental conditions, the confining pressure, saturation and so on.

The mechanical properties of fractures, here we are dealing with mechanical properties in a laboratory scale baseline conditions and again we vary the environmental conditions.

The thermal properties generally deal with density and porosity, volumetric heat capacity and thermal conductivity. And we also have the laboratory thermal expansion testing.

DR. DEERE: Tom, on the compressive testing, will any of that be triaxial compression tests?

DR. BLEJWAS: Yes.

DR. DEERE: Uniaxial and triaxial.

DR. BLEJWAS: Right. As a matter of fact I have a slide showing what we are presently doing, or a picture rather.

The sampling program is laid out in the study plans and generally what we try to do with the sampling program is for a particular core hole, we in advance pick out n evenly spaced intervals depending on how many samples we think we need from a statistical perspective. And then given that interval, we take a look at that interval and we try to take out a sample that is intact for our intact properties, and we try to take out samples that include fractures for our fracture properties, so, we are not just going through the whole hole and picking out the best rock we can find. We have tried to predetermine the locations we would attempt to select the material.

And in terms of the planned view of the sampling, this is out of date because it doesn't show the present drifting for the exploration studies facility, or the drifting that is planned. But, in general, what we have tried to do for not just our thermal mechanical but for all the rock properties, the hydrologic as well, is to come up with a sampling program that includes an even distribution of holes around the site. And this program is laid out in another area in the site characterization plan, so that we would get core out of each one of these holes. Then once we have evaluated that core, if we have enough data, if the statistics look good for it, we would stop. If not, we would go back and do more.

You asked me about triaxial and uniaxial, this is a testing rig that we have that is being used at New England Research at New Hampshire. Right?

DR. HANSEN: Right on the border.

DR. BLEJWAS: Right on the border, yes, that is the reason I hesitated.

They are doing presently for us some testing dealing with very, very low strain rates, and also what we traditionally call creep tests. And we view these as scoping tests to a degree, but if we do these tests now, it may eliminate the need to do a lot of testing later. We felt that with the existing core we could come up with some ideas of whether or not; (A) is creep important; (B) do things really level out with lower strain rates.

As a matter of course in our laboratory program, we also do some non-destructive testing on the samples that we test. Here we are looking at some sonic velocity measurements, P waves and S waves type of testing.

DR. DEERE: Question.

DR. BLEJWAS: Yes.

DR. DEERE: Can you do the sonic velocity under uniaxial stress conditions?

DR. BLEJWAS: Yes, we can. And I don't know if we have actually done that yet, have we? Well, you are not the laboratory testing guys, you wouldn't know. I am not sure whether we have.

DR. DEERE: I think one can get a lot of insight into the cracking and stress relaxation and such things.

DR. BLEJWAS: Yes.

Part of the plan would be that eventually you might use the sonic velocity measurements to test a lot more samples than you would do from the destructive testing. That looks very promising at this point.

I also wanted to point out to you that laboratory does not necessarily mean very small scale. This is a sample from TSw1 where you have a lot of lithophysae voids and we've done tests on samples as large as this, some preliminary tests.

So in general, the laboratory experience that we've gained involved a pretty good understanding of the rock from a laboratory perspective. In the intact mechanical properties area, we have done compression tests, dry and saturated with various confining pressures. We are in the process and we have in the past looked at strain rate effects and sample size effects, and we've also done some preliminary tensile tests. The preliminary tensile tests have shown us that we really do need to do direct tensile testing that when we tried to use the disk approach, that is the Brazilian test, that did not give satisfactory results.

We have done some testing for fracture properties, thermal expansion, thermal conductivity, bulk properties, and as a general rule we try to take mineralogy and petrology determinations of the samples that we have testing.

Going up just a little just a little bit in scale, I wanted to mention a little bit more about fracture testing because of a program that we have going right now. These are some traces that I just drew up from some samples we had of natural fractures from the Topopah Spring member; the scale being ten centimeters. And even at that scale, you see some very significant large irregularities in the rock. If G-Tunnel is similar to underground at Yucca Mountain, what you will find is that as you go up in scale these irregularities go up in scale too.

I think what I am leading to is that rocks actually moving significantly along a joint may be more of a non-problem than we have made it, because, as you have those larger regularities, they just all lock up. So we are doing a little bit of testing right now, scoping tests at the University of Colorado, where we are looking at doing testing with the shear testing with the normal force being a stiffness loading instead of a constant normal force. The constant normal force test tends to give you a misperception I think of what would really happen in the field. Because, in the field as the rock shears, if all these asperities aren't destroyed, there is a dilation that greatly increases the normal force. That normal force is going to prevent further motion. So we are doing some scoping tests in that area.

I would like to now just switch gears and to get into the main body of the talks today, that is the experimental underground program, the in situ tests. And what I've got here is a drawing for the old concept for the exploratory, what was then known as shaft facility, two shafts, to give you an idea of what range tests Sandia was planning in the rock mechanics area. So what I have done in this view graph, is to put all of Sandia rock mechanic tests in blue, Sandia blue. And you can see that we had some tests planned in the shafts and in an upper demonstration breakout room, and these were intended to be fairly early tests. The ones in the shaft were intended to look at shaft performance.

We also have tests at the main test level, that is where most of our testing is. Now, the most complex of those tests being what we call the sequential drift mining experiment where we have three large drifts that have to be excavated. We will talk about that in more detail.

We also had a lower breakout test and heater tests and a heated block experiment, and a canister scale heater tests. So a large suite here, but I wanted to mention the tests in the openings.

Well because we didn't have the exploratory studies facility, we had to go someplace else to do our early tests, and I mentioned G-Tunnel already. I am not sure--have all the Board members been underground in G-Tunnel?

DR. DEERE: Yes.

DR. BLEJWAS: I'll go through this very quickly, because I wasn't sure whether I needed to bring any of you up to speed. You know that G-Tunnel is very favorable from a rock mechanics perspective.

Just to refresh your memory, G-Tunnel is located about 40 kilometers away from Yucca Mountain, but the ash flow there is different ash flow than the Yucca Mountain ash flows. Looking at a cross-section through G-Tunnel, you may remember when you were underground that a lot of the rock mechanics testing was done in a very thin unit called the

Grouse Canyon Member. It is the location for the highly welded tuff, but it is surrounded by the nonwelded tuffs. So we had some tricky times trying to conduct experiments in the welded because the welded unit was so thin there, particularly compared to Yucca Mountain. And again that is what G-Tunnel looked like when we first finished our demonstration drift as a part of our experiment.

Very briefly, the G-Tunnel experience consisted over the years of a large number of tests. We did heater tests in both the welded and the nonwelded tuffs, because when we started this, it wasn't clear what unit the repository would be located in.

We did a heated block experiment that I'll show you a slide of in just a moment. We did a mine-by experiment that we called the mining evaluation experiment. We've done several slot tests, one of which I think you saw the remains of in G-Tunnel; several different approaches to stress measurements and we've also spent a lot of effort in equipment and instrumentation evaluation. Most of these experiments, these in situ experiments get very complex and there is a lot of developmental work in terms of the equipment.

This is what the heated block experiment looked like during the time period of when it was being conducted. It is a relatively complex experiment dealing with the isolation of a block of rock, heating that block of rock, applying stresses to that block of rock, and then monitoring the behavior of that large block of rock. This looks very complicated and it was. One of the things that we have learned from experiments like this heated block experiment is that future experiments should probably be simpler. This got to the point where you weren't sure what was causing what. So the heated block experiment that we have planned underground in the exploratory study facility would be a simpler experiment.

One of the last and maybe the last experiment that we conducted in G-Tunnel was a slot test. And here we first drilled a pilot hole and then used a chain saw to cut a slot, inserted a flatjack and pressurized that until we actually got some local failure of the rock. Again, you can see we needed flatjacks, we needed chain saws in order to do this type of experiment.

About the time when G-Tunnel was closed, we were preparing for another test that we called a thermal stress test for want of a better name. This is a type of test that really hasn't been conducted anywhere else because we have a unique problem. Namely, what are we going to do with these high thermal loadings in a fairly large rock mass? When we looked at the original plans that are in the site characterization plan for long term tests, we kind of jumped from things like small heater tests to a large room test. The large room test, we were estimating would take, three or four years or longer before we'd get any real results out of it because we were heating such a large mass of rock. We divised this test to try to heat up a smaller volume of rock, yet still in a location where you might get some type of a failure, because, I had mentioned to you earlier that deciding whether or not an opening was good or bad, giving your analysis was very important. So this is an experiment we were working on when G-Tunnel was closed. It deals with heaters in the rock and then a bunch of monitoring devices in a large part of the rock mass.

The next view slides are a little bit out of order from your package. I apologize for that but they inadvertently got shuffled. It was not the fault of the people preparing them, but my fault.

I just wanted to mention to you the types of things that we've done under equipment instrumentation development and evaluation. We probably over emphasized chain saws, but that has been a large effort for us and that is the reason we dealt with them so much. We have developed some high-pressure flatjacks that seemed to be working very well or were working well. We also developed an impression flatjack that I'll show you in a moment; multi-point borehole extensometers; we've worked a lot on data acquisition systems; and we were in the process a couple of years ago developing a laser interferometer for drift/shaft convergence measurements. We found that this was an approach that had a great promise as opposed to using actual physical devices that go across the drifts and the shafts. We don't have that fully developed yet.

Just an example of the kind of chain saw that we've developed, and you can see from the slot that they do cut nice, clean slots.

DR. CORDING: Tom, is that chain saw able to cut, or do you think it will be able to cut the repository level materials?

DR. BLEJWAS: Yes. This is a large piece of welded tuff. And it was successful in cutting this in the set-up like we have here, but we also cut the slot in G-Tunnel in rock that has about the same properties with a similar saw. So yes, we do.

DR. CORDING: I have seen used for some of these slot tests the Corps of Engineers used some European technology, this large diamond saw, basically.

DR. BLEJWAS: Right. Well, we were looking at diamond saws also.

DR. CORDING: Right. Circular.

DR. BLEJWAS: Right. The circular saws. And that may be a better approach. The development of these chain saw was not without problems. And at the time we stopped doing testing in G-Tunnel we were still having intermittent problems with the chain saws, but they were successful for cutting one or two slots before we started running into problems with the chains.

DR. COSTIN: I can give you a quick update on that.

The saw you see here is in fact a chain saw. It is very similar to a woodcutting chain saw, except the cutting is done with carbide cutters. We had a lot of problems with that for a number of reasons. The chain kept wearing out; got slack. So we investigated some other mechanisms and our newer version of this which works extremely well is based on a quarrying type chain saw that is used in the midwest, in Indiana especially to cut large blocks of limestone and is essentially a rubber belt that is impregnated with a diamond cutting stuff and it runs on a chain saw like arm.

It cuts a slightly wider slot than this, but we have done some cutting tests on welded tuff from Fran Ridge which is an exposure of the TSw2 and it just cuts through that at a phenomenal rate. In fact you can do plunging cuts with this saw which we couldn't do with the chain saw. So we can make very clean slots without having to have even the starter holes now.

DR. BLEJWAS: Larry and I have mentioned it cuts nice, smooth slots, but even in the smoothest slot what you find is that the rock itself will have joints running through it, and as you cut it you'll have some spalling. So what we developed was an impression flatjack so that we could go in with a piece of foil on an expandable flatjack and get an impression of the slot. This is the kind of thing that helps us determine how successful the testing would be in that slot and whether we need to make some modifications to the slot.

I wanted to finish off with just a few view graphs dealing with where we are and where we are going. I don't know where we are going because I don't know what the funding picture looks like for the next few years. But I'll go back to 1989 and I'll tell you what we thought in 1989 that our near-term activities were.

In 1989 we thought our near-term activities included prototype thermal stress experiment. We were planning some scoping rock-mass "strength" tests. Strength is a very ambiguous property in rock, and we had some very difficult times trying to develop some experiments in this area. We were continuing to do our equipment and instrument checkout. And we developed something that we called an unheated block. It looked similar to our heated block experiment, but simpler, something we could do earlier underground in exploratory studies facility. None of that work continued after 1989. We still think some of this needs to be done and we are hopeful that in the program over the next few years will be included.

Where does all this go when we've done all these experiments? What is the flow of data? Part of the reason I include this is we've always been the keepers of the reference information base. We keep it at Sandia right now. So we view it as an important component on the project.

Once we've done the test or the experiment, we actually archive complete data records, but we also write data reports. Those data reports get put into in one form or another our site and engineering properties data base. So, if you want actual data from the test, you can go to that data base. Then those properties get interpreted and some analyses may be conducted and information goes into our reference information base. That reference information base would also include other sources. You would take the best information you have available anyplace and that is what helps you to determine what properties to put into your reference information base. This is used in performance and design analyses and hopefully you can use it in the issue resolution.

There are though, sometimes some spatial studies that you'll do that will not use the reference information base information. That is because you want to look at a range of parameters that is outside what the reference information base presently includes, looking at alternatives.

I mentioned earlier our Rock Mechanics Review Panel. I wanted to just let you know who was on that panel; some people I am sure you are familiar with. Dick Bieniawski from Penn State, Steve Crouch from the University of Minnesota, Howard Pincus was an consultant, Jim Russell from Texas A&M, Chris Scholtz who was with Lamont-Doherty, and Hans Swolfs who is with the USGS. We have been meeting with this group of people for a little over two years. And we've had meetings on the order of like once every six months. They have reviewed this program; given us recommendations. We have attempted to include their recommendations in the program, and things have been working out very well with this peer review group.

That's the last slide I have in my presentation before we get into the next presentation which is Larry's. Are there any questions for me?

DR. DEERE: At the meetings of the review panel, do they prepare a report?

DR. HANSEN: We haven't requested them to prepare a report yet.

DR. DEERE: Okay.

DR. CORDING: Tom, I had one question on this reference information base. Presumably now the designers of the exploratory ramps in the initial facility, portions of the facility, their exploratory portions at least are using that in their design.

DR. BLEJWAS: Right.

DR. CORDING: Are there areas where there is some real major questions right now that are impeding the ability to come up with a certain of the design characteristics or--

DR. BLEJWAS: No, there aren't any major impediments to that process, but it is something that we have to pay a lot of attention to. We have to interact a lot with the designers to be sure that we have the properties for the areas that they need them. And we have done a lot of that and put a lot more things into the reference information base specifically for the design, and we are continuing to do that.

DR. CORDING: There has been quite a bit added to that in the last couple of years, is that right?

DR. BLEJWAS: Yes. In fact, the area where most of the additions have occurred is in the thermal-mechanical area, because that is what the designers need a lot of.

DR. CORDING: So you have been translating the information from G-Tunnel, taking into account of what you think is in the site to come up with some of those relationships?

DR. BLEJWAS: It is more based on--it starts with the laboratory properties that we have from Yucca Mountain. Then uses the experience from G-Tunnel to interpret that laboratory information and then we do some analyses in addition to determine for example, rock-mass modulus. Well a modulus value, you can't get that directly from the laboratory sample, so we have used the experience that we have gotten from G-Tunnel in terms of how to use the laboratory samples to come up with a value for a modulus for the rock-mass. And that's the kind of thing that is in there now.

Larry.

DR. COSTIN: I am going to go through the series of experiments, the first three of the four study plans that Tom mentioned.

The first one being the in situ mechanical properties. I think as you'll find as I go through each one of these studies, that the information presented is kind of organized in the same way for each study, so it will be kind of a repetitive thing going through each study.

The things I wanted to mention were, to give you a little bit of feeling for the objectives of each one of these studies, kind of the rationale for why it is being done the way it is being done, what the configuration or plan was as discussed in the SCP, and now how we are intending on modifying that plan to look at the advantages that we can take of the new configuration and just information that we've learned since the SCP was written about some of these tests causes us to revise these tests. I'll try to discuss a little bit about some of the revisions.

Then I'll give a brief view of what the test looks like, a short description. And then discuss a little bit about the previous experience that we've had with this type of an activity. This experience includes this sort of meeting of the testing community and the analysis community and how we have tried to weave these two together using the analysis to interpret the tests, and in fact to redesign the tests and come up with a better test that helps us get the information that we really need.

With that in mind, let's start out with the in situ mechanical properties. The purpose of this test was really to obtain mechanical measurements in properties and we were focusing here on an intermediate scale. We have as Tom discussed a variety of scales that we need to look at. You can't simply go from a laboratory scale looking at the size of samples that you can test in a laboratory to full room size scale and expect to make that translation. There are many things going on in the scaling, and we wanted to conduct a series of tests that we could control in a similar way that you could control laboratory tests. You can control the boundary conditions, you can control the thermal input, or in these cases we don't have thermal input, but you'll see tests in Frank's presentations where there are similar scale tests that you can control the boundary conditions.

You can look at the properties of a rock mass on a intermediate scale. Where you have a chance of doing some analysis of those experiments and getting an idea of really what is going on physically, so that our models can then be validated at least on an intermediate scale, so then we can move up to the next scale which is the following study plan that I'll talk about.

The SCP approach basically discussed two sets of tests. One was a plate loading test which is a pretty standard kind of thing to look at rock mass modulus. And then there was a series of tests called the rock mass response experiment, which was essentially two kinds of tests. One was to look at rock mass failure on a smaller scale. And the other one was to look at the response of joints. And as you'll see here in the next slide, we've had a lot of discussion about that second part of this, primarily because we felt that the rock mass response experiments were not very well defined in the SCP and we had a lot of problems with how they would be conducted.

Since the SCP was written, of course, we now have a new configuration and we have tried to see how we can take advantage of that configuration to do some additional testing or to modify our testing approach to get additional information. The modifications to these tests of course for the plate loading tests and for some of the rock mass response experiments is to conduct some experiments in the Calico Hills.

Now, as Tom told you, most of these tests are really directed towards getting information that would allow you to design a repository. So, the question is, why do you want to go and do any tests in the Calico Hills? And the reason is that because of the exploration of the Calico Hills, we are opening up these new drifts there, we feel that these drifts will potentially be used for long-term monitoring during the emplacement period, perhaps, and certainly during the performance confirmation period. And so therefore it behooves us to try to keep these openings, design these openings so that they are stable and to monitor these openings so that we have a good feeling that these openings can remain stable for that period of time.

So we do want to do a few tests in the Calico Hills.

A few tests in the Calico Hills, the change from shafts to ramps now allows us to do some of these tests in units above the Topopah Springs, which again allows us to get more information; it allows us to investigate the response of near faults that the ramps cross outside the block to get a feeling of what the opening stability would be near those faults. And finally, as I said, we've taken a long hard look at the rock mass response experiments and tried to be a little bit more realistic as to what we could do with those experiments than what was discussed in the SCP.

The rock mass response experiments now is expanded to three different kinds of tests. A uniaxial response which is essentially a large uniaxial compression test and a set of ambient block tests, which is similar to the heated block test except very simplified and only to look at the mechanical response and not thermal mechanical response. Then a series of slot tests.

The reason this suite of tests was selected, one is we feel very comfortable that we can conduct them. We get virtually the same kind of information from each one of these tests, but in a completely different way, and therefore we have the opportunity to blend all of these in with our analysis and come up with sort of a consistent picture of what is going on.

So now I am going to step through and just briefly describe each one of these tests. You'll notice in your view graph package that there is a lot of view graphs that have words describing some of these tests. I am going to kind of skip over those in many cases, and when I get down to discussing the tests I'll just use the figure of the tests, etc., and do some talking. Those sort of wordy view graphs you can keep for notes.

The idea of the plate loading test of course is to measure the rock mass deformation modulus. It was also because as a legacy from anticipating using a drill and blast methods for the repository, to try to evaluate the degree of fracturing around the openings and how far this damage zone might extend into the openings as a result of using drill and blast excavation methods. So this part of the test is kind of being de-emphasized now because of the anticipated mechanical mining, although we still need to look at this issue from both a pre and postclosure points of view.

Also I wanted to do a number of tests over the repository. Now that we have long drifts that go virtually everywhere into the repository block, we can get a good idea of representativeness from the testing point of view. And we are also going to conduct some tests in the upper DBR area, the upper demonstration breakout room which is in the TSw1, and main test level and as I mentioned down in the Calico Hills.

DR. DEERE: Question.

DR. COSTIN: Yes.

DR. DEERE: Don Deere, here. Did you consider for the test for the deformation moduli the use of the radial jack test, the type the Bureau of Reclamation has used, the very large scale?

DR. COSTING: Core jacking?

DR. DEERE: Yeah. It's done with flatjacks against a frame and they have one. They developed one, they have one, and it has given some just absolutely outstanding results. I've seen it in other countries. To me it is advantage over a plate loading test which I've done probably three or four dozen in different countries. It is very, very great. This is the only major comment I had on your program. It is more expensive, but the plate load tests are not cheap either.

DR. HANSEN: That's true. This is Frank Hansen. Actually with the new configuration I think that the radial plate bearing tests should be considered more.

DR. COSTIN: Absolutely. Of course back at the SCP level, where we were drilling and blasting and this would be the right test then.

DR. DEERE: Let me describe that briefly for those in the audience who may not be familiar with that test.

It tests a short length of tunnel and a circular tunnel would certainly be better for this. It uses a series of I-beams over a length perhaps of 15 or 20 feet. And they are loaded against the rock with flatjacks and with timber so that flatjacks won't break or else with a mortar. And what it does is to allow you to apply extremely high pressure that goes out through the rock mass so we can get loads--literally we'll be loading thousands of tons against the rock mass. And then the borehole extensometers which have their anchors at different depths can be placed in about eight directions.

So we are getting moduli values, not just in the direction that we are loading, but we are getting moduli values in every direction, wherever we happen to have our measurement devices. And then the anchors can be at a depth of one foot, then again five feet or ten feet, etc. And we can from them calculate very well how the modulus changes.

I am glad to hear that you are considering this. And certainly, I'm glad it is on the record. We will highlight it in the report two or three times. It will be one of the recommendations.

DR. HANSEN: I think that is excellent.

DR. CORDING: Larry, is one of your objectives here to be testing, doing some of these loading tests in TBM excavated sections or are you looking at mainly being in drifts at this point, site drifts?

DR. COSTIN: Mostly in alcoves off of the main tunnels, mainly because the initial ramps are 25 foot diameter and they are going to cut a number of side excavations from that, which would allow us to test at various horizons as we go down.

DR. CORDING: So you will probably have to test surfaces that are not the same as the TBM excavated surface?

DR. COSTIN: That's right. But with mechanical mining, you can perhaps shape and carve those rooms any shape you want, really. If you want to make them round, you can probably make them round.

DR. CORDING: So your thought would then be to use the boom excavators of some sort to excavate the alcoves.

DR. DEERE: Right. Don Deere again. Ed, that was a question I was going to ask them. Did they think that a real spatial high strength boom excavator could be used for this material. I know you did a little work in the G-Tunnel with it and it was pretty tough.

DR. HANSEN: That's correct. The success ratio in G-Tunnel was limited. It beat the heck out of the Alpine Miner. However, I think that we will be able to have small alcoves mechanically mined. In fact, we put that on our wish list to have a spatial small alcove, because we can't do these plate bearing tests on a 25 foot back.

DR. CORDING: I think there is continuing to be development in the industry in the capacity of these boom excavators. And heavier machines and even different types of even hammer type of excavators that are being used now. It seems to me that for the entire program that that investigation of that type of equipment at least finding out what is available and then some of it is just a matter of there might be some development that is involved in some of it. But certainly just enough--the development required is something that I think you could say certainly could be functional and useable by the time you are getting down into the facility. I think that that could be an important part of the whole excavation program.

DR. DEERE: Don Deere here again. I just wanted to comment on his comment. The use of the hammer, you know the hydraulic ram sort of thing, it may be being used more Ed, but in one of the drifts that we are excavating now for the large chamber under the English Channel has operating in the same tunnel the hammer excavator, the hydraulic pick going in one direction and the boom cutter going in the other. And one is going eight meters a day. That is a Westphalia, which is a boom cutter. And the other one is going between a half meter and one meter a day.

The amount of vibration that they create is really different. In approaching it, we have several drifts there, and as you approach it you can put your hand on the wall and actually feel the hammer excavator working in there. It is not very efficient but boy does it create an awful lot of vibration all over the place, while the boom cutter is just grinding away and doing its thing and not giving near the damage to the rock.

So, I am sort of in favor of the boom excavator.

DR. CORDING: If it can cut. I mean you are in the harder rock, I think you just need to--that is an area that I think we need to be looking at the best equipment that can be used for that.

DR. COSTIN: Now that you have basically described the tests I won't need to. But, I'll just show you a picture. The more classical plate test is done between flat pads and again the key element is to put a multi-point extensometer below where you are loading so that you can measure displacements between various points and get modulus measurements down into the rock. You have a set of support columns in your flatjack loading system that is incorporated in there.

I just wanted to show a couple of quick photographs of the kinds of equipment that go along with this. These were given to me courtesy of the Bureau of Reclamation from some of the equipment that they have used in their plate loading tests.

This is one of those borehole extensometers and we will be talking about this extensively in all of our experiments. This is a particular kind that has mechanical anchors. Some of them have anchors that need to be grouted in more permanent type anchors. It has several anchors, two of them are shown here, and then rods connect all the way back to a head back here in which the actual measurement is made.

Now there are different kinds of these. One of the kinds that we intend to favor is slightly different. It is based on the same kind of a mechanical anchor. But, between the anchors you actually have the LVDT that does the measurement. So you have the measurement device actually located between the anchors, so you don't have to rely on these long rods to come back to a single head where you are making the measurements. And they tend to be far more accurate and you have a lot more latitude as to how you place those anchors.

Just a quick picture of a loading test. This one is a horizontal one, but you can see the reaction columns, flatjacks are in there and this is a dial gauge you use to look at the surface-to-surface displacement.

Our previous experience with this is basically no experience. We did not plan to do any prototype tests because this is pretty much a standard kind of a test. There are a lot of experienced people around who have done a number of these. And because we were going to do a large number of them or a fairly large number of them, we figured that we could gain experiences as we did them. So, the things though that we thought were quite important, in particular, was the load capacity that along with our flatjack development program, that we certainly needed to have flatjacks that could get up into the range in which we want to look at the change in modules. In other words, we need to get up into the 30 MPa range of looking, because that is when the effective fractures or effective joints really begins to take hold and you begin to notice it. And you really begin to get large non-linear effects in the deformation of the rock mass.

Let's go on now to the rock mass response experiments. We had a number of objectives in these. Again we wanted to look at rock mass response on an intermediate scale, on a scale that we felt we could control boundary conditions and get a little bit better data and kind of make this transition between laboratory scale and the much larger field scale.

It accomplishes several things. One, it again allows us to evaluate scale effects. And particularly effects with regard to jointing. As you go up in size, the effects of joints they become and their orientation of course become more predominant. The effects of scaling on any one joint is really unclear at this time. And it provides us data in a fairly good amount of data from each test and we could repeat tests at various locations. So it provides us a very good means of validating models, because, again we can control the boundary conditions a little bit better, so we have a better means of modeling those experiments.

The duplication of information between tests I talked about a little bit, that we really are kind of getting the same information from each test but in a different way. And so the combination of all the tests gives us a little bit better understanding of what is going on.

We did three kinds of experiments: Compression tests, block tests and slot tests. I'll briefly go through a description of what those constitute.

Let's start with the compression tests. This is essentially like a plate loading test except now we are using our slot cutting saw to define a block in the floor. We can cut a slot or define a sample approximately on the order of like a meter square. We can put in multiple gauges and then we can load it vertically to look at the deformation versus load in a similar fashion to a uniaxial compression test. And we hope that we can get up to sufficient stresses that we may in fact be able to fail rock, that is in certain areas. And of course, it is going to depend on the joint structure in those rocks, etc. We can also, of course, insert flatjacks in the slots and get something that would approximate a triaxial type of loading as well.

In the second kind of test, the block tests, that is essentially what we do. We don't apply a load vertically, but we do put flatjacks in here and because of the joint structure, we can orient these blocks relative to how the joints are oriented and get various amounts of shear stress on the joint sets. So by loading in various combinations of loading opposing sets of flatjacks, you can actually shear joints or you can get normal loading on joints, and look at the normal closure of joints and the shear strength of joints. This kind of an idea and being able to do it in a fairly simple way replaces really the complex test that was described in the SCP for looking at joint shear.

In this case, instead of having of course, the instrumentation vertically, it would be horizontally in a way that was very similar to the heated block test. This is in fact the same configuration and idea of the heated block test, except that we are not applying any heat and we are simplifying the instrumentation so that we can repeat this test in a number of places.

Now we can skip down to the slot tests. Tom described this in some detail. The only example I have is from one that we conducted in G-Tunnel. The basic idea is simply to cut a slot, insert a single flatjack, or a sandwich of flatjacks depending on how much displacement you want to get and put displacement gauges at various places across the flatjack. If this is done in the wall we also put cross-drift displacement gauges to see how much heaving out we are getting, depending on the angle of the joints and how they intersect the drift.

We get various kinds of instrumentation, mostly displacements, but we also make extensive use in all of these experiments really of the acoustic emission and location devices. Primarily you can get slipping on joints or the beginning of intact failure in rock. You can pick these up acoustically. We have very nice systems that if you have enough sensors located around, you can actually locate the sources of those micro emissions. You can tell on which joints things are beginning to happen. And then you can go back and correspond that to of course your analytical solutions to say does that correspond to what our models are saying? So you get a little bit more information that way.

Previous experience in this area was that we did conduct one very complex heated block test. We know some of the things not to do and some of the things that we can do very well. We have also conducted a series of slot tests. Those were conducted primarily for the development of the technology needed to do the slot tests, namely how do we cut the slots, how do we get flatjacks to survive up to the kinds of pressures that we need for those tests. But, fortunately the last test was instrumented fairly heavily and we got a lot of data out of it, and I am going to show you a little bit of that in just a minute.

Again, the important parameters are how we orient these tests with respect to the structure in various places. We can orient them differently and get different kinds of information depending on how we want to do things. And again, the load capacity, how much load can we apply to these sort of small in situ samples?

The example, and I think from each of the three sections or at least two out of the three sections that I am going to discuss today, I am going to go through one example that we kind of worked our way through. And I think the primary reason for doing this is basically to give you a feeling of how indeed analysis helps you interpret the results of these experiments, and in fact helps you design experiments. And how this interplay between what information you really need and what you can do analytically comes into play.

One of the last tests that was done in G-Tunnel was this slot test. The interesting feature turned out to be that the slot of course was oriented at a 30 degree angle to the principal joint structure. And we were able to get up to about 30 MPa flatjack pressure, at which time there was a small failure in the edge of the slot. So we did actually fail a chunk of rock along some of the pre-existing joints. And we did a fairly extensive amount of analysis of this experiment. I would like to briefly go through some of that.

DR. DEERE: Didn't we see the area when we visited the tunnel where this had sheared off?

DR. COSTIN: That's correct. You saw this test.

I'll show this again. The only thing I want to emphasize is that when I show you data, it will be data from this particular gauge which spanned the slot just above the center line. We did some calculations to look at what our models would predict.

We used a couple of different models to look at this experiment and I'll discuss those in a minute. I just want to show you this to illustrate that of course the jointing is not very even there. It is banded. There was a lot more jointing on this side of the slot than on this side. So the stiffness of the rock on one side of the slot was probably different than on the other. And in looking at this view, the joints go into the rock at a 30 degree angle. I can show you that on a plan view here. This is the slot and these joints are set at an angle like that.

So the response of one side of the slot is going to be different than the other. This is why the interpretation of these tests requires more detailed analysis than just looking at simple elastic solutions of inflating a slot in an elastic media. The interpretation of what's going on could be extremely complicated. But, we feel we have a handle on it.

Just to refresh your memory this is what it looked like as the test was being conducted. You can't really see the slot because there is a plexiglass guard over it. But these are the gauges, the near gauges that we are talking about. We had wire gauges that went much further distances to look at response of rock. And in fact from some of these pins, we had gauges that went across the drift to look at the third dimension of outward heaving.

We did as I mentioned, we did two sets of analyses. One dealt with looking at a continuum approach to modeling disjointing structure. And we have a continuum model that incorporates the effect of joints. And this was one of the blocks on Tom's view graph at the bottom where you go from the simple to the more complex. This is somewhere in the middle there. It is a continuum approach, so basically you can model the behavior of joints, but that behavior is smeared out over an area. And this is a finite element mesh that we used to model that. Here is the starter hole that we used for the slot and the slot is in here. We just simply applied a pressure.

If you look at the results of what we've got in a highly distorted view, that is what the shape looks like. Because this is a continuum model, and we modeled the mean distance between joints was differently on one side than the other. That is why there is not really a plane of symmetry here. But the fact that you have joints concentrated here and then very few joints near the slot is not modeled. And because this is a two dimensional model you don't get the effect that these are actually at a 30 degree orientation either. The models assume that the joints are going straight into the wall. This is as I said, the displacements are magnified highly just so you get a visual view of what is going on.

If we look at a comparison now of the analysis versus what the data showed for that one gauge anyway, and we got similar results from most of the other gauges. The actual displacement measurements as we went through a couple of different pressure cycles, this one went up to about 15 to 20 MPa, and then pressure was reduced to zero. You'll notice there is some residual displacement. And then it was re-pressurized and when they got up to about 30 MPa, a piece of rock failed causing the flatjack to fail.

We did four different analyses. Same model; same everything. The only thing we were changing was one parameter and that is what was the initial aperture of those fractures, because we have to know how much they can close down as you apply the pressure. And we tried--the first analysis that we tried was using the apertures that were measured when we did the G-Tunnel heated block experiment. Roger Zimmerman had made a bunch of measurements there, and it turned out, of course, because you had sawed out this or formed this block of rock. The in situ stresses were relieved; the aperture that he measured was much larger than it would be in situ. So we got a much softer system or predicted a much softer system than what is really the case.

So, we tried a bunch of different ones just to see what effect it has. We can of course match the shape of the curves. The magnitude is a little off. Again, you are dealing with two dimensional simulation of a three dimensional problem. Most of the rest of this error is due to those sorts of effects. The fact that you are not modeling the angle of the fractures correctly; the fact that you are not modeling that there is a roof and a floor there which stiffens the system considerably as well.

One of the things we really wanted to look at was how much of an error are you making by using two dimensional simulations of really three dimensional problems? So it is interesting to compare say those results with a set of results of a truly three dimensional type of model.

This is a three dimensional version of that in which we didn't model the hole in the slot because that was a little bit too difficult, but we did model the slot and we actually put in the sets of joints as they were there by using slip planes in the model. The results from that kind of an analysis looked like this. This is the stresses as the flatjack pressure approaches 30 MPa. You'll notice that I tried to mark the slot. This is the slot here. You'll notice that the stresses are actually skewed off to one side again, because of the asymmetry of the problem. And the maximum stresses are off of the line of the flatjack to the right. And in fact, if you will recall from looking at the experiment in G-Tunnel, it was in this lower area here where you do have one of the maximum stresses that the rock failed, in fact pushed out a chunk here from between those sets of joints.

If we make a quick comparison of displacements in the truly three dimensional case, we do a much better job of modeling the data. The only thing we changed in this set of calculations was the friction on the joint itself. We tried two different size models just to see how big of a model we needed to use. It seemed our small model was big enough. So that didn't make a whole lot of difference. We are able to calculate those results reasonably well.

I am going to close this section by just pointing out that not only is there an intimate coupling between these field tests and the analysis, there is also a coupling between what we are doing in the laboratory in these field tests. And I tried to point that out by this slide, showing that we are trying to compliment each other.

From the laboratory, of course, we can do many more experiments than we can in the field. We get a little bit better statistical competence than we can get from the experiments that we can get in the field. Spatial variability, the reason we went to intermediate scale, fairly simple experiments was so that we could replicate them in a number of places to address this issue a little bit better.

That is why I have a maybe in there. Certainly the laboratory test is designed to do that. And again, code validation, we really need these intermediate scale tests and the larger scale tests to have any confidence that our models have a predicted capability.

Laboratory tests we are conducting some laboratory tests that are aimed at model validation. I think there are some tests being conducted in my opinion quite clever to look at some of the details of how you model joints. At some other time we can get into that, because I think there is a lot of very interesting work going on in that area.

Just as a reminder, those are the four sets of laboratory experiments that kind of correspond here. And with that, I will close this part of the talk and we can move onto the excavation investigations portion, unless there are any questions.

DR. DEERE: Yes, Don Deere here, I think it would be good to ask for questions by any members here or anyone in the audience before we leave his mechanical testing.

I have one then. While you were discussing the various kinds of tests and taking into consideration the advantages that we had mentioned of the radial jack test in the tunnel. I don't know if it is too big to be your so-called intermediate test. It is certainly slow and expensive and heavy, but worthwhile. There is however another test I saw performed in Yugoslavia, in fact I have a picture where I am standing beside the test apparatus and it is slightly more round than I and a little taller. I think it was probably on the order of about a meter or a little less in diameter and about two meters high. And it was the borehole deformation gauge. They used it for their design or lining for high pressure hydroelectric tunnels. They devised a method of boring and removing a cylindrical sample, it was just really a large core, down to a depth of about two and a half meters, and then dropping in their device which they could then inflate. At three different levels it had diameter changes at 60 degrees so it gave the same information.

What brought this to mind, in our field trip two weeks ago up into Canada they had just finished drilling a 1.2 diameter hole in the bottom of the drift. Now that is the size of the canister that they are thinking of in the repository, or a little bit larger, I mean the canister and then they have three buffer zones in between. They did that with just a normal diamond bit. It took them awhile, but gee, it was just a beautiful looking hole.

If you have one a little smaller and a gauge on the order of a meter or 80 centimeters and once you had the drilling equipment, it is very portable, very set up to do the thing, and then the gauge is also very portable, I mean the device once it is designed and built. And it would allow you to get quite a number of tests in different orientations which might prove to be a rapid intermediate type test. And of course, to be meaningful, the jointing has to be somewhat smaller than the diameter of the test thing, so you could use it in areas where the jointing is a little closer and you could compare perhaps the in situ value determined by that with any other method that you might get by trying to interpret seismic velocities or attenuation of shear wave, other things that allow you to get a sample and then extrapolate across a large area.

Again, not a strong recommendation; just a point that you might want to consider it. I think this was Professor Kedzunzic published in the Conference of International Commission of Large Dams. Probably about 1968 or 1972, or something like that.

DR. BLEJWAS: Would you also recommend that we go over to Yugoslavia and take a look at it?

DR. DEERE: Not at the moment.

DR. HANSEN: Don, I have a question on your radial jacking test. What sort of pressures would one obtain typically?

DR. DEERE: I think you can go to about probably 100 psi which is about what you want.

DR. HANSEN: 100 psi doesn't seem to be very high, not for activating joints. In fact one of the problems with the plate bearing test was that in our opinion we needed to have increased axial load compared to what the U.S. Bureau of Reclamation typically uses, which is 1000 psi to maybe 2000 at the utmost. We were thinking more in terms of pushing that limit upward so that it becomes a very large, massive, unwieldy type of a structure. In fact I had to differ slightly with you, Larry, inasmuch as I think we do have to prototype the plate bearing test simply because we are changing it manifestly in terms of axial load.

DR. DEERE: Well, one of the difficulties we've had when we try to have these tests done commercially, not the radial but the plate bearing test is, to get to the high pressures they almost always decrease the size of the bearing plate. And this defeats the purpose. I think we get more erroneous results from plate loading tests because of the scale effect. It just doesn't cover an area large enough to incorporate. But it is a question also being able also to create the pressure. I don't think this is a limit. I'd like to do some calculations and see what's the largest that we have used before.

DR. CORDING: Basically it is limited by--it's a flatjack test and as long as you've got a reaction, you can do whatever you can do with a flatjack. And I am not sure what the actual experience has been, but it may have been higher.

DR. HANSEN: The geometry at least is conducive to taking larger stresses.

DR. COSTIN: Okay. Well the next one is Excavation Investigations.

The original objectives of this study was to again provide early data for model validation and to look at the behavior of the rock mass now on a larger scale. The secondary reason is to look at the extent of the stress-altered region around openings. This was primarily intended for the shaft because it was drill and blast constructed and it could potentially represent a pathway for water migration if the damage zone and the stress-altered region were such that it increased the permeability in a larger region around the shaft. So, we were looking at what was the extent of the stress-altered region in some of these openings. And we also as Tom mentioned this was kind of a demonstration of constructibility of these openings of repository scale openings.

Just to get our definitions straight of what I talk about disturbed zone, again this was aimed and this view graph really comes from the days when we were looking at shafts, but it is equally applicable to ramps if they are tunnel bored, except that the zone will probably not be symmetric concentric rings. At any rate, at that time the drill and blast technology, we anticipated a fairly measurable zone of damage around these openings. It was an intermediate transitional area in which the permeability may be different, although mechanical properties may not be too different, and then certainly there is a stress-altered region that goes to some extent around there. If you look at elastic solutions, of course, it is related to the diameter of the hole, a couple of diameters away and you are pretty well out into the natural state.

But this is what we wanted to look at and also because of the joint structure imposed on this, what really could we expect to be the extent of those disturbed zones?

The SCP approach and in fact the approach that is in our as yet unrevised study plan, is that there were three basic experiments. One was to look at convergence measurements in the shaft as it was constructed. There were two demonstration breakout rooms; one on the upper horizon of TSw1 to look at construction in the high lithophysal area, and then one on the main test level. And then there was a sequential drift mining experiment in which we really wanted to focus on what the response of a repository scale drift would be in the Topopah Springs.

Well as you can imagine, this set of experiments is being revised considerably. Of course the shaft convergence has been renamed now to access convergence. That way no matter what happens we won't have to change the name again.

And also because of the change from a drill and blast excavation method to predominantly machine mined. There is probably less emphasis now in looking at what the short-term response is. We probably will not have direct access to the working face to install instruments as we go along. We'll have to do that behind the mining equipment, therefore, we are going to lose a lot of that early response data.

We have, however, the opportunity to put more emphasis on what is really the response of the rock near--especially near faults and things that you go through in the ramp in getting down to the TSw2 and above the TSw2. And a look at a little bit more of what the altered regions might be influenced by transecting these faults.

And instead of doing in situ stress measurements out in front of the excavation, we will probably now have to devise some scheme of doing them from angle boreholes that go out into the rock and allow the mining to go through.

The Canadians have done a similar set of experiments using this, and they feel that they are reasonably successful in getting data of that kind.

The demonstration breakout rooms, the upper demonstration breakout room, we do need a test area in the TSw1 to look at response in the high lithophysal areas. The lower demonstration breakout room is kind of going away, I believe. I don't believe there is any real reason to have that anymore, primarily because you will be constructing this large ramp all the way down there. You will have a lot of experience in constructing in the Topopah Springs before you even get to the main test level. So there is no reason to do that early demonstration of constructibility and to look at where the joints are before you start constructing the main test level. You will already have that information by the time you get down there anyway.

The sequential drift mining, the only major change is, of course, now the idea is to simulate as close as possible what would be done in the hypothetical repository, and of course we are changing from drill and blast to for sure mechanical excavation.

So let's go through quickly again these experiments. The objectives of this experiment was really to look at the response around openings as they are being mined. Again the emphasis here is now probably to address more in the units above the Topopah Springs and where you cross faults, and also to look at changes in in situ stress as you are conducting this mining, primarily to look at what the extent of this stress-altered region is.

A nice thing about changing from round shafts to round ramps is you don't have to change your view graphs very much. If this were a ramp and not a shaft one could visualize that the instrumentation layout would be very similar to this. What we had envisioned was a number of stations in which we would collect across the excavation displacement measurements using tape extensometers or other means, and a series of MPBX gauges that would be installed for long-term monitoring of the movement of those openings.

There would be a number of these stations installed in the original SCP. We had three stations at various horizons. Now that we are in a ramp we are also going to investigate those same horizons, but we will probably install more stations than that because we will be crossing a number of geologic features of interest that we would like to monitor.

DR. CORDING: Larry, how far back do you expect to install these behind the--

DR. COSTIN: They will be installed probably behind the trailing equipment of the TBM as it goes down the ramp. I am going to get--based on some analysis, I am going to discuss this point a little bit about really what kind of information you can expect to collect by installing instruments a fair distance behind where you are excavating. There are some problems with that that I'll try to address and that is why I said we are de-emphasizing this sort of early measurements, because we are just not going to have the opportunity to do that. And even if you can install instruments in the working face which is one of the points I am going to make in a minute, you really also miss a fair amount of what is going on. And, so that is one reason we feel at least the sequential drift mining is a far more relevant experiment to looking at that kind of information then that these.

This is primarily to get a very early look as you are excavating especially in the upper horizons which you don't have another opportunity to look at, and to install some long-term monitoring capability.

We haven't really prototyped this particular experiment although we do have a fair amount of experience from the various investigations in G-Tunnel of making the kind of measurements that we intend to make. And we did do a fairly extensive series of analyses of which one, I would like to allude to right now to kind of make that point which I've just talked about. And, that is we did look in quite a bit of detail at what the effects of sequential excavation would be and how you would get your instruments to the right location to measure the things that you wanted to measure if you were installing them as you excavated. So this was a finite element calculation really of a shaft excavation. This area in here is the excavated shaft.

What we did was we sequentially removed these elements maintaining of course the proper in situ stresses applied, etc., and looked at what the deformation was as we progressed in the mining sequence. The results of that came up with two interesting things, which I'll show you.

One relates to where do you need to place your gauges for stress measurements in order to pick up something, pick up a change in stress that you can then relate back to. How big is this? Is this stress relaxation zone around the opening?

This is a curve of how the vertical and the horizontal stresses change as you progress in front of a mined opening. And what we were looking at, at least in the shaft configuration we were looking at going ahead of the shaft approximately ten meters which would be a point here, and then excavating which would bring you back along this curve down to a point about five or so meters away before of course the next blast round would destroy your information or destroy your instruments.

It seemed to us that you were looking at pretty small changes in stress doing that kind of a technique. That is why we now prefer to perhaps go out in the ramps anyway at an angle. Perhaps we can without destroying our sensors here we can pick up this whole curve and be able to match that better. That seems to be the technique that Canadians are using and works quite well.

The other thing related to the sequencing is, suppose you put in your displacement gauges right at the working face and you put in say cross drift pins and do some tape extensometer readings right at the front face and then go ahead and mine.

Well if you look at the displacements out in front of where you are mining, if you are a fixed observer out in the rock ahead of this working face and it goes by you, this is the displacement picture that you would see.

When the working face is right at your location where you can install the instruments, in fact about 50 or 60 percent of the displacement that is going to occur has already occurred. And so what you are actually measuring after you install the instruments is only this remaining 40 percent or so. And that in this rock with this stiffness is a very small amount. It's on the order of a millimeter in a 25 foot diameter drift.

The accuracy required in order to pick up this information bothered us quite a bit. That is why we are not crushed by the fact that we can't install instruments at the working face because we didn't feel that really overall that we would be able to interpret that information any better than installing these long-term monitoring and just looking at it over a much longer term.

That early time response is just going to be very difficult to pick up. Although, as you'll see in the G-Tunnel experiment, I'll show you some data from that, we were able to pick up changes as we went along, especially when we crossed a fault.

The demonstration breakout rooms experiments as I said there was going to be an upper and a lower one in the shaft. We still maintain that we do need to look at the upper area in the high lithophysal area at TSw1. There were a number of reasons for excavating that room to begin with, in that there were other tests going to be performed in that area, so the opening was going to be constructed anyway. But, it does provide us again with a means of instrumenting the rock mass as we mine an opening. It gives us an early look at what potentially how to orient openings more favorably, so by the time the ramp gets down to the TSw2, we have a better picture of what is going on. There is lot of advantages of doing this in a time frame which you can actually get this information before you reach the lower horizon where you want to do more extensive drifting.

That experiment is pretty much intact.

I'll give you an idea of the scale of that experiment is--of course now, these are hypothetical drift shapes. We really haven't settled on what particular shape it is going to be. We do pretty much insist that it be mechanically mined because this is intended to be a demonstration of how you would do mining on repository level and get an early look or at least in the MTL. If you are going to mine most of the MTL mechanically, certainly you would want to do this by the same method.

A pretty standard suite of instruments; again we have these measurement stations to install at various places sequentially as the drift is mined. And we look at, as soon as possible anyway what the response of the drift is.

This is in the TSw1 again. We are looking for a zone that is a high lithophysal area, and so we need to pick that area and to try to orient it with respect to the structure, etc., and then look at the response.

Again I allude to a previous experience. It all relates back to G-Tunnel in which we did a demonstration drift of this kind, installed instruments as it was constructed and of course in that case it was done by drill and blast and the ground support that was installed was comparable to what we expect in both TSw1 and TSw2.

Finally we looked at the sequential drift mining experiment. This is kind of a cartoon of how the experiment goes, so it is probably just as easy to describe it here as anywhere else. We intended to make two instrumentation alcove rooms, put instrumentation into this central pillar and then mine an additional drift and monitor those instruments as we went through.

Again, the objectives of this experiment are more directed towards model validation and again looking at what is the disturbed zone around these drifts? Because now we can install instruments from these side alcoves, we can determine what the state of the rock is before the mining is done and then look at how it has changed and where it has changed. So this is really a major focus of this experiment. We will get a lot of good information again to support our model validation effort.

And it does serve essentially as a demonstration that these openings can be made and supported for long term stability, although again, a little bit more de-emphasis on that because of the fact that you are doing so much more construction now on the main face level than we were in the old concept that there will be plenty of opportunities to do that.

The other point is that these same sets of rooms are going to be used later for a heated room experiment which Frank will talk about. So we have really a set of baseline information of what the changes in the rock were due to the mining, and then we'll have another set of what those additional changes are during the heating up the rock around these drifts.

One major point I wanted to make in all the word view graphs that follow is that on this one, is that really, the mining of the center drift should be done as though it were going to be a repository emplacement room to get something of the same scale, the same construction technique, and to provide the same sort of support one would provide for those kinds of rooms.

This is kind of a cartoon of the instrument layout. The types of things we are going to measure, of course, are the changes in stress state, changes in permeability, displacement measurements as close to the drift, and away from it as we can get. These angled holes here were inclinometer holes to look at changes in inclination in those holes. Based on our G-Tunnel experience we feel that is probably a pretty useless measurement to make. We don't think that we are going to get anything out of it. We certainly didn't get anything out of it from the measurements that were made in G-Tunnel. But it points out the usefulness of being able to try out some of these things beforehand. You can really get a good feel for what is going to work and what is not.

Previous experience, I am going to talk a little bit about this demonstration drift experiment which was sort of a mined-by experiment. We did look at rock mass displacements. We did try to do cross-drift convergence and measure stress changes and permeability changes as the drift was mined.

If you look at what that experiment looked like, the geometry was something like this. There was a pre-existing drift down here called 12-Drift. And before this drift was driven, the demonstration drift was driven, we installed some instruments. This was a multi-point borehole extensometer, and there were some permeability measurements made along those boreholes before these were installed. But, along this drift, there were a number of stations in which we angled the hole up here to look at the displacement from an alcove here, as the drift was mined.

We also installed instrumentation at the front face at a series of stations and I have highlighted in orange here the two sets of instruments that I'll show you some data from, and of course the corresponding analysis. We can't forget that.

If you look at that in the plan view, the interesting thing about the experiment was that we crossed this fault here in which there was on the order of a meter or so offset--it was several meters. But when I show you the data we have a station here at Station B and station at Station D, and you'll see the change in displacement, etc., as you go across that fault. That is what the drift looked like. I think Tom already showed that picture.

This is what we got from the cross-drift tape extensometer measurements, both vertical and horizontal. As you can see the difference between after the mining went far enough away that you got sort of a steady-state response, the total displacement that you measured or change in displacement that you measured at Station B was a lot different than Station D. As you got further away, you'll notice the further downstream stations go back down in this region and a similar kind of response over here. So there was a change in how the drift responded near that fault from one side of the fault to the other.

We only did two dimensional calculations of this experiment. We did not include the fault. What we tried to do was to take some kind of an average cross-section and see whether we could predict the response of this experiment and then we sort of--the number of stations that we used, we took kind of the average response and compared that to what our calculations were.

But in this cross-section, we did make a concerted effort to include all of the different geologic horizons that were intersected by this drift, including some rubble zones that were fairly week. And there was a large change in material properties across these units.

The way the analysis was done was to try to simulate again this excavation sequence. So because the models that are used of course are path dependent and so you have to have the right path. The entire mesh was laid out and then the lithostatic loads were applied so that it came to equilibrium. Then we mined out or killed off these elements in this drift and again let things come to equilibrium and then we mined out this drift. So that is sort of the view of the deformed shape, if you will, of what those drifts look like after both have been mined. Again, highly distorted or highly magnified in order so that you can see it visually.

Looking at comparisons of data, this is from a string of extensometers that was emplaced from the 12-Drift. That was the line that was marked 7. And this is at Station C and E. This is the station near the fault and then one downstream from there. And as you can see, what we did was we normalized everything to where the collar of the displacement gauges was in the 12-Drift. And so we took our calculated displacements and matched them to the collar displacement and then tried to match or tried to look at what the comparison of the displacements predicted and those measured at the various anchor points. There were four anchor points along that displacement gauge string.

Now you can see even the difference between Station C and Station E, is quite a bit of variance in those. But the general trend and the magnitude is reasonably well predicted.

This is the kind of exercise that we need to go through many, many times in order to build up this level of confidence that we do have some predictive capability in our modeling.

To look at again the comparison of measurement stations that you install inside the drift as you are mining it with what is predicted, the calculated one is the total calculated displacement. Again, this time normalized to the end anchor which is about 15 meters out in to the rock. The drift wall then would be here. What we calculated was the displacement and by that I mean the difference in displacement between not having the drift there and having the drift there.

What you actually measure again is only about 40 percent of that, 40 to 50 percent of that, because you've mined part of the drift before you can install that instrument, and so you are only actually capturing about 50 percent of that displacement. But we wanted to make that comparison just to see what the difference was again showing two different gauges, one at Station C and one at Station E. And this is a string that went out on the left rib.

Again we measured a fairly large or convergence inward whereas actually the models were telling us that there should be very little, but there should be some movement in the rock out further away from the drift down in here, which we did pick up a little bit of that. But actually, the largest amount of convergence comes right near the drift wall is what we measured.

And with that I will close this set of experiments and again I'll turn it over to you, Don.

DR. DEERE: Any questions?

DR. CORDING: Just one question on the last analysis. Did you--what sort of modulus fit that data? What sort of E for the mass were you using?

DR. COSTIN: We did a number of calculations. One is using what we call a compliant joint model which is a continuum model that incorporates the joints. So the modulus we put in is the modulus of the intact rock that we measure from laboratory experiments. Then we put in the other parameters we need are again this joint aperture, what the closure of the joint looks like as a function of stress, etc., so that the joints are modeled explicitly in there but the effect of them is smeared in a continuum sense.

The other kind of modeling we do, again to use a simple elastic and put in a rock mass modulus. Based on our experience, we have taken the laboratory values and used about 50 percent of the laboratory value as a rule of thumb rock mass modulus. Some experiments we've been able to model reasonably well with that; some we have not. It goes all over the place.

Comparisons between what we calculate with the compliant joint model and with that, turn out that in many cases, the slot test especially, but you've got to understand that these were drill and blast excavations. The slot test was only measuring what was going on in like the first meter so it is pretty degraded rock. We actually measured a modulus that was something like 25 percent of the intact modulus.

DR. CORDING: I think that slot test gets involved with surface effects and boundary effects where it is not a continuum.

DR. COSTIN: Very much so. That is why you have to be a little careful in your interpretation of all of these kinds of tests. Why we have gone to really great lengths to try to model these in many different ways to look at and to see if we can really understand the data that we are getting out of these tests.

DR. DEERE: Anymore questions from the audience?

MR. VOSS: Charlie Voss, Golder & Associates. Were any permeability tests done, air permeability tests done in association? Could you discuss what the results of those were?

DR. COSTIN: Yes. Maybe Tom can discuss them better than I can. I remember the general trend, but I don't know the details.

DR. BLEJWAS: Actually we did water permeability tests. Those gave the best results. I don't remember any air permeability tests. In the water permeability test we actually took permeability measurements from the lower observation drift before we did the excavation, and then went back in and it was in general terms, there was not a clear change due to the drifting until you go within about a meter of the drift. So that any real disturbances, any real changes in permeabilities, seem to not occur further into the rock in general. There were some locations where things were different, but that is generally what happened.

MR. VOSS: So far out did these effects--could you see the effects or you couldn't see them at all?

DR. BLEJWAS: You could see them on the order of about like a meter from the face or so. And like I say I'd have to go into it in a lot more detail to explain--to show you the variation. There was a large variation in how much the effect was.

For example, as I recall, closer to the fault there was a bigger change. But in most locations there wasn't much of a change once you got about a meter or so into the rock mass.

DR. WILLIAMS: Were these permeameter tests or in situ?

DR. BLEJWAS: They were in situ.

DR. WILLIAMS: Injecting water?

DR. BLEJWAS: Yes. Two packers injecting water.

DR. WILDER: Dale Wilder, Lawrence Livermore.

Larry, I was intrigued with your sequential drift mining. It looked very similar to what we did there at

Climax.

Just as a comment that you might consider, as we looked at our data as we crossed some of those shear zones, it was very difficult for us to interpret some of that, and I am sure you are well aware of the problems we have had with wrong signs and so forth. We wished that we had placed a displacement gauge down the length of the drift where we could monitor any shear displacement. And I would just suggest that for your consideration.

DR. COSTIN: And that is in fact in the plans, I believe.

The reason for that, and in fact in the demonstration rooms, our reason for doing that was maybe a little different than your reason for doing it. Our reason for doing it was to try to look at the two dimensionality of the problem so that we could have some confidence that if we did two dimensional calculations, that we were in fact ignoring anything that was going on in the third direction.

It also gives you a little bit more flexibility in being able to interpret funky results when you get them.

DR. DEERE: Thank you. Let's take a coffee break, back in ten minutes.

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

MS. EINERSEN: Do we have a James Winslow? James

Winslow? I have an emergency message. Does anyone know a
James Winslow?

(No response.)

MS. EINERSEN: No. Thank you.

DR. DEERE: Well, we're back with you again, Larry.

DR. COSTIN: This is the last of the three investigations that I'll be discussing. This one kind of almost falls into the category of more of the same and many of the things you will see discussed in this investigation really are intended to be a longer term focus and a longer term point of view of the things that we've already discussed. Monitoring drift stability, looking at the excavation methods, et cetera. Really, the intent of this study was to look at the ESF facility as a prototype for a repository and do some monitoring of stability of openings, especially included the openings that are going to have some thermal loads applied, but in general, over the entire area, monitor the stability of openings, monitor the methodology used in excavation. Originally, in the SCP concept, of course, drill and blast, it was a very systematic look at blast patterns, how you would do the smooth wall blasting and there were a lot of experiments involved in doing different kinds of smooth wall blasting, different kinds of patterns, and et cetera, to look at various orientations with respect to the geology, how best to excavate and support those openings. That particular investigation, of course, has changed considerably to more looking at same kinds of effects, but with different machine mining methods.

The one thing that's not really geotechnical that was added was to look at the ventilation and also do monitoring of ventilation in the ESF as a means of validating the ventilation codes that would be used to design the repository.

DR. DEERE: Did you hear some of the discussions--I guess, you weren't here the first day, but you know about the phenomena of the air blowing out the borehole, et cetera, et cetera. We'll probably have a little of that, don't you think, as we come in and short-circuit with our tunnel to the atmosphere? I would think we'll probably get a pretty good natural ventilation going there.

DR. COSTIN: I wouldn't doubt it, at all. Diurnal breathing effects in the mountains are, I think, pretty well documented.

The objectives of the study really were to do some long-term performance monitoring and, to document those, document the construction methods used in the ESF and to collect data for ventilation. And, this was divided up into sort of four sub-activities which is in the SCP which related to monitoring of the drift stability, again evaluation of the mining methods which was a fairly complex study to look at drill and blast excavation in the Topopah Springs, evaluation of the ground support systems, again to look at various type of ground support and to look at the methodologies that one uses to select ground support, and again the air quality and ventilation surveys that would be done. One of the main elements of that is to look at the radon gas emanation.

I'll go through these fairly quickly primarily because they were intended to be monitoring tests. The instrumentation that would be installed is very similar to that we've already seen. So, there's no reason really to dwell on it very much. The post-SCP modifications, of course, is that we needed to revise the scope of the mining methods evaluation and we are doing that. And, we are also, because we're going there, going to monitor what's going on in the Calico Hills.

The drift stability experiment, really the main point was to along with the intermediate scale and the larger scale experiments was to try to develop some kind of a criteria to look at drift stability. How do we asses that over the performance confirmation period? Also, techniques used to monitor stability in the repository, this is a test bed to look at instrumentation arrays that we would install in the repository to monitor drift stability primarily from the retrievability issue point of view. Again, what instrumentation can we install that would allow us to identify impending instabilities?

So, it was basically looking at monitoring type instrumentation, settling on what kind of monitoring instrumentation we would really need to assess stability of drifts, the geotechnical kinds of instrumentation that you've seen already on a number of viewgraphs, basically to look at convergence, look at movement in the rock mass, acoustic emission devices and other such devices would be added.

One area of interest which we haven't really discussed before and that is looking at intersections which is usually one area where you tend to have stability problems if you're going to have them, at all. And, we're looking at different arrays of instrumentation to look at intersections in our drift design methodology. One of the analyses that's called out for in that is to do full 3-D analyses of some of the different kinds of intersections that are anticipated, especially as the heat load is applied to those intersections. But, in this case, we're going to monitor movements in various directions of those intersections and see whether or not we can predict or anticipate any instabilities that might occur.

From a mining methods point of view, the goals, when we were considering drill and blast methods, there was a fair amount of work called out for and described in the study plan. Some of this is being preserved. Of course, we would still like to look at what is the range of ground conditions over which various mining equipment can go through, what is the amount of damage done to the near field as a result of construction, but these goals have essentially sort of changed these--really, the goals based on mechanical excavation is pretty much related to a demonstration of constructability--and to do some documentation as far as excavator performance, so that we can do better design and develop better costing information for repository construction.

And, the ground support systems, the idea is to document the different kinds of classes of ground support installed, look at rock mass quality relationships, and again relate that to the performance of those drifts as far as stability goes. We will install periodically rock bolt load cells and we will have stress measurements in the rock to look at the performance of ground support and to evaluate what the--to do some analyses to evaluate what the interaction is between a ground support.

That primary feeds looking at being able to update our design methodology that's used to select ground support. There's really very little experience from an empirical point of view or case history point of view for mining large openings in welded tuff. There's virtually no experience of what happens when you apply heat loads to that. So, the methodology is kind of in its infancy right now. There is a report going through the review system right now that describes the methodology as to how one would step through a logical sequence of things to do to select ground support based on rock mass quality classifications and things like that. And, we want to try to validate some of the concepts that are in that methodology.

The air quality and ventilation, a number of activities. One of the primary ones was to look at radon emanation, looking at heat balances, surveys, and to try to determine or get some idea of the heat transfer coefficient to look at what the effect of ventilation would be during the emplacement period, how much heat can you remove from the repository during the emplacement period? Dust generation, look at development of friction factors, again those relate to designing your ventilation system, size of fans, et cetera, that you need.

To conclude here, the previous experience. We've done all of these kinds of things save looking at the ventilation problem in the various activities that we've already done in G-Tunnel. We've done the same kind of monitoring activities that we would like to do on a smaller scale. In the demonstration drift, each mining round or each round that was mined, different kind of rock support was installed or various kinds of rock bolts just to look at what the differences were. We tried out various kinds of rock bolt load cells, things like that. Get some idea of what we would like to use in this type of an evaluation. The other instruments, of course, are very similar to the ones I've described previously.

And, I think that's all that really needs to be said about this particular investigation. If there's any questions?

(No response.)

DR. COSTIN: Okay. I'll introduce Frank Hansen who will carry the ball from here.

DR. HANSEN: Well, I'm very glad to be here. Finally, we get to present the thermomechanical rock mechanics investigations to the NWTRB. I've been on this project for about three years, I guess, not quite. I've worked for Tom Blejwas originally, then I worked for Larry Costin, and now I don't work for either one of them. I'm one of the last sentries on duty on this Yucca Mountain Project and, most recently, transferred over the Waste Isolation Program where I am a principal investigator for large scale seal tests. So, I'm not currently active in this, but I came down to Sandia to work on this project, specifically, because I think the thermomechanical rock mechanics program on Yucca Mountain Project had the potential of being a showcase for the entire world and has some of the best rock mechanics going, especially in the United States. So, I hope that we get this reinvigorated. I also don't intend to take 40 minutes on this particular talk because a good deal of the framework has been set by Tom and by Larry and I don't think that we have to go into excruciating detail. So, we might pick up the pace a little bit. Interject questions as you wish.

The in situ thermomechanical investigation comprised one of the WBS elements and within this particular study are five tests that we consider thermomechanical experiments. The purpose of these investigations, of course, are to provide some sort of validation for the computer codes. These include, of course, heat transfer and thermomechanical results. Also, we need to assess the rock mass thermomechanical response in increasingly larger scale. This is a theme that Tom started and Larry continued in the idea that you start somewhere, you have to have all scales of information. So, these tests will be all scales of information, excluding, of course, the laboratory thermo-properties which are separate and in addition to these tests. We also eventually want to end up at the scale of the repository.

The SCP, as you remember Tom's first slide, has some history to it and so we have a certain amount of history that we inherited and some of that is good history, some of that isn't so good. But, those ideas were conceived a long time ago. So, those are cast in the SCP. They're not necessarily cast in stone. The SCP has five experiments called out and, just to use the same phraseology they use, they have a heater experiment in TSw1. Then, they have a canister-scale heater experiment in the repository horizon which is TSw2. Thermomechanical units, I assume everyone is familiar with those; Topopah Spring welded tuff unit 1, Topopah Spring welded tuff unit 2.

The third is the Yucca Mountain Heated Block. They call it the Yucca Mountain Heated Block to differentiate it from the G-Tunnel Heated Block. They're the same thing inasmuch as that information from G-Tunnel Heated Block experiment was transferred into the SCP. Okay?

The thermal stress measurements is a little more ambiguous and very, very interesting. We've changed it considerably from what's in the SCP and probably that isn't the best thinking on the thermal stress type of an experiment today. And then, lastly, of course, is the heated room experiment. Most of these five things have been touched on a little bit by both Tom and Larry and so I will just give them the broad brush.

Some of the modifications I've already mentioned, that being the thermal stress test, isn't what is in the SCP and the heater tests are conceived in the SCP to be horizontal and vertical, I believe, two orientations. And, now we're going to put them vertically and also the heater test will be the same scale and that will be full canister-scale. So, those are some of the modifications that are different from the SCP.

Each of my presentations on these experiments will be in essentially four areas, not all of equal detail. So, I'll establish the purpose and the rationale. The purpose and rationale was discussed by Tom Blejwas earlier. We start with the higher order documents, the issues, the resolution, the performance allocation, undsowieder. I'll describe these experiments in some detail, discuss what experience we have, and then I will speak a little bit to the analyses that Larry and some of his colleagues have performed.

The heater experiments will be the same for both the TSw1 and TSw2 and the purposes of these experiments are obvious. They are to obtain the thermal and thermomechanical rock mass data in these two horizons. They have also included in some of these the idea of borehole stability when you overdrive these experiments to fairly high temperatures. That's also one of the facets in the SCP. And then, it will be a direct canister-scale mock-up, the same geometry, the size, and so forth of an actual thermally and radioactively hot canister.

The data will be used to validate the thermal and thermomechanical codes and, as I mentioned, the concept in at least one of these cases is to overdrive the country rock to see if it will decrepitate, not so much decrepitation in the sense of salt decrepitation, but in the sense of a thermal spalling.

I shouldn't have to dwell too much on the idea of canister-scale heaters. Heater experiments have been done numerous times and various countries and in the United States. The idea is to place some sort of a heater and make the--you should do several things. Make the geometry conducive to modeling is one and then measure those parameters that can be modeled. So, I think that the test description is fairly straight forward. In addition, in your packet are some drawings that I don't really think I have to go into in too much detail. If you consider this is the horizontal layout, you can just flip it on its side. Basically, you have a central heater and then a variety of gauges around this heater.

But, let me talk about the experience because I think this leads to some of the problems inherent in these kinds of tests, particularly for the welded tuff. There's a lot of heater experiments not only at Sandia within the G-Tunnel, but also, for example, at WIPP and other places, other countries and other efforts around the world. So, there's lots of experience out there on these kinds of experiments.

But, Roger Zimmerman and several other colleagues at Sandia ran three experiments in G-Tunnel. Two of them were in nonwelded tuff, one was in welded tuff. Two of them were horizontal and one was vertical. So, that's our experience base. In addition to that, the analysts at Sandia spent a good deal of effort on calculating these responses from these small scale heater experiments in G-Tunnel.

So, this is a typical model (indicating). This was run by a fellow named John Holland who is a subcontractor on site at Sandia. It's a basic room configuration with a heater in it. We've been doing these types of calculations for about 15 years or so, both on the salt program and on this program. The layout of the model, this is the room here. This would be some sort of a backfill canister and then the adiabatic surfaces far enough away so they don't influence the isotherm. And, there are several more examples in your handouts which I don't believe we should take the time to dwell on.

But, typically, these data and those of many others look like this. That as a function of depth around a heater into the country rock and on the sleeve and on the borehole wall, one measures the temperatures and then one can go back and calculate these or you can predict these. And, to make a long story somewhat short, calculations of temperatures do pretty well. Temperatures do pretty well. That is not a major problem.

I would just like to maybe add something to this. On these particular heater experiments in G-Tunnel, one could have done a better job on getting more data in a geometry conducive to modeling for the country rock. In other words, these are real sparse data here (indicating) and so perhaps we could use a lot more in terms of data for code validation. Point-wise calculations aren't too good in terms of code validation.

What's in the SCP regarding the heated block is essentially a duplication of what was the experiment in G-Tunnel and most of the panel or several on the panel have seen that. At least, the artifacts remaining in G-Tunnel of the heated block experiment has been discussed already a couple of times today. So, I won't spend a lot of time on this particular test. Obviously, the purpose of the test is to project the rock mass thermomechanical behavior. It's very important for code validation to conduct--to cycle the material of a sufficient mass through the proper stress paths so that you can evaluate the constitutive model of the material. It's also important to make the right kinds of measurements so that you can compare one to one from the codes to the field data. In this case, we hope to obtain these kinds of data by establishing a one dimensional heat flux by running the stress states uniaxially and bi-axially and then become more sophisticated in stress and temperature cycles. And, again, finally, we want to run this in a geometry that is simple to model. Everything can be modeled, I've been told. Some of it can be modeled quite well.

I don't even think we really need to discuss a description of the test, but I will give you just a little bit of history. The G-Tunnel Heated Block experiment started 10 years ago, in fact, and it took three years at least from beginning to end and the SAND report here referenced and discusses a good many of their results. And then, subsequent to that, my colleagues, Larry and many others, Tony Chen, spent a good deal of effort making various calculations to those data. We used particularly some of the analyses results to establish the procedure by which we would conduct a similar experiment in the Exploratory Studies Facility.

There's a cartoon. I think this has been explained before, right? Two meters by two meters by two meters, two lines of heaters on either side. We're going to use the flat jacks that Tom discussed. We're going to saw the slots with the saw that Larry discussed. To do something like this, Tom's overview of this--Tom's photograph is better than mine, but this is actually what Zimmerman and others--SAIC, provided the field support for this experiment. And, you can see that it has numerous--a numerous number, a high number, it has a hell of a lot of instruments on it. They didn't all work real well. These are not straight forward and simple experiments to deploy in the field. And, in fact, the analyses pointed out some of the ways that we could make this better. As Tom mentioned, the guiding philosophy is not just to re-do things that have been done before, but to make them as simple as possible and then not make them any simpler. To make them as simple as possible so that you can deploy them easily, efficiently, and successfully and not leave anything out. That might be a good deal of wishful thinking.

What the data here do suggest is that we--well, they backed out from a lot of these measurements the rock mass modulus and the Poisson's Ratio. It's not really a Poisson's Ratio, but it's a measure of the lateral strain, the principal strain ratio really. And, the attempt is, of course, to better estimate the rock mass properties. They could do this by taking the real data and making the calculations. See how many of the parameters they have to tweak to get it to fit and how seriously they have to tweak them, and then when they're all done in the fits, how realistic are they? Is that too simple? That about covers it.

Anyway, in my opinion, the real key is that the experiments and the analyses comprise parts of a circle and they come back to the beginning. So, when we get through with the analyses, we really have a better idea of how to go about putting these experiments in the field. So, they improve our experimental techniques.

These are just some references of the various codes. There's been a lot of work, a lot of very good work, sophisticated work, and I think the bottom line on this is not that they can't model quite well. They probably are a long ways ahead of what we have done in the field. The point is that we need to get some experimental data against which to validate. There's a big gap out there. You saw the extensive calculations on that slot test. That was a pretty good series of data because it captured just the right amount of data and it had enough detail in those measurements, like the fractures and the geometry, et cetera, so that they could calculate against that. There's a big void out there of experiments against which to validate codes.

This is an example of after Larry worked real hard for a long time. He was able to match up the displacements. This just happens to be a displacement. It doesn't really matter on that heated block experiment. It shows the flat jac pressure here going up to something like 8 or 9MPa and some sort of a displacement. They were able--this is the code calculation. These represent the original and the real data here and you see a couple of features are important on this curve, one of which is that for the calculated data this displacement goes straight up, comes straight back down to zero. That's not the way a fracture deforms a course. The model of a fracture should have some sort of an offset, some sort of a permanent set, which I believe they are working to incorporate that element into the code, but that's obvious. And, the second thing has to do with the magnitude of this particular stress here. It wasn't high enough really to promote extensive deformation on the block experiments. So, a couple of things have to be added to each of the models and a couple of experimental details have to be added to the field experiment. And, that's why we're going to run what we call the unheated block at G-Tunnel and that would be specifically to run those stresses up higher through a series of stress paths.

This is a summary slide, I guess, of some of the modeling results. As I mentioned, the permanent deformation upon unloading was not modeled. They were able through some parametric study to actually match up some of the displacements and other things that we saw. And then, thirdly, the stresses were too low to really capture the fracture behavior. The G-Tunnel Heated Block experiment wasn't very well constrained and I don't mean that to be critical. I mean that as a learning experience. There are several constraints that turned out to be critical to the evaluation of the experimental data that just weren't taken care of at the time, such as insulation, you know, heat loss through the floor, a couple of other things. So, there is a lot to be learned in these field experiments.

I guess I'm just one slide ahead because this is just what I mentioned. One of the main problems with this was that there was a significant heat loss correction. They needed to have an insulation blanket or something. Of course, if you put a blanket over it, that obscures a good deal of what you're after. So, you know, you have to be aware of it and figure out how to work around that. Then, finally, of course, from those conclusions, from the analyses, we decided that there were other ways that we have to go about doing some additional models specifically for constitutive laws, development, and code validation.

The next test is again one that has been discussed by Tom and Larry previously. So, I'll just brush over it with a broad stroke and probably editorialize just a little bit. The thermal stress test was conceived and is written up in the SCP for the purpose of measuring stress buildup using a flatjack which is based on another series of analyses and was judged to be inappropriate. That the experiment just, as conceived, would not work. We could not evaluate that data. So, we decided that, as Tom said earlier, that it's still very important to assess or at least to demonstrate under thermally-driven geometry whether or not this will remain open under repository type conditions, a rock mass thermally-driven strength experiment, if you will, thermal stress experiment.

There are several reasons for prototyping this, but one of them that we haven't mentioned very much before is that when we go underground and try and make thermomechanical measurements, we're going to have a hard time. And, the reason is because you might have noticed on Larry's previous presentation and this one that those deformations that we measured are very small. He was talking about modeling from a drift from 50 feet away a deformation of a millimeter. The thermal compensation, thermal calibration, of that instrument swamps out that particular type of a measurement.

In addition, anything else that you put in that thermal transient environment is going to have to be corrosive resistant, it's going to have to be calibrated against that thermal environment, and the concept of these experiments have to include the idea that you have to replace the instruments. I just finished reviewing a very large document on some experiments at WIPP and I talked to Daryl Munson who happens to be a principal investigator down at Sandia and his rule of thumb for in situ tests is that you have to replace 25% of your instruments every year. That every year, at least 25% of your instruments will go down for one reason or another. That doesn't mean that they all fail, but there are lots of things that impact these instruments. So, his rule of thumb was like 25% a year. I just throw that out because I happened to remember it at this time. But, it impacts an experiment like this severely because this experiment, as it is conceived or as it was conceived--I guess, it's still current--would have precluded re-entering that room and, of course, this was thermally overdriven. So, it was a little bit more severe than what you'll see in situ. But, basically, we were going to heat up the roof of X cubic meters of country rock to some sort of a stress state that we, based on calculations, assumed it would be something of the order of the failure strength of the rock mass really to see what would happen. In addition, we'll learn a good deal about instrumentation and we'll learn about the rock mass strength and probably a lot of other things.

DR. CORDING: What temperatures did you figure that would be to get to that, Frank?

DR. HANSEN: Okay. They did a lot of calculations on that, Ed, just an enormous number. Lots of different geometries, different heater loads, different times, different blankets for insulation. Their analyses can go on forever. In this particular case--and I just grabbed one, there are thousands of these. But, this is the excavated drift and this happens to be for a time of 90 days of heating. I think the loading was 1200 kw. It doesn't matter, it was just one of the typical representative experiments with the line of heaters. This is a plane strain analyses. And, to answer the question, these internal isotherms right here (indicating) are 400 degrees Centigrade. The 100 degree isotherm isn't on this particular graph, but it would lie something out here (indicating) which is nine meters higher than here (indicating). So, it's about 20 feet out into the country rock. And, the 100 degree isotherm tends to be kind of important in terms of instrumentation. Up until then, your--and things kind of hold together. I went out of order there so I could address your question.

But, the data analyses include two dimensional plane strains. Always, when you run an in situ experiment, if you want to have code validation subsequent to that, you want to make your geometry so that it can be modeled without too many overlying assumptions and simplifications. So, we wanted to make this two dimensional. And, so the calculation can tell you how long that has to be and what sort of guard heaters you need and things like that for your test layout.

So, those are the types of inputs that we need to decide on. You can control the boundary conditions. You can input the thermal loads you want and you can make the calculations for the durations. The output, of course, displacements are the best. They're the easiest to measure and you can directly relate those to the calculations. It would be very nice if you could get a stress measurement, but that's another problem. But, they can calculate the stress output with the various--whatever kind of constitutive model they use and they can give you the temperature profiles.

Hopefully, we will compare these with some experimental results. That's the bottom line.

That's all I have on the thermal stress experiment. Again, if we were given the wherewithal to conduct that experiment today, we'd probably have to go back and re-think quite a bit of it.

The heated room experiment has been discussed a couple of times already. Primarily, the geometry of this has been discussed by Larry. The geometry is the same as mine by experiment with the central drift being a repository type configuration, excavated like the repository opening would be. One of the important considerations on the heated room experiment is that it's not really a licensing type of an experiment. You don't get this information for the purpose o licensing, but it's a long term experiment. By comparison, the WIPP experiments they used heaters ran for four or five years. They were actually shut down because of roof falls and things like that. But, typically, these types of heated room experiments were run--40 months here, I would say, would be the short end and probably they would run for a considerably longer period of time. So, it's for long-term data under expected repository conditions. As it says there, some of the information may be available by the time of licensing, but most of it will be for performance confirmation.

You've seen and heard about the description of the test. So, maybe I will just go straight to the cartoon. After Larry gets through with his mine by experiment, the people with the heaters and the MPBXs will come in and set up some configuration. Now, I say this is a cartoon because, really, when you get down to the point of deploying these experiments underground, it takes at least a year and a half to figure out all of the things that you need. You have long-term delivery items and lots of other things. So, we wouldn't probably deploy this experiment exactly like the cartoon shows here, but the idea will remain the same. The idea being that this repository, the repository configuration will be subjected to those types of temperatures that ultimately a repository emplacement room would see and then we can make some thermal structural evaluations. And, the reason for these heaters going in laterally like that is so that you can get to that temperature state faster than you would if you put in the conventional role of heaters in the floor.

So, we don't have within the Yucca Mountain Project any experience on these within this project. There are lots of other people that have done similar type of experiments at WIPP and at Climax, but our experience, so far, to date, has to do a lot with the instrumentation evaluation. As I mentioned earlier, all of these instruments have to be calibrated for a thermal environment which is corrosive and also for the thermal expansion. We have run heater experiments before. There's a good deal of heater technology out there. So, that's not a problem. And, we've had large scale data acquisition systems work successfully before not just within this project, but also within Sandia.

Here, just a couple of isotherms show you the temperature contour. This one happens to be at 40 months with a particular power to the heaters. These are just representative and typical of what sort of calculations one could run out. These are important because they help you place your instruments in the right place. They allow you to evaluate what sort of displacements and assign, you know, as you might expect. So, they help you not only to estimate what sort of stresses you will drive this thing to, but they will also help you with placement of instruments and geometry and length of diameter ratio and so on and so on. So, there's lots of uses for these types of analyses. Those are temperature contours. We would just note that, let's see, the D Contour out here in the middle ground is 150 degrees C. That's after 40 months of heating under this particular configuration.

A little more confusing, you have this in your handout, you can also back out some sort of horizontal, even back out any stress you want. This one happens to be horizontal. The idea, as someone mentioned--Tom, I believe, early on--is that the principal stress contours change and the directions change as a function of heating. So, that's what will happen in this case where the horizontal stress, especially in the roof, will increase.

This is my last slide and it happens to be one that says necessity for thermal modeling. Really, as I say, it's a big circle of events and they're all tied together. The analysis guides the experiments. First of all, you conceive of the experiments and they analyze it and they come back with their calculations. You reconfigure your experiment and you go around the loop and open the lead to deploy this experiment underground and compare. It's that comparison by prediction that validates the code.

I think I'll stop there.

DR. DEERE: Are there questions? Ed?

DR. CORDING: In those borehole tests, heater tests in the boreholes, did you see spalling in those in G-Tunnel?

DR. HANSEN: No. No.

DR. CORDING: And, you're heating those up above 500 C or--

DR. HANSEN: No, they didn't go that high. Those were not very hot. The idea of thermal spalling hasn't been established yet. My own--I have a bias. I don't think it will happen, but I think it's one of those things that just has to be demonstrated.

DR. CORDING: Happen in the emplacement holes?

DR. HANSEN: Yes.

DR. CORDING: All right. Because those temperatures will be higher than the surface of the excavation itself.

DR. HANSEN: Yeah.

DR. CORDING: One other. Do you plan to use in some of these room experiments, either the thermal stress overdrive experiment or the heater room, do you plan to put in several variations on support systems to look at their capabilities of being able to work under those stresses? What the best designs might be, for example?

DR. HANSEN: Larry, do you want to take that one, have a comment on that?

DR. COSTIN: Yes. As part of the excavation investigations, you know, when we excavate the room in which the large scale heater tests, again and as a part of the design verification program, a variety of ground support systems will be monitored--will be installed and monitored at various locations, especially in that particular drift. So that when it is heated up, we can look at the performance of different types of ground support under those kinds of stresses.

DR. HANSEN: One of the big controversies we had--not big, but consideration that we had was for the thermal stress experiment it was my opinion and perhaps some others that I didn't want any roof support in it, at all. But, it remains to be seen whether we will be able to do that or not.

DR. COSTIN: Yeah, for the thermal stress test, in which case you're looking at a large scale kind of failure envelope test. In the heated room test, you're trying to demonstrate if you constructed an emplacement room of such a configuration, would you be able to maintain stability and what kind of ground support would that require. And, at this time, we don't really know the answer to that, but we're going to try various kinds of ground support that is aimed at supporting those kind of loads.

DR. DEERE: In your room experiment, you have just one instrumentation section or will you have a whole variety?

DR. HANSEN: The heated room experiment?

DR. DEERE: Yeah, in the heated room--

DR. HANSEN: The cartoon probably only showed one series, but there will be a sequence periodically of the MPBXs and the--measurements. But, one of the important components to field an experiment like this, remembering that

25% of your measurements go out every year, is redundancy. So, you have geometry, mirror image redundancy, and then you have instrumentation redundancy, so that you can get the same kind of data a couple of different times. So, that cartoon isn't complete and certainly the thought process isn't complete on instrumenting that heated room experiment, but they'll be--typically, I'll give you an example of the A-Room experiments that WIPP had, some 600 or 800 types of measurement devices, particularly displacements and thermal couples.

DR. DEERE: Well, the reason I ask this question and I'm glad to hear that you will have various sections that will be well-instrumented. In trying to run shear tests, for instance, on a shear zone or on an important throughgoing fracture, I have found it to be very important to try to pick an area where you think you have the very most unfavorable condition. You have more infilling, you have more weathering, you have less roughness, or whatever, and then to pick a second one. But, you're convinced this is about the best I've ever seen this. And, number three, this is sort of an average. And then, we set up our tests and run three of them and we have a pretty good feeding then.

DR. HANSEN: Um-hum.

DR. DEERE: And, so if you have several instrumented sections, you're going to have different geometry of your joints, different intersection patterns. So, your code would be able to be validated under about five or six different conditions. And, I think that's very good. The bigger you can make this, the better.

DR. COSTIN: And, I think you see that in the demonstration driven experiment in G-Tunnel that each measurement station was quite different than every other measurement station. But, if you do a typical cross-section and then you kind of average what went along, you can predict things on average. Or if you focus in on one particular cross-section, then you can look at the details of that particular cross-section. It does say up there that's a typical arrangement. That will be repeated through the whole length of the drift and in various ways.

DR. DEERE: What's the length of the drift you're talking about here?

DR. COSTIN: It's on the order of about 150 feet.

DR. HANSEN: Okay. We're approximately on schedule. So, I guess we'll let Tom Blejwas give the summary.

DR. DEERE: I'm not going to let you off that easy.

DR. HANSEN: Oh, I'm sorry.

DR. DEERE: Since you have 150 foot length, this is very--you did say 150 feet?

DR. HANSEN: Yeah.

DR. DEERE: Yes. You could almost put an intersection in there, couldn't you? Because this is the critical thing of our repository, are the pillars and the corners. I think we have experience to show this.

DR. BLEJWAS: Well, we actually--I think in some of the drawings we have for this experiment, we show a drift cutting across the back of the three drifts and connecting them and at various times we've intended to do that. I don't know what we finally have in the study plan, but I think that is a part of our planning to do that.

DR. HANSEN: Maybe I should just interject this. We haven't made, to be real honest, very much progress on these experiments from the concept because we don't have very many people minding the store. We don't--you know, it's a luxury to be able to sit down and think about how to go about doing this business and it's not an easy proposition. It takes some real hard science to figure this out. And, if you don't have somebody working on it, it just doesn't get done. And, you know, we were just barely able to take care of the test plans and the study plans and the procedures and then re-do them and the quality assurance grading packages. We were barely able to keep our head above the paperwork. So, we haven't, to be honest, spent very much time on the development of these experiments.

DR. DEERE: Yes, I realize that.

DR. CORDING: Could I ask another question here just a little further on that comment you made. You didn't expect spalling in those emplacement holes. Was that for, say--what sort of stresses or temperatures are you looking at there?

DR. BLEJWAS: We're typically looking at temperatures on the order of 200 degrees C in the boreholes. The exact temperatures, I don't have. But, that's the general range we're considering for the borehole walls. And, to perhaps differ a little bit with Frank, my recollection of the heater tests in G-Tunnel was that that's the range of temperatures we got in the borehole tests in G-Tunnel. So, we got up to temperatures in that general range, maybe not quite as high as 200 degrees C. The other thing we have is a lot of experimental laboratory tests on core where we heated the core. And, so that's the reason we put all that together and we can come up with a way to get spalling to any significant degree. Is the rock strong enough--

DR. CORDING: Your stresses are just not high enough

DR. BLEJWAS: That's right. The stresses are just not high enough.

DR. HANSEN: Here's that viewgraph that we looked at on those isotherms measured at the skin of the heater, at the skin of the hole, and then .25m out into the country rock and they range like this. 400 degrees C would be typical on the interior; then, at the borehole wall, something of the order of 300 degrees C. And then, I believe this is .25m--yeah, 25 centimeters out into the rock at the mid-height. This isotherm is something of the order of 150 degrees C. So, you're right, Tom, these measured temperatures got up on the borehole wall 300 or 400 degrees C range. But, it still remains an issue that has to be addressed.

DR. DEERE: In the meeting last week that was held here in Denver also on the engineered barrier system that DOE sponsored, the workshop, there were quite a number of comments by various people about the advantages of having a liner of some type. So, these may not be open holes. Whether you have a liner and a buffer system, you simply have to have the ability to be able to retrieve them, but a system could be worked out. I'm not saying there's necessarily going to be a change in it, but certainly people are looking for long-term stability of those holes for earthquake loading and a whole series of reasons.

DR. BARNARD: Frank, I've got a question.

DR. HANSEN: Certainly.

DR. BARNARD: You've described a series of experiments that range in scale from small scale heater tests to large scale heated room experiments. Would these tests be done sequentially so that you can learn from one test before you start the other test?

DR. HANSEN: I'm trying to think how we have that configured. Not necessarily. Many times these are confirmation type tests. For example, the heated room experiment would be started with some sort of expeditious speed as soon as we got the mining evaluation, but, no, not necessarily. If that's sufficient, I'll let that go as the answer, but it's not necessarily that we would scale up like that. For example, we would--I think we would do them as soon as we could as a--maybe we have a better definitive schedule there.

DR. BLEJWAS: Well, I think it is clear, though, that we would get the results from the small scale heater tests first. Could you be using those results, but concurrently we would be working on, say, the thermal stress test and those results would come at a later time and we'd use them? The room scale tests will take years. So, even though we would try to start it very early, we will have to do our planning without the benefit of those other experiments, but the results will come again later. So, it is a learning experience in terms of using them for validation of our codes. But, we will not have the luxury of finishing the smaller experiments and getting all the information to help us plan the next experiment. That luxury, we won't have.

DR. HANSEN: Time and size are co-dependent.

DR. DEERE: In your calculations, I think you showed the stress distribution around the openings in your second slide this morning. In these studies, does it indicate, Tom, that you would be getting stresses with heating that approach the unconfined compressive strength of the rock?

DR. BLEJWAS: If you look at the distribution of compressive strengths, yes. Some of the weaker rock, we will be approaching that.

DR. HANSEN: In addition, if I might add, the rock mass strength is substantially different and lower than the individual strengths can be and right now we're thinking that might be around 4500 psi, you know, 30 MPa or so, because of our experience on the slot tests where failure actually was achieved. It wasn't just the fact that it failed either. I mean, we were cracking all over the place.

DR. CORDING: That's fairly close to the stresses that would be imposed by heating to 100 degrees C?

DR. HANSEN: Yes. I don't know if heating to 100 degrees what that isotherm would be, but--

DR. CORDING: I believe it's close to that.

DR. HANSEN: Those stresses calculated by St. John using--you know, there are all manner of assumptions that go into those calculations, but those are within that range. Certainly, a good probability that you'd be approaching that rock mass failure strength.

DR. BLEJWAS: I have to admit, though, that I probably was careless in using a fairly old set of analyses except that the viewgraph was such a good one--we've had it around for a long time--and it demonstrates the point. If you look at some of our more recent analyses like ones that Eric Ryder is doing, for example, on the temperature distributions, you know, taking into account the heat losses due to the two phase flow, the ventilation system, we were probably looking at higher temperatures in those early analyses than are realistic for the repository even if we went with the conceptual design that presently exists. So, the problem probably looks worse than it really is. We have never felt that we were going to have a serious problem with rock failing in the unconfined compressive stress sense from just the thermal loading. But, we felt it needed to be investigated and how close we were to that.

DR. CORDING: Yesterday, we saw some of this umbrella effect of the 100 degree contour between the excavations. Is it true that you are not--that the spacing is such that you will have in between these emplacement drifts temperatures which will always be less than 100 C? Is that right as you recall?

DR. BLEJWAS: That's not necessarily true.

DR. CORDING: I'm just wondering. I thought I heard situations where you would have over almost the entire repository level--the contours would be such that you would have 100 degree C or above at that level.

DR. WILDER: I wonder if I could comment? The information you're talking about, Ed, was for older spent fuel. What I was showing was for 20 year old spent fuel. Then, we would have a situation where the 100 degree isotherms would not coalesce. But, for the approximately 10 year old fuel, then those isotherms would coalesce and you would not have any part of the pillar that would be below 100 degrees C.

DR. CORDING: Thank you.

DR. HANSEN: Shall we bring Tom up?

DR. DEERE: Yes.

DR. HANSEN: Okay. Tom Blejwas.

DR. BLEJWAS: Well, the title for the last presentation of the day is a summary of the rock mechanics program. I'm not really going to give you a summary. I don't think you need it, but I'd like to use the opportunity to try to make some finishing points, if that's all right.

I would like to bring you back to one of my earlier viewgraphs where I said that the objective of this program was to collect data of sufficient breadth to support a wide range of anticipated and unanticipated analytical and empirical activities. I hope that you gained an appreciation of that from the presentations from Frank and Larry. I think you can see that we do have a very broad program, but we think it is necessary for it to be that broad.

It may not have been apparent, but what they were talking about was four different studies, the four bottom ones on here, and this is a better version of the earlier viewgraph I had. There was a line left out on that viewgraph. There are four studies dealing with the laboratory properties and so it's a total of eight studies that we've been talking about that cover the whole range of the rock mechanics program.

The status of the program, as you probably gathered from comments that Larry, Frank, and I have made, is that the field testing at least is on hold. That's become pretty apparent. But, the laboratory testing is continuing and we have had that going on through the last several years. I didn't emphasize that in my talk, but it has been going on. Also, we have a lot of design analyses that are ongoing. In addition to the kind of analyses that Larry showed comparing experiments, we are doing analyses using those same tools and with more confidence because we have compared those tools with the experiments in G-Tunnel and that is ongoing. The field testing activities, after G-Tunnel closed we did conduct some continuing efforts in developing equipment. For example, Larry mentioned the latest round of work we've done on chain saws. But, as of this point in time, even that work is essentially discontinued or put on hold.

For my final viewgraph I'd like to talk from, I'll bring you back to what we thought the near-term activities were in 1989. Namely, that we wanted a prototype thermal stress experiment and really do some scoping on some rock mass strength tests and continue some other activities and do what I called an unheated block, but Frank referred to it as an ambient block test.

We think that somewhere along the line, as the budget increases again, that we need to get back to doing these kinds of things. Even though we've portrayed that staff have left the rock mechanics program and they have, we are confident that when funding is put back into this effort that we can staff back up to continue this kind of work and we hope to gain over those years from the analytical work that people have been doing that Larry showed you, but also from ideas like the ones that you've suggested today for alternatives--because while we're waiting or more funding, the rest of the world is gaining experience--we hope to be able to benefit from that in the future. So, we do see this activity getting going again before we do the final experiments in the exploratory study facility and that's the kind of thing that we've been discussing with the Department of Energy.

With that, I'd be glad to answer any larger questions.

DR. DEERE: Thank you. Any questions? Arch, do you have any comments?

MR. GIRDLEY: Tom, I don't know who to address this to among you, but since you're trying to coordinate your inputs into design for the test, for fielding the tests, I've seen flipped up here now two or three times the prototype thermal stress test and certainly I recollect that we closed G-Tunnel down on you just before you began to field that test, the prototype test. Do you have any plans, since I haven't seen them come in, to the test planning process to prototype that perhaps in the north access someplace or is that something you need to do prior to the schedule for that excavation?

DR. BLEJWAS: Well, personally, I consider prototype testing as still being open issued, something that hasn't been determined--hasn't been fully defined. As you may be aware, we were hoping to do some prototyping at a site in

Creede, Colorado, but we were unable to obtain the funding for that activity. We have hopes that if we cannot do the prototyping at an alternative site like a site in Creede, Colorado or a site like G-Tunnel that indeed we will have to change some of the planning for the exploratory studies facility. We just haven't gotten to that point where we're convinced that there isn't going to be enough of a ground swell of support to prototype testing that the DOE will indeed open some type of another facility and I know there are continuing talks about a facility for not just prototype tests, but also it might be worthwhile to have a facility where you could do destructive testing without the stigma of somehow damaging the repository itself. So that, for example, if we want to do a large scale test where we ran temperatures up to the point where we have a lot of rock fall, if there were an alternative facility where we could do that without doing it actually in the repository horizon, I personally think that that would be beneficial. And, that same facility could be used for prototype work. Maybe Dave would have some ideas of where exactly we're going on prototype testing, but I don't know myself.

DR. DOBSON: No, I guess I would just add that there are several proposals in front of us with regard to the development of facilities where you could do those sorts of tests. And, there have been proposals to develop something, for example, at Fran Ridge. And, I guess, at this point in time, much as Tom said, you know, the direction that we're going is largely contingent on schedules and funding and things like that. Right now, we're trying to put most of our dollars into getting ready to design and construct the openings for the north access. Different schedules and different funding scenarios might lead to a different decision.

DR. DEERE: Are there questions or comments from anyone in the audience?

MR. DATTA: In your program, I see almost 100% reliance on MPBXs for measurement which is fine. A whole body of experience on MPBX is all right. But, did you consider or maybe try to develop any kind of innovative method of measurement which is more realistic in terms of rock mass measurement? Many MPBXs are still point measurements and interfered with seismic or something like that both for rock mass properties and rock mass response.

DR. BLEJWAS: Well, I mentioned early-on that for the rock mass response measurements we were trying to develop an interferometer approach to measurements that we thought could have been used at many, many--a lot of locations to the point where you would get almost, say, a continuous description of the displacements. We just haven't had the resources to put into developing something like that and there doesn't seem to be anything that you could pull off the shelf that you could readily use. It would have to be a developmental effort in that area. At least, that's the way I remember it. Things may have changed since we were doing that scoping a few years ago. In terms of the actual measurements for properties, I don't believe we're looking at any real alternatives to the MPBXs.

DR. HANSEN: One of the many intermediate laboratory scale tests that was shut down involved an engineered block experiment and waterways experiment station. On that experiment, we were going to, in fact, put a grid of dots with a matrix measurement of a large format camera. Okay, now that's one way to go about it and also you can do that in the field if you can prototype it properly. That also is very valuable for the kinematics of the model. Okay? So, that's one other idea that we had, but were not able to consummate it.

MR. DATTA: What I had in mind, you know, you are interested in the in situ modulus of your rock nest, right?

DR. HANSEN: Yes.

MR. DATTA: For modeling purposes. Now, we are making all these point measurements. So, what about putting two holes in the drift and try to make that--of the dynamic models of the old rock mass and maybe supplement that with the--

DR. HANSEN: Backing it out from velocity?

MR. DATTA: Um-hum?

DR. HANSEN: Yeah, yeah. There's a large error bar on that, I might add.

DR. DEERE: Just to comment to answer you, I don't think these are really point measurements you get from the borehole extensometer because really you're taking what happens between two points. So, you're really dealing with a mass. And, you can determine from the extensometers that you're getting different behavior, different displacements at different depths, either associated with the excavation or associated with your plate load tests. So, though they're anchored at a point, you're really getting sort of a mass behavior.

MR. DATTA: I was thinking that--

MS. EINERSEN: Excuse me, please use the microphone.

MR. DATTA: I was thinking that in seismic methods you could capture the old rock mass better.

DR. DEERE: Well, this is right. I would agree that there is probably more difficulty in correlating seismic velocity within in situ modulus. There are a variety of ways of doing this, as you know, but none of them are really very exact. They have an awful lot of scatter. It could be a good indexing test, though. It could be an indexing test.

DR. HANSEN: Incidentally, we had some velocity measurements made at G-Tunnel by the University of Texas, I believe. And, they made some calculations that were not too bad, inasmuch as they agreed somewhat with the module we backed out from various measurements.

DR. DEERE: Yes. And, I don't know if you are familiar with the method that has been developed, I think, primarily in France, although it's being used in several countries, of the attenuation of the shear wave velocity which has been helpful in correlating with rock quality which, in turn, correlates with the in situ mass. This would certainly be something to look at, I think. It's a very easy thing to determine.

DR. HANSEN: As an index.

DR. DEERE: Are there other--yes?

DR. PARK: This question is to Tom Blejwas. I have a couple of related questions. This is primarily a rock mechanics presentation. However, I'm looking at it from post-closure performance assessment point of view. Now, one in the performance department that we have which gives us a lot of problem is, as you know, the groundwater travel time which starts from the boundary of the disturbed zone. Now, in order to determine the extent of the disturbed zone, we have to look into mechanical disturbance due to preparation, as well as the thermomechanical effect and the geochemical effect. But, largely, the extent is determined by the mechanical disturbance and thermomechanical. Is this the study that would provide that information in determining the boundary of the disturbed zone?

DR. BLEJWAS: Well, I don't think it will necessarily directly determine the boundary of the disturbed zone because that's a complex interpretation and it's a regulatory problem. But, from our results in G-Tunnel, we would conclude that any real disturbed zone from a mechanical and thermal performance or from a mechanical performance is probably very small. Now, when you say the thermomechanical, we'll be able to predict, yes, how far out the stress has changed significantly, how far out we get significant slip along joints, perhaps, or at least make some estimates of those kinds of things, but what is the impact of those things on the likely transport of radionuclides wouldn't come out of this study. And, you'd have to interpret that with the work that we're doing.

DR. PARK: Okay. That's my next question. The reason why they set up that disturbed zone is primarily that they were concerned about the effect of disturbance on hydrologic parameters, primarily permeability. Now, for the near-field environment, we really need to address that hydrologic impact; for example, the thermomechanical effect on the fracture, the closure, or expansion--now, I don't know which way they'll go--which, in turn, will determine the permeability. And, we need not only to determine the extent of the disturbed zone, but also to model the near-field.

DR. BLEJWAS: Right.

DR. PARK: Okay. And, you mentioned earlier to the question from Charlie Voss that there were some water permeability measurement. Is there any program to study not only during the pre-closure, until the repository closure, but extending into the 200 or 300 years beyond the closure to study the effect of thermomechanical on the permeabilities?

DR. BLEJWAS: Yes. Yes. I think that the tests that are planned by Lawrence Livermore Labs dealing with the near-field environment are more directly aimed at the post-closure period and they are looking at the entire near-field environment during those tests of which some small component is the rock mechanics. And, we have interacted with Dale Wilder who is in the audience and other people at Livermore to insure that the experiments that we're conducting and that they're conducting would be compatible and would compliment each other so that our work is indeed aimed more at pre-closure and their work is aimed more at post closure concerns.

DR. PARK: Thank you.

DR. BLEJWAS: Sure.

DR. DEERE: Don Deere here. I just wonder if the increase in stress from the thermal loading is really going to go back into the--the effect of increased stress is going to go very far away from an opening because, you know, you start getting into triaxial confinement. Just as soon as you go away from the opening, you immediately have the triaxial confinement and the strength goes up, what, five to six times the confinement. So, if you have 100 psi confinement, why, you have 500 psi stronger material there. So, it seems to me like if you don't spall material, you don't cause a breaking across some asperities and a block coming down. You go back five feet, you may be out of the critical area.

DR. BLEJWAS: Yeah, that's very consistent with what we would conclude from our analyses. There may be heat effects further into the rock.

DR. DEERE: Exactly.

DR. BLEJWAS: But, there will not necessarily be some type of coupled thermomechanical effect. The changing of properties of joints, for example, is not likely to be so significant that you're going to change the flow field in any way that would be measurable. That's my own personal perspective from what we've seen, so far.

Dale, do you have a--

DR. WILDER: Yeah. I'd like to add just an observation that we saw at Climax and you may have seen this in the final report of Climax and which is we tried to look at the predicted from model calculations, deformations, versus what we measured. And, we lowered the modulus as was previously mentioned expecting that the moduli would be much lower because of the rock mass than what we measured in the laboratory. But, once we heat it up and then put the stresses on, we had to go back to an intact rock kind of a modulus. And, so I think it follows up with what you said.

DR. DEERE: Thank you. That's very interesting.

I would like to close. Any other questions?

(No response.)

DR. DEERE: Then, I'd like to close and, before I thank everybody, to state that again we have the experience of the large cavern being excavated out 300 feet below the sea and we're experiencing some problems due to the increased stress that is being brought about because this cavern which is about the length of two football fields and about 50 feet high and 90 feet wide is being constructed by small little perimeter drifts and backfilling with concrete and then another. We're doing four at a time now. Well, to approach this area, there are two inclined ramps at 10% and these ramps were supported with a few rock bolts--well, sort of a pattern of rock bolts and locally some shotcrete. Now, the whole mass is getting the idea that these are not just individual little tunnels going in. They're starting now to see the picture that the big cavern is starting to take shape. And, immediately, we're seeing effects of increased stress which I think are very similar to what we are going to have here with the thermal loading. What happens? We are getting spalling of the shotcrete.

Therefore, we have a continual maintenance program of adding additional rock bolts, additional mesh, and a second or a third layer of shotcrete. We have cracking of pillars and particularly where you have a pillar cut by an intersection where you get a stress concentration on top of a stress concentration. And, that's bringing the local factor of safety very close to one and what happens is the narrow pillars or corners are cracking. You can see it. What do we do? We add additional bolts and more shotcrete. We're measuring the whole thing.

Okay. Now, we're getting movements every week. I get about 50 pages of them by fax every Friday of all of the convergence measurements and we have areas that are moving one to two millimeters a week, we have other areas that are moving up to three to five millimeters a week. And, so we're looking very much at these trends. Now, hopefully, this three dimensional effect is pretty well going to be taken care of when we get our last two drifts in and our arch is completed and we can take out the remainder of the core.

But, these are things that, Ed, I think we saw at the Nevada Test Site 25 years ago, an excavation of the very large openings, the Red Hot and Deep Well and some of the others, where there was high stresses and you're starting to get at that depth and the stress concentration factors of safety of one. And, yet, those were built safely. How? By a tough yielding support and that was mesh, shotcrete, and bolts. And, the wall movement was about three inches. Isn't that about right, Ed, was the maximum?

DR. CORDING: About two inches, yeah.

DR. DEERE: About two inches. So, I think we have to look at this whole area with the thermal loading. It's going to need a tough yielding support. And, that, to me, means reinforced shotcrete of some type held by bolts.

So, any comments, Ed or Tom?

DR. BLEJWAS: I just might add that we have been thinking that perhaps we wouldn't use shotcrete. I agree with everything else you said. But, a wire mesh and rock bolts may require less maintenance in that as you get some spalling behind the wire mesh, it may not be a problem. You may have to go back in and put more rock bolts in, but you don't have the constant spalling of the shotcrete. The thermal loads would suggest that the shotcrete would be a problem.

DR. CORDING: I think that's an interesting area for part of the investigations here is to study--of course, you're not interested in just studying the rock, but the rock structure interaction and rock support interaction. And, there's a lot of details as to when you put shotcrete on or if you put it on and how that interacts under these different conditions. I think that's a good point. As you drive the TBM tunnels, you won't be putting shotcrete up near the front of the TBM if the people that use TBMs have anything to say about it. You certainly don't to have it up in the front unless it's absolutely necessary. So, that would be the normal type of support for the TBMs. But, I think that some of the details on these support systems remain to be resolved from some of the experience. I certainly think the tough yielding support system that you're talking about is the type of thing that one needs to do. Exactly how you put that together is something that needs a little more observation.

DR. DEERE: Right. I agree with what you say, Ed, and also what you said, Tom. However, we ought to build in for thinking of shotcrete some resistance to tension which shotcrete doesn't have very much of and that's--I see you had comments today--or not comments, but I saw it someplace--of the use of the reinforced shotcrete with fibers. And, it seems to me like this would give you a lot more resilience against this cracking that would happen at pillar corners and such things.

DR. CORDING: In the high porosity soft nonwelded tuffs, I think you will probably have to put shotcrete up at some point because that tends to dry out and spall. At least, that's been my experience with it in the Rainier Mesa. So, that there may be some different approaches as you come down through the different layers and even down into the underlying Calico Hills.

DR. DEERE: The good thing about unreinforced shotcrete is that it's a wonderful indicator of areas of (laughter). I mean, it really is. It's a fail-safe sort of thing. You get evidence of movement taking place and you get a chance to come back in and to reinforce it usually with rock bolts and additional mesh and shotcrete. But, it is a real, real good indicator of potential trouble.

DR. CORDING: As long as you're continuing to talk here this morning, just one brief comment. I think that you'll have a lot of targets of opportunity in regard to mine by situations where you can get an estimate of rock stiffness or rock mass modulus by going in and putting a couple of extensometers across before you come through. It would be part of the normal construction and you're not really trying to delay advance of a machine or something like that. But, I think there will be a lot of those opportunities and to have some instruments where it may not be a full test section, but at least being able to put in a few extensometers of some sort and to pick up that will give you really your large scale rock mass behavior, relatively large scale rock mass behavior.

DR. BLEJWAS: I think that's always been part of the idea whether mining evaluations, what we call experiment--it's really not an experiment, it's a monitoring program--to do pretty much what you just said and, as the plans for the exploratory study facility have changed, we haven't kept up to date with those changes, but we'll have to. And, for example, when you go with two ramps from the surface where you have a Y going off of that so that you have the ramp at the Topopah Spring level and then you're going to go down to the Calico Hills, that Y is an excellent opportunity for instrumentation to look at what happens at intersections and we probably will try to take advantage of that.

DR. DEERE: Right. It sounds very good.

Well, before adjourning, I'd like to say how much we appreciate the--oh, excuse me.

DR. WILDER: Sorry. I would like to follow up just a little bit on the comment about shotcrete. I think that it's very good to pursue some sort of a support system, but I'm sure I heard last week in the workshops--well, I hope you did. I had to leave early and I asked Wayne to read my comment. But, from the standpoint of the waste package container materials, something of a cementaceous grout or shotcrete would be very advantageous because it would control the pH, make a higher pH. We're looking at conflictive objectives, however, a little bit in that when we talk about repository openings we're talking about a drain retrieval period of perhaps 100 years, 80 or 100 years. Yet, when we're talking about the performance of the container material, we're talking perhaps 1,000 years or 10,000 years depending on how much reliance you're going to put on that.

From the standpoint of the leaching of the waste form, which currently we're relying on for about a 9,000 year period of time, raising the pH of the water chemistry is adverse. It increases the dissolution of the glass. And, so, I guess I would encourage some creative thinking on how to come up with a support system or at least we need to take that into account on the total system performance.

DR. DEERE: Okay, thank you.

So, I wish to thank DOE for responding so well and organizing this meeting, not only today's session with professionals from Sandia, but the sessions of the first two days. We think it has been most valuable for our board to get this in-depth briefing and I'm sure that others in the audience also have benefitted from it. Hopefully, DOE has benefitted from organizing their thoughts and organizing their information and some of the feedback that has come from the audience and from the board members and staff.

So, Dave, do you have a final comment?

DR. DOBSON: I guess I would just reaffirm what you just said, Don, and the DOE would like to thank the board for the opportunity to do the presentations and we hope that they have provided the information that you were looking for. And, I guess just address your last comment, I would say that as usual in our interactions with the board, we have learned several things that we will take home and hopefully use as we try to improve the site characterization program. We'll be talking to you about them again, I'm sure, and we look forward to it. So, thanks again.

DR. DEERE: Thank you. Meeting adjourned.

(Whereupon, the meeting was adjourned.)

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In the Matter of:

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PANEL ON HYDROGEOLOGY & GEOCHEMISTRY
and
PANEL ON STRUCTURAL GEOLOGY & GEOENGINEERING

Location: DENVER, COLORADO Date: JUNE 27, 1991
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