UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

Meeting of the Panel on Structural Geology & Geoengineering

Repository Sealing Program

Wyndham Garden Hotel
18118 Pacific Highway South
Seattle, Washington

November 12, 1991

BOARD MEMBERS PRESENT

Dr. Don U. Deere, Chairman
Dr. Clarence R. Allen, Chairman, Morning Session
Dr. John E. Cantlon, Chairman, Afternoon Session
Dr. Dennis L. Price

NWTRB STAFF

Dr. William D. Barnard, Executive Director
Mr. Russell K. McFarland, Senior Professional Staff

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Strategies to Seal Shafts, Ramps, and Underground Openings

<u>PROCEEDINGS</u>

8:30 a.m.

DR. DEERE: Good morning, ladies and gentlemen. I am Don Deere, Chairman of the Nuclear Waste Technical Review Board and a member of the Panel on Structural Geology and Geoengineering. We welcome you to our panel meeting dealing with Repository Sealing Program.

This is a topic that has always been in the back of our minds for the last two years, two and a half years, and about six or eight months ago we thought we would like to be updated. We know it's not a real active program at the moment, but it's one, certainly, that we needed to get some background ourselves, and with borings in progress, why, sealing and repository and are somewhat related so we felt this would be an appropriate time.

This morning the meeting will be chaired by Dr. Clarence Allen, who is Chairman of the Panel on Structural Geology and Geoengineering, and this afternoon Dr. John Cantlon will chair the meeting, and tomorrow morning, Dr. Dennis Price. Tomorrow afternoon there will be a field trip which was a great part of the reason we are meeting in Seattle and we'll have a chance to hear just a little about that in one moment.

So now I would like to introduce Dr. Allen and ask him to introduce the other panel members.

DR. ALLEN: Thank you, Don. Let me also welcome you to Washington in the winter, or at least near winter. I recently built a second home on the coast of Washington. I've been out there for three days and, believe me, this morning is the best weather I've seen yet.

Although the other Board members have been mentioned by Don, let me formally introduce Dennis Price, a member of the Board; John Cantlon; and, of course, Don Deere. Also present with us are staff members Bill Barnard, who is just approaching the table, and Russ McFarland. Russ has been instrumental in working with the DOE and setting up this particular meeting, and let me ask Russ to say a few words perhaps about this field trip tomorrow or anything else he wishes.

MR. McFARLAND: Thank you, Clarence.

The field trip tomorrow afternoon is scheduled to leave here at twelve-thirty by bus and return approximately three-thirty. Depending on interest of the group, that is the plan.

What we have is an invitation from the Robbins Company to visit their facility. There are two machines in some degree of manufacture at the plant. The invitation was extended by Dick Robbins with the idea of being able to expose the Board or DOE to some of the current tunnel boring machine technology that's evolving. I think we're seeing a rather large change in technology with the new developmental machines in Europe and the instrumentation of machines now in order to develop special purpose cutters. That is a brand new variation on this whole theme of the development of the machines.

Unfortunately, Dick Robbins was called to Europe. He won't be there, but his staff is expecting the group. Please sign up so we'll know what sort of transportation to provide. The sign-up is in the back of the room. The plant is a very short distance from here. It's no more than several miles, if my memory is correct. It's perhaps a ten-minute car ride and it's not on a main drive. You aren't going to be fighting commuting traffic. I think it will be a very interesting afternoon. They're prepared to receive us and address a number of issues that would be very pertinent to the application of machines for the repository.

DR. ALLEN: Thank you, Russ.

Before we get started, let me simply remind the participants and the audience that we welcome your comments and your questions following speakers during the meeting. Please, on the other hand, use the mike. Please speak up, and please be sure you give your name and affiliation if you do have comments or questions.

We're already ahead of schedule, so let's start right out on the program and I'll introduce Jon White with the Department of Energy. He's going to make the introductory comments this morning.

DR. WHITE: Thank you, Dr. Allen, and good morning, ladies and gentlemen, members of the Board. Welcome to Seattle, and I might point out, Dr. Allen, with regard to your comments a moment ago, many of us would prefer, I think, that a TBM manufacturer were located in Orlando, perhaps.

I'm Jon White. I'm the Program Element Manager within Department of Energy for seals and also for repository, and to go back to the original issue here of Seattle for just a moment, I understand from the hotel staff that there's a new proverb going around the permanent residents of Seattle, and that is something to the effect that they don't worry anymore about getting a suntan. They simply go outside and rust out.

The Department of Energy serves the public interest by characterizing the site at Yucca Mountain for potential licensing and use as a high-level radioactive waste repository. The legal authority for this activity is the Nuclear Waste Policy Act. Part of that effort includes research, development, and engineering for seals, for sealing a potential repository should the site prove suitable and should a decision be made to proceed with construction and emplacement. These sealing activities in which the Department of Energy is engaged are authorized and required by the Nuclear Regulatory Commission in the Code of Federal Regulations. The citation is 10 CFR 60, and we see here a few of those requirements.

10 CFR 60.112 requires the Department of Energy to design a total system, which would include sealing components, to assure that the release of radioactive materials would be below the applicable EPA standard. 10 CFR 60.113 mandates the design of an engineered barrier system to contain waste and to limit future releases, and 10 CFR 60.134 contains guidance for the design of seals for shafts and boreholes.

There is a general design criterion which is that the shafts and boreholes must be sealed in such a way as to not degrade or limit the performance of the repository to meet the standards. There is also, in 60.134, a section on selection of materials and placement methods, and a requirement that the Department of Energy minimize the creation of preferential pathways for radionuclide migration.

As a perspective to the sealing program, we find that there are publications from Sandia National Laboratory dated in 1984, and we can say then that the work has been going on for very nearly a decade. It's been a good period of development, research, and design. A great deal, I wouldn't say all, but a great deal of foundation work has been done and, of course, a great deal of effort remains in the area of obtaining site characterization data and performing in situ and laboratory tests.

Any potential repository would have a number of features which would be good candidates for sealing. In the case of Yucca Mountain, this would be exploratory boreholes, certainly, shafts and ramps, drifts, faults, fractures, joints, and shear zones, and of course, any other water-producing zones.

We see here a diagram which shows the logical relation between some of the aspects of the Yucca Mountain project. We see here in the post-closure the issue of seal characteristics which influences and is influenced by configuration of underground facilities which, in turn, has a logical relation to waste package characteristics, emplacement, engineered barrier system, and the issues of system performance, individual protection, groundwater protection, groundwater travel time, and the siting criteria which the Nuclear Regulatory Commission has established.

These post-closure issues relate also, logically, to pre-closure design issues of design and technical feasibility, waste package characteristics, and some of these other features. The purpose of the meeting today and tomorrow is to discuss--and for the Department of Energy to present--issues relating to seals, and with that in mind, the Department of Energy has a logical diagram or a flow diagram which indicates the procedure which we expect to follow in doing the seals work.

We begin, then, with a identification of post-closure requirements. We go to an identification of sealing components, and then we address the issues of functions for shaft and ramp sealing, exploratory borehole sealing, underground facility sealing, and by and large, these three issues are the ones we will address this morning.

We then go to issues relating to design performance goals and identifying site and test data, and again, those issues, in a very broad sense, are the ones that we'll address this afternoon. And then Wednesday morning we'll address the issues of the conduct of laboratory and field investigations.

This shows, in a general sense, the anticipated sequencing of the Department of Energy's sealing activities. We see here a line for the year 2001 for submit license application. Of course, in the present thought, the Department of Energy would begin construction then in 2010.

We see, first of all, that performance assessment activities will continue throughout the lifetime of the sealing program. We see, second of all, that repository design and seal design--as Dr. Allen mentioned--go pretty much hand in hand and, of course, that activity extends also into seal design for performance testing and design verification activities for the life of the sealing program.

The Department of Energy expects to determine sealing environment and perform testing long before the license application begins, and we pointed out here, also, that laboratory evaluation of materials blends into seal component and placement methods, evaluation, and one doesn't simply cut off and then the other one start. There is a blending here.

The Department of Energy expects to install the seals during the closure period, but we do not wish to say that that would preclude the installation of seals at some other time should that become appropriate, and I'd like to point out that we in the Department of Energy believe that at the present we're about here, on a rather tenuous time scale.

These are major activities of the sealing program. We see first an addressing of the requirements and performance analyses. There are an addressing of performance goals and design requirements and the Department, of course, will do performance evaluation at every stage in the research and design process. The second major activity is design, where the Department will define sealing concepts, will prepare conceptual designs for sealing components, and of course, prepare a license application design. The third major activity is testing, where the Department of Energy will evaluate materials, will determine a seal environment, and continue with performance evaluation. The fourth major activity, of course, would be the closure operations.

We see here an organization chart for the Department of Energy's sealing program. The Department of Energy has long had a relation with Sandia National Laboratories to do technical integration and program development, and Sandia has subcontract some of this to IT Corporation as a supporting organization, and also to JFT Agapito & Associates.

The structure of the presentation for the rest of today and tomorrow morning is shown here. We will discuss sealing concepts. We'll discuss a design philosophy, and also the historical work which has been performed. We'll address a technical basis from a hydrologic and an airborne point of view, and finally, we'll go into current and planned work in the geochemical area. We'll discuss strategies for exploratory boreholes and backfill and sealing, and we'll discuss field testing.

This slide begins to show the speakers for the next day and a half. Following me, Tom Blejwas will address issues of performance assessment in the sealing program. Joe Fernandez will then present twice to us on sealing concepts and progress to date in the repository program. We'll take the morning break, and after that Joe will speak to us again on hydrologic goals. John Case of IT Corporation will speak to us after lunch on airborne goals and requirements for sealing components. Tom Hinkebein will speak to us then on geochemical considerations. Joe Fernandez will speak to us again on overall approach and performance calculations, and John Case will come to us once again and present selected design calculations. Then Malcolm Jarrell from IT Corporation will discuss technologies for sealing boreholes, and Joe Fernandez will come to us yet again and discuss overall strategy to seal exploratory boreholes.

Joe Fernandez and Mike Hardy, in the late afternoon, will speak to us on overall approach for sealing shafts and ramps, and then John Case and Ian Hynd from IT Corporation will discuss technology to seal shafts and ramps.

On Wednesday morning, we'll have some opening remarks and then Archie Richardson will speak to us on technology to seal underground openings, and Joe Fernandez will speak to us once again on field test plans, and I'll conclude with some concluding remarks. After the break, then, we'll have a discussion and, of course, tomorrow afternoon we'll have a visit to the Robbins Corporation.

I'd be pleased to answer any questions of a programmatic nature which the Board or the audience might have.

DR. ALLEN: Thank you, Jon, for that overview. Are there any questions or comments from the Board or the staff?

(No audible response.)

DR. ALLEN: From the audience, one question; Carl Johnson.

MR. JOHNSON: It's not a question. I just want to make a comment and it's mainly for the edification of the Board. The emphasis of the next two days appears to be on sealing if the repository is constructed. That's one of the things the State of Nevada is interested in. Another thing the State of Nevada is interested in is the sealing of boreholes and shafts if a repository is not constructed, because there is state requirements that such sealings be done to satisfy state groundwater protection requirements.

DR. ALLEN: Any further comments or questions?

(No audible response.)

DR. ALLEN: Okay, then let's proceed with Tom Blejwas.

DR. BLEJWAS: Thank you, Dr. Allen. It seems like just a few weeks since I was talking before you last, and I guess it really was.

DR. ALLEN: It was.

DR. BLEJWAS: Well, again, I'm going to make my comments brief. I'm going to be talking about performance assessment in the sealing program, and actually, what you're going to hear about over the next couple days is a lot about performance assessment, so I'm not going to try to cover all aspects of the sealing program, but I will try to put the sealing program and performance assessment into perspective with each other.

Now, the sealing program has, in my way of looking at it, three different types of interactions with the performance assessment, and the first one is that we have to be concerned about the performance of sealing systems. Sealing systems are required by the regulations. We don't have a choice of whether or not we have sealing systems. So we have to analyze those systems and decide whether or not they're performing the functions that we identify for them. However, as you're going to see over the next couple days, most of the performance assessments that have been done for sealing systems use substitute criteria; in other words we come up with a criterion or a set of criteria for the sealing system that's based on certain scenarios or events that may occur. Quite often, those scenarios and events are fairly incredible. We haven't really assessed what the probability of them is. Instead, we're trying to come up with some kind of a bounding type of event that might occur at the site.

Based on that, then, we would do some analyses to see whether or not the sealing systems perform. This performance of the sealing systems would be a part, also, of our design of the sealing systems because we would design the sealing systems, then do analyses to see how they perform relative to these criteria and the scenarios that we have designed for the sealing system. That's kind of the first half of the picture, how do we deal with seals and what effects does performance assessment have on the sealing system.

Going the other way, what do sealing systems, how do they affect our performance assessments, the performance assessments that we do for the repository as a whole. These first two are going to be discussed over the next day and a half, as I mentioned. This one is not going to be addressed significantly, and I will concentrate my remarks on how sealing systems fit into the performance assessments that we perform for Yucca Mountain as a whole, or for the repository system as a whole.

And in particular, my comments are going to concentrate on the total system performance, so we're concerned about how important are the sealing systems in meeting total system performance objectives.

Well, there are at least four ways that the sealing system can affect the performance, the total system performance of Yucca Mountain. First of all, we know that the sealing systems are going to modify, change the way water reaches waste emplacement areas. It may make it better. It may just reduce the amount of water, or it may actually change the form in which water meets the waste emplacement areas; in particular, the waste itself.

To some degree, the sealing systems will change the potential for human intrusion, although that's not one of the primary design criteria for a sealing system. It will have some effect on the potential for human intrusion.

In addition to changing the way water could reach the waste, the seals will also change preferential pathways for waste to get to the accessible environment. Now, notice I'm using the word "change" all the time, and not necessarily "reduce" or "make better," because we're going to have to evaluate the sealing systems for that. We can't conclude that right up front.

Also, the seals are going to, to some degree, change the near-field environment. In particular, backfill that would fill the drifts--the waste emplacement drifts in particular--is going to change the way that water might reach the waste packages.

Now, we put together this little circular chart for total system performance assessment, and it's not intended to be a thorough or complete influence diagram, but rather to give you an idea of the kinds of things that go into a total system performance assessment, and I actually put this together with Dr. North in mind and I'm a little disappointed he isn't here, but I didn't want him to think that I wasn't working on some of these things when I go back to Albuquerque.

DR. DEERE: This is Dr. Deere. I'm sure he will get a copy of this.

DR. BLEJWAS: Okay. Well, let's imagine that we're in the middle of our way to license application and we have some revised total system performance assessment, and we've used that total system performance assessment for some application and determination of site suitability, design of the repository, license application, et cetera, but now we decide that we want to go through the next cycle. What are the kinds of things we're going to look at?

Well, we're going to take a look at regulatory interpretations to see if they've changed. We're going to look at event trees for the performance of Yucca Mountain, and the physical site models that are available at the time are going to change those event trees. So as we go through time, we're going to get smarter with our physical models, and we'll modify those event trees. Based on those event trees, we will have some sets of scenarios that we will analyze. We'll have flow and transport codes, and you've heard about some of those and some of those that we're developing. We're going to have to do model development and model verification--and we've done a lot of that, but we still have a lot more to do. We're going to do model validation, but model validation is going to be very heavily influenced by field and laboratory testing.

I've put in sealing technology in here as kind of a separate item because it's going to affect several of these things, and then we're going to have, based on the field and laboratory testing, we're going to have parameter interpretations. Again, I've put in physical site models again because they feed directly into our total system analysis, but the physical site models are going to depend, to some degree, on natural analogs and empirical data in addition to the things that we learn at Yucca Mountain directly.

We're going to have to use expert judgment, and we're going to have some tools that we call total system analyzers--and you've heard about some of those, and we're in the process of devising some improved versions of total system analyzers. One of the things I didn't mention was part of what goes into our models is the near-field environment models. We need those as well as the far-field models.

Okay, now where in this circular activity does the sealing system fit in? Well, obviously, it fits in here but, as I mentioned, it's going to affect some other things. It's going to affect the near-field environment, as I mentioned earlier. It's potentially going to have some influence on the event trees that we generate and the scenarios that we generate for Yucca Mountain, and it may change our physical models, our conceptual models for the way that Yucca Mountain behaves and we may have to use different conceptual models for analyzing the mountain.

So these are the three areas that I see the major impact of sealing systems on total system performance assessment. They are going to change the way, to some degree, that we look at scenarios for Yucca Mountain. I have to admit to you, though, we have a very large program going on presently at Sandia looking at event trees and scenarios for Yucca Mountain. At the present time, the sealing systems are not included explicitly in those scenarios. In other words, the people generating those scenarios did not come up with sealing scenarios as a part of the bigger picture.

The other way, though, that the sealing system can affect the total system performance is for some part of the mountain our conceptual models may be different because of the existence of the sealing systems. In addition, for some part of the mountain, the parameters that we assume for performance may be different. For example, we're going to have to have parameters for backfilling shafts. We'll have to have conceptual models for the way the water flows through that backfill on its way to getting to the waste.

But both of these things are going to be heavily influenced by the geometry of the system, and so what I've tried to do here, or we tried to do at Sandia with our graphics system is put together a cutaway view of Yucca Mountain that you've seen previously. I think Mike Voegele used it in one of his previous presentations.

Here is the underground layout, and the green lines are intended to be the drifts, and what I asked our people to do was to put in some representation of the accesses to the outside world, so they've put in two shafts. They were in the process of putting in ramps for me, but the person that was doing it got sick and I didn't get that view graph in time for this presentation, but you can imagine two ramps coming out from the north and the south, according to our present concept of some of the accesses for Yucca Mountain. The present concept doesn't two ramps, and that was one of the things I was having them change.

You'll notice, though, that if this is all of Yucca Mountain, if we have a shaft here, a ramp here, a ramp here, there aren't too many places where the conceptual model or the parameters are going to be changed significantly. Another way of looking at that would be a view graph that was used by Tom Buscheck a few weeks ago in the discussions on the thermal loading for the repository.

This was a view graph that was intended to show some concepts for the way that water is flowing through Yucca

Mountain, and I should have mentioned this right up front, but everyone on the Board knows this. We need to constantly emphasize that for our sealing system, we're dealing with an unsaturated site. So our sealing systems will look very, very different from those at a saturated site; at least potentially they could look very different.

And in this view graph--it doesn't show up quite as well as I would like--the green is intended to pass for fracture flow through Yucca Mountain, and we can see that we would start out with some infiltration on the surface, and eventually we may end up with a few pathways cutting through the repository horizon, and I'll emphasize--even though Tom didn't--that right now we would expect this to be a very few locations where we potentially could have fracture flow. When we get underground, we'll find out more how many locations that's likely to be. But in the future, as the climate changes, the number, logically, would go up.

The point I was going to make, though, is if we envision a shaft here coming down through this, what's the likelihood that some preexisting fracture flow pathway is going to be changed significantly by that shaft? There is some potential for that. We can't ignore it, but it's not likely that we're going to change significantly the total behavior of the flow system for Yucca Mountain just because we've put in a couple shafts or we've put in a couple ramps.

Similarly, if we look at gaseous flow--again, another view graph from Tom Buscheck--if we are down here in the repository and we're generating Carbon-14, or Carbon-14 is coming out of the waste packages, for example, we know that we don't expect the repository to be totally impervious. We're going to have pathways for gaseous release. Again, if we have a shaft or a ramp, that is going to change, potentially, the amount of releases. If we do a good job with sealing that, or do a modest job with that, it may not look all that different from the rest of the mountain.

So my reason for all this is it's not clear that we need to think of the sealing system as totally changing the system. What it's really going to do is probably bring the ramps and shafts and other accesses back to looking much like the mountain already looks, and that's part of the objective of the sealing system.

So in conclusion, let me say that we know that the sealing systems have to be considered in meeting the performance objectives. It's a requirement in the regulations, and we will consider them in meeting the performance objectives; and specifically here I'm talking about the post-closure performance objectives, such as the total system performance objectives. But right now, from our perspective, looking into the future, we think that the geometry and the present conceptual models suggest that the influence of the sealing systems may be small. It may be almost insignificant. Why? Because the sealing system, if it's designed and constructed properly, may very well get us back to that part of looking very much like the rest of Yucca Mountain, or not changing that part very significantly from the behavior of the rest of Yucca Mountain.

The seals will be included in future cycles of total system performance assessment, but I will admit to you, I don't know when they'll be included, and it will probably be someplace much further in the future, not over the next twelve months, for example.

Finally, there are some aspects of the sealing systems that we're going to have to look at in a fair degree of detail because they change the types of calculations we do, and that's the near-field impacts of some sealing components, particularly the backfill, because they may be important on the flow and they may have a direct effect on what we calculate for source terms for the rest of our performance assessments. Right now, the models we're including do not change because of the existence of that backfill, because the simplifications that are made, it's such a broad brush that we can't capture that. Sometime in the future we're going to have to be able to capture that.

Now, there is another thing that I haven't put up here and I should have, I just realized, and that is we may come up with a scenario where the failure of a seal is very significant in terms of total performance, in which case we would include that in a scenario and it would have a very direct and significant effect, perhaps, on total system performance.

But right now, total system performance assessment is not heavily influenced by the sealing system except to assume that the seals are going to do their job, and that's not to downplay the importance of a sealing program. Quite the opposite. We are assuming that they will do an adequate job. If they don't, we're not sure what the implications of that are, but they're probably not huge in terms of all the scenarios that we presently have to look at for Yucca Mountain.

And with that, I'd be glad to answer any questions you might have.

DR. ALLEN: Thank you, Tom.

One question I have, it's not quite clear to me why the sealing system might affect a potential for human intrusion. Can you explain that?

DR. BLEJWAS: Well, I had intended to start off with the fact that I had help with some of those first few words from some of the other people in performance assessment at Sandia and neglected to mention that, but I won't use that as an excuse. My own concept is that if you were--some of the sealing system include things like plugs, large plugs near the surface. If you're a driller, you're going to notice those things very quickly and you're not likely to ignore them. If you are going down further, there are also some backfill you might come upon that would change your perspective on what you're finding.

Now, notice I didn't say they'd necessarily decrease the amount of human intrusion. How a future society would react to finding these things, I don't know, but it would certainly change their reaction, so we would have to consider that in our assessment of human intrusion.

DR. ALLEN: Thank you.

Questions from the Board? Staff? Audience?

DR. DEERE: Don Deere. I'll come up front.

This was a little talk I was going to make a little later but I think it's appropriate right after Tom's presentation because it is extremely difficult to evaluate an unsaturated medium as compared to a saturated medium when we go underground with the exploratory shafts or with the exploratory ramps, because in a saturated medium, you can have placed before you go your piezometers, and as you go in and start cutting across various features, you'll find you get response from the piezometers in different positions and suddenly you'll say, "Well, what in the world is causing that piezometer over there 200 yards away to change while this one right next to it is not even moving?" And what you find is, of course, that you have the faults that are separating and you have different blocks, each one with its own little set of equilibrium conditions.

So when you come through with your shaft or come through with your ramp, you change them--often one at a time --or you change one through-going fault, and so anything near that will be affected and things not too far away will not be, so you start to get a fairly good idea of the geohydrology and how important your various seals are in what position.

When we're starting out with something that is unsaturated, it is extremely difficult to know about what these interconnections are really going to be and which ones are the real important ones, so I think we have a real difficult task, there's no doubt about that.

Now I'd like to talk a little--yeah, John, please?

DR. CANTLON: John Cantlon. That being the case, the substitute would seem to me some kind of gas flow system, which is opportune. Couldn't we get the equivalent of a piezometer working on gas flow to get some sort of an index of changes as these pathways are altered?

DR. DEERE: Sounds like a reasonable hypothesis. I think still more difficult to do.

DR. CANTLON: Oh, much.

DR. DEERE: Much more, but certainly you can get some idea. But the real beauty of exploration and going underground into the saturated medium is that all kinds of things happen and that's when you really learn what your system is doing, is when you perturb it, and it's going to be more difficult, but maybe the gas situation is certainly worthwhile for the hydrologists to think about.

Now, the second thing is about failure of seals and plugs. I would imagine I have seen as many seals and plugs and tunnels as anybody in this room, and perhaps as much as anybody in the world, and I've seen a number of them fail. I've seen them fail as they're filling them for the first time with pressure behind them, and I've seen them fail many years after they'd been in operation, and those are the ones that are a little worrisome, is the time effect.

So almost always, it's--Tom, when you presented there the seal technology--and it's not only the design of it, but it's that the construction of it is done as you think it should be done to meet what the conditions are right around it, and that is where there have been failures. Sometimes the failures have been due to a geologic feature that was not known, maybe just a weathered joint, and the seal was placed on top of it and eventually the water got into the joint under high pressure on the high pressure side, and very slowly seeped along the fracture that bypassed it and came up into the dry part of the tunnel--maybe 10, 20, 50 feet away--and wasn't noticed and wasn't particularly treated, other than they grouted around the seal. And in about five years, there was enough water moving through that it started to pipe the material, and these plugs were up in the side of a mountain in an old adit that went into the tunnel line for the construction. It was sealed off, and then nobody ever went back to that adit. I mean, there was no use to, and about five years later, suddenly they lost the power down at the power plant. So they started looking around, where did the water go? And it didn't take them more than about a day to find out where it went, and when they went in, this is what they had found, that they had actually piped material out maybe five to ten feet below the plug, and it kept working backwards until more and more water could come through, and then it just finally washed everything out and there was about a 20 foot long concrete seal of solid concrete broke and displaced, and all the water leaked out. So that's one that was long term.

The other happened to be in another country, in South America, and I was standing there at the time they were filling the reservoir, and we usually fill it very slowly, no more than 25 to 50 meters in a day, look all piezometers around the area, check every plug during the filling looking for cracks, piezometers around the plug to see what's happening, and all of a sudden the bottom piece of concrete, bottom half just blew out and it was quite a shock. The guy actually was standing there. Part of the concrete and the water hit him and they've haven't found him yet. He's still running. He was really scared, and I just a little less so.

But what had happened there was a design change by the construction people, without having approval by the designers. They wanted to pour the plug in three lips rather than two, and this particular plug had a lot of concrete in it because it was a steel pressure door which allowed them to open it later and to go back into the tunnel, and so they had to imbed this concrete steel door into a plug to join it, and so the contractor said it would be better to do that in three stages than in two stages of concrete. He didn't get a good seal between his first and second stage, and the water simply got in there--took it a few hours--exerted its pressure, and broke the concrete, just plain sheared the concrete off, and that was the piece that came out and let the water come down.

While I was on my way down, I saw another gentleman in the airport in Miami. I said, "Hey, Barry, I haven't seen you in quite awhile. Where you headed?" He said, "Oh, I'm headed for Colombia." "Well, what's going on in Colombia?" "We just blew a seal." So here in the same week, two seals in two different countries, after probably about four or five years of work in building these things, the very first day or the first week, as they were being filled, they got a plug failure. Now, these were under pressure, of course, and much more difficult, but by the same token, it shows that the influence of geology is very, very important, and the of the design and construction details are just as important.

So I simply wanted to point out our experience, I would say, is accelerated in terms of what we're doing here. It's accelerated in that it's being done under very high pressure conditions, sometimes only maybe 100 or 200 feet of water, but in other cases, between 1,000 and 3,000 feet of water just on the other side of the plug, and you're standing downstream of the plug with zero. So the hydraulic gradient is pretty great across there and obviously, the longer the hydraulic gradient, the longer your plug is to make sure that you get good contact.

So I think it's a very important topic, and obviously, this is one of the reasons that our Board was interested in hearing the considerations and the preliminary designs that you have been looking at, as well as to educate all of us on this particular problem. So I'll probably bring this same thought up again from time to time, but I thought maybe it was appropriate to add at this time.

Thank you.

DR. ALLEN: Thank you, Don.

Any further comments? Yes, one from the audience.

MR. VERMA: Teek Verma from M&O/MKE. In light of what Dr. Deere said, I'd like to make a comment. Last fall, I was a part of a team. We looked at Russian grouting technology and we saw its application under different geologic conditions, and we saw the work they do in the lab, and from what they indicated, that the grout they use for sealing remains pliable, and we saw examples of that grouting in two mines where they had used it to seal off a water-bearing formation, and it had a utility drift going right underneath that water-bearing formation.

And after about 15-16 years of application, we found it is holding very effectively. We found it still very pliable, and the point I want to make is that maybe we need to look at that technology and its applications for our repository program, especially for the near-field applications where it could be used to mitigate either the impacts of construction or the fractures or joints we may run into when we get underground, because the near-field is part of disturbed zone, and to get proper credit to enhance its performance, maybe you could use the Russian grouting technology.

DR. ALLEN: Thank you.

Don Deere?

DR. DEERE: Yes, I would comment. Isn't it true that in this Russian technology, they were using clays which were not necessarily bentonitic clays--because I think they were expensive--and they didn't use cement because they were a long ways from a source, and I believe the technique developed is the best they could do and, therefore, they had to pump an awful lot of material in over a long period of time, but that for purposes of economy, I don't think it developed as a better technology the normal high-strength grouts. Now, I may be wrong.

Russ, you may some insight, or perhaps you would like to comment. I don't think it was a preferred method. I believe it was the availability of materials.

MR. VERMA: Yeah. The way it started out, it started out when Russians couldn't get any cement because the cement had a very high priority, and all of it, or most of it went for defense-type projects. So that's when they started playing with clays. The kind of clays they use are caolinitic clays, but the additive they used, they claim that they could also use other type of clays but they preferred not to use bentonite. They used a very little amount of--nine to ten per cent, or maybe eight to nine per cent of cement, so the bulk of the material is clays.

They also claim that they could use fly ash. They could use some other fine-grained material if they can't find clays. But again, they are experienced. They have about 20 years of research and development effort into it, which they talk very openly about and are willing to share with us if we are interested.

DR. ALLEN: Russ McFarland.

MR. McFARLAND: Looking at the history--and they had a very unique application in going through major limestone aquifers to get at coal formations underneath Moscow Basin, the Don Basin, very deep coal. The techniques had been, that they had developed in the fifties and sixties, were the freezing techniques and Professor Kipko, along about 1965, indicated that the cost was extremely high to freeze and excavate, so they developed--as Dr. Deere mentioned--techniques unique to their particular application, and they found that in going through very permeable aquifers, even carstic limestones--and the cost of cement--that the different clay techniques, they were talking--as mentioned--about ten per cent cement, perhaps even silicates, but predominantly clay grout that were used and pumped in extremely large quantities, and perhaps in a year or 18 months required to do the zone grouting before they would start excavation of a shaft.

But once they had completed the zone grouting, the excavation went very rapidly, and the net cost was favorable to the new technology, as opposed to freezing.

DR. DEERE: Yes. Don Deere again.

Isn't it true, though, that they had a couple rather phenomenal failures?

MR. McFARLAND: They don't list them in the history books.

DR. DEERE: Well, you see, there's very, very high gradients in some of their mines. You're trying to keep the water out, and yet you have a clay sitting in there that is not real strong and so you're subject to the potential for piping through that, and what you simply have to do--apparently their experience has taught them--is just put enough in. You just put a lot of it in so that you decrease that hydraulic gradient. You get a wide enough zone that you do a pretty good job. But I think those are interesting comments; appreciate them.

DR. ALLEN: Thank you.

Okay. Let's proceed, then, with Joseph Fernandez on the first of many presentations--don't get laryngitis on us, please--on sealing concepts and design approach.

MR. FERNANDEZ: Thank you, Dr. Allen.

In Jon White's presentation, one of his view graphs showed three boxes, and the first one of those boxes, corresponding to the first part of the presentations that we'll make today and tomorrow, was associated with sealing concepts and design philosophy that we've used in the sealing program, and also historical perspectives. That was the first box that Jon White had presented. The first two talks that I will make today will address the items that he had listed in those boxes.

Again, my name is Joe Fernandez. I'm Task Leader for the Repository Sealing Program. Before starting, I felt it was very important to make it clear exactly what we're talking about when we're talking about sealing. The sealing program is part of the permanent closure of the underground facility, shafts, ramps, and boreholes. I have here the SCP-CDR design. As we all know, this design has slightly changed. Looking at the figure to the left, we have the perimeter drift. Formerly, there were two ramps coming into the underground facility. There were a number of shafts, two exploratory shafts, a men and materials shaft, and an emplacement exhaust shaft. There were a number of other access drifts and also mid-panel access drifts, and these were the primary mains for the reference SCP-CDR design.

On this figure to the right, we have again the perimeter drift. We have existing as well as proposed boreholes. This is meant to be a schematic. These are actually pretty close to what the boreholes would be. The solid bullets here represent the existing boreholes. The ones that are open are the proposed ones that are changing from time to time.

I thought it was worthwhile to go through what the sealing concepts are, the development of the sealing concepts, and where we stand today with those concepts. When the sealing concepts were originally developed back in 1982 to 1984, we considered several things. We considered the federal and state regulations. We considered the requirements that we would like to see the seal system perform. We also did some numerical analysis, very simple analysis, to see if the concepts that we would propose would seem reasonable for the site that we were looking at, so again, the site became a very important aspect of the development of the sealing concepts.

When we first developed the concepts, the objectives basically were for containment and isolation, human intrusion, longevity of the sealing components, and cost. What you see here is the primary objective, now as well as back then, was to meet the regulatory performance requirements. The first four here really relate to the first of those original objectives, containment and isolation.

We wanted to reduce the amount of water that would potentially enter into the underground facility via the shafts and the ramps. If, in fact, some water did get into the underground facility, we wanted to divert the water away from the waste package. We wanted to control the release of gaseous radionuclides. Now, this wasn't one of the original considerations that we had in the sealing program, but it is one that had evolved over the years. The fourth is to preserve the structural integrity of the host media. We're going in there with a penetration. How do we modify this rock mass? Will it enhance, potentially, the performance of the repository, or detract from the performance of the repository? The decision was made to basically try to retain this host rock in its pristine condition, or pristine perhaps isn't the correct word, but to try to minimize the amount of structural disturbance that we would create for the host rock.

The other objective was to limit or deter human entry. These objectives are contained in, actually, several reports; the original report, the Sealing Concepts Report, Sandia 83-1778, as well as the Site Characterization Plan that was issued by DOE.

I mentioned one of the very important considerations in the sealing program was to take a look at the site. I think it's a very logical first step. What I've done here is just categorized the basic geologic units, and I really won't talk too much about geology in the presentation today and tomorrow because at this point in time I think we know a fair amount of the geology. We certainly need to know a lot more, but many of our analyses, many of our performance calculations have considered three basic rock types.

The first is a densely welded, low porosity but highly fractured units with a high hydraulic conductivity.

Second is a non-welded, porous, zeolitized, but relatively non-fractured unit with a low hydraulic conductivity, and the third is a non-welded, porous, vitric, but relatively non-fractured units with a high hydraulic conductivity.

Now, if we were to go down in the stratigraphic sequence, as you may recall in Tom Blejwas's slide, first we had the densely welded, highly fractured Tiva Canyon unit. Underneath that, we had the Paintbrush Tuff, which is comprised of the Pah Canyon and Yucca Mountain members, as well as any bedded tuffs. Below that, we had the Topopah Spring member, the densely welded, highly fractured. Below that, we have the Calico Hills member, vitric and zeolitic.

This first bullet would apply basically to the Tiva Canyon and the Topopah Spring densely welded units. The second bullet would apply to the Calico Hills zeolitized unit, as well as portions, perhaps, of--well, basically the Calico Hills unit. The third would apply to the Calico Hills as well as the Pah Canyon member; the Pah Canyon, again, between the Tiva Canyon and the Topopah Spring.

DR. DEERE: Don Deere. If you could keep that on for a minute, I think this is very interesting the way you have expressed that, because it's almost the opposite of what the people at the University of California have used, or at Lawrence. They have referred to what I thought was confusing terminology, apparently because their earlier work was in matrix permeability, so they refer to those just the opposite, you know. They would say that first unit is low permeability, and I remember I was very confused by that presentation. And then when they got into the non-welded, zeolitized, it was called their high permeability unit, while here, of course, it's low hydraulic conductivity. And they were, I think, speaking entirely of the matrix flow. The way you have used it here, I think, is much clearer and, I believe, more correct.

MR. FERNANDEZ: This is the bulk rock properties rather than matrix properties, or strictly the fracture properties; correct.

Again, I'd like to re-emphasize, we are in the unsaturated zone. When we first developed the sealing concepts, we said, well, what assumptions should we go with? Well, these were the assumptions that came out of our first go-around. We assumed that we had predominantly vertical downward gradient as a driving mechanism for groundwater flow. Lateral flow is possible at the material contrast interface as in between, we'll say, a non-welded and a welded tuff unit. The third bullet here is the flow in discrete fault and fracture zones is a potential flow mechanism, and vertical flow is expected to dominate and lateral flow is expected to be minimal at the storage horizon. We really don't expect very much flow, but conversely, the sealing program has been structured around looking at unanticipated conditions as well as anticipated conditions.

One of the logical things to look at was analog systems. Is there something that we can learn from knowledge elsewhere that might help us in the development of our concepts? Well, the first place to look was at the Nevada test site, where they have a number of tunnel complexes. Two of those tunnel complexes, G-Tunnel and E-Tunnel, some information was available on those on the water inflow; not very much, but there was some. So our first strategy was to take a look at what we can learn from those tunnel complexes as far as water inflow, recognizing that their geologies were somewhat different, but nevertheless, they were fairly close to the site in question.

In G-Tunnel, there was one water-producing fault zone, and that water-producing fault zone had a flow rate of approximately .01 gallons per minute, approximately 100 gallons per week in other terms. It was also observed from some of the hydrologic analyses that were performed that the hydraulic conductivity, the saturated hydraulic conductivity could vary by several orders of magnitude over very short distances.

In the case of E-Tunnel, they found that the only free water was in open fractures, mostly fault zones. There were initial discharges, typically around 20 gallons per minute, decreased rapidly within by an order of magnitude within a month. The 50 per cent of 110 faults yielded 98 per cent of the water. There was poor hydraulic connection between the fractures. The whole system, really, was based on draining perched water. Once those lenses of water were penetrated through the drain, there was very little water subsequently that came from those fault zones or those water zones. And finally, the saturated matrix retains water.

Now, all of these certainly aren't necessarily--there isn't a direct analogy between those tunnel complexes and Yucca Mountain. Nevertheless, I think there's a couple of things that I'd like to point attention to: One, that the water inflows in the unsaturated zone were actually quite low. Normally, when we think of saturated zone flow we think of on the order of 100 gallons per minute or 1,000 gallons per minute and very difficult situations to grout. Here we were dealing with .01 gallons per minute.

The other thing I wanted to point out is that the saturated matrix retains water. There was one reference in the report by Bill Thordarson back in 1965 where they had observed these boreholes that were penetrated into a non-welded material that had a very high saturation, but yet there was no freely-flowing water out of that borehole. I think that's important to keep in mind, particularly when we look at some of the numerical analysis that I'll present in a minute.

The next two slides deal with the fundamental or the basic hydrologic analyses that we used in order to develop the sealing concepts. We have a site potentially that is densely welded, highly-fractured tuff. It has the capability of draining an awful lot of water from a fracture sense or a bulk rock sense. We also have some elements of it which would still be dominated by matrix flow.

In our first set of analyses, we looked at the flow from a matrix standpoint around an emplacement drift, and here is the waste package, and also with a shaft that had an incline contact with an upper host rock formation and a lower host rock formation. Again, we're looking strictly at matrix flow at this time, and the objective of the drift analysis was to determine if the backfill can enhance the performance. If we were to place different types of material here, could we somehow control the flow that would pass the waste package?

Our approach was basically to use sand and clay as the backfill material, and we also varied the rock conductivity by several orders of magnitude. At this point in the development of the Yucca Mountain Project, or the NNWSI, as it was called back in those days, the information on matrix property of rock were very limited. Some were just being completed by Sandia and Pacific Northwest Laboratories, and we had used some of the basic information that was coming out at that time, which corresponds pretty well--if you look at the matrix properties--to what we're using right now, but not exactly. In fact, what we had used was slightly higher saturated hydraulic conductivity than what was published later, which actually make this situation or emphasize some of the points that I'll make a little bit better.

Anyway, what we were trying to do is we had, again, vertical flow going around the drift or through the drift, and trying to assess how we could control the water past the waste package. Four analyses were done there, basically looking at different rock types and looking at sand and clay inside the drift.

The second case here was to take a look at welded tuff and non-welded tuff, flipping those over, doing a sensitivity analysis, basically five analyses, where we tried to encourage water by selection of properties into the shaft. And we would, for example, increase and decrease the hydraulic conductivity of the clay material, and also have a sand material to see what the effects would be. That was the first set of analyses that we did.

The second was looking more at fracture flow of bulk rock, the influences of how much water we can actually drain in this highly fractured media. The concept here is if we have water coming into a shaft, how effective would the shaft be in draining off that water? Again, we're not in the saturated zone, so we wanted to take a look at some of the unsaturated analysis, unsaturated work that was done for boreholes, so what we had done is taken a look at two analytical approaches, Glover and Nasberg-Terlatskata, which was presented by Stevens in one of his papers back in 1982, to try to determine how much water we could conceivably drain from the bottom of the shaft.

The other missing part of this numerical analysis, or this analysis was, you know, what really is the bulk rock hydraulic conductivity? What are the properties of the fracture? Some work that Roger Zimmerman had done approximately in that time, he looked at the welded tuff in G-Tunnel, and he had looked at a discrete flow through fractures and he had some very low hydraulic conductivities and some higher hydraulic conductivities. We had taken the lowest values that he had come up with in welded tuff, and we came up with a bulk rock hydraulic conductivity, basically, of 10^{-6} centimeters per second saturated hydraulic conductivity. We also had taken the average of the fractures as far as the bulk rock, and that value was 10^{-4} centimeters per second.

We applied that to this particular situation to find out how much water actually could drain at the bottom of the shaft. We did the same thing for flow in a drift. Now, the point that's missing here, or the part that's missing here is how much water do we really expect to have to actually flow into the shaft. We had come up with some preliminary performance criteria, as Tom Blejwas had mentioned, to see if, in fact, these concepts were viable.

We have looked at a very coarse material, almost a rubbley material just being dumped down the shaft. We had assumed 7 per cent settlement over the entire length of a 370 meter shaft, and then we looked at what sort of surface depression would occur at the surface, and then we assumed that all of the rainfall that would fall within this depression would be transmitted into the shaft, okay, and that was one input of water into the shaft.

The second was what if we had a series of fractures at the surface that would actually penetrate into the shaft, and we had a certain percentage--it was 2 per cent, I believe--of the annual rainfall that would be transmitted from the fractures into the shaft. Well, those numbers came up to approximately 100 to 150 cubic meters per year, and so that was one of the performance criteria that we had for the shafts.

For the drift, the performance criteria was the .01 gallons per minute that we had observed in G-Tunnel, and we said, can we drain those quantities of water with these concepts?

The results from the hydrologic analyses are those that are listed here. From a hydrologic viewpoint, backfilling the repository and the shafts is not essential. Again, we only looked at matrix flow.

The second bullet: If backfilling is needed for other reasons, coarse materials are better because of their capacity to drain and act as capillary barriers. So a sand actually had more diversionary capacity, considering matrix flow in the unsaturated zone, than would the clays. In fact, the sand drained very rapidly and had a very low saturation state looking at steady state conditions.

Third point: Greater inflows into the drifts may occur when saturation in the surrounding host rock formation is high. As I mentioned before, in E-Tunnel, they observed that the saturation state of the rock could be very high and there is no water inflow, so I think it's a very important point that the numerical analyses are also indicating basically the same thing.

The fourth point: The anticipated drift inflow can be effectively drained through the drift flow, and what I don't have here--which would be comparable to this, would be the anticipated flow into the shafts can also be drained very effectively, as described.

DR. DEERE: Don Deere here again. But isn't this sort of symptomatic of what was going on back at this time?

Concentration was on matrix flow. It seems like when we first came into the program, we heard nothing but matrix flow three years ago. We said, well, how about the water that's coming through the fractures? That's where we find it, and it seems that if you read those conclusions right there now--and I'll bet you matrix flow only probably wasn't in those original conclusions.

MR. FERNANDEZ: Oh, it was in the original conclusions for us, yes.

DR. DEERE: Was it? Okay, I'm sorry, but when, you know, when you read that and you say, well, is matrix flow the only thing we're really talking about here?

MR. FERNANDEZ: No. Certainly, the sealing concepts are looking at fracture flow, the flow through the rock. If, in fact, we have enough water there, the flow will occur through the fractures, and certainly, there will be a certain amount of water that's imbibed into the matrix. But, you know, part of the problem we had back in 1982, there were, I think, very few or no codes that were actually available to look at combined matrix/fracture flow, so we were really restricted by the techniques that were available at that time to look at matrix flow only, and then we had to go to some other situations, look at analytical solutions that were developed by others in the field, analytical solutions developed around boreholes and unsaturated zones in order to look at the effects of the bulk rock, and that's why we went to looking at the drainage capacity of the shafts and the drifts and doing some very simple analyses.

DR. DEERE: Because the experience during the construction of many of the drifts at the test site indicated you didn't have any water in your drifts until you hit a fracture.

MR. FERNANDEZ: Exactly.

DR. DEERE: Or until you hit a fault, and often you found that the tuff, the matrix rock back from that fracture was entirely different than it was when we got, let's say, 50 or 100 feet away, simply because of the alteration that was associated with it. We had much different properties even of the matrix, much more montmorillonite and secondary development, but the water didn't come from that even though they were close to saturation. The water came from the faults. Every time you hit one, you usually got one.

MR. FERNANDEZ: What I'd like to do is go into the sealing concepts. There's only about five slides here, and one thing that should be really obvious is these are not your typical sealing concepts as you would, perhaps in a mine in which you're trying to control high water inflows, these are not developed, or the basic concepts are not developed around high water inflows. They're catered to our site, although there are some designs that we've looked at in order to address highly unanticipated conditions, and you'll see some of those. These, in total, represent our reference conditions, and these are also presented in totality in the reports developed as part of the sealing program, and also in the SCP-CDR in the SCP.

The first one is the shaft sealing concept. Originally, we had a large concrete barrier at the surface to deter human entry. We had an anchor-to-bedrock seal which was used primarily to restrict some sort of surface water or perched water situation that potentially could occur at the surface. We had a general shaft fill, which would be filled with the material that was basically excavated or some other shaft fill that would suit the performance and other goals for the program. We had proposed some settlement plugs if they were necessary, and the other thing is we had a large station plug which would be used basically to restrict any-- to deter or restrict any large amounts of water that might actually enter into the drift and then subsequently go off into the repository horizon.

The feature that is not, unfortunately, drawn up here but I think it's in your view graph, is we also had anticipated to remove the liner at the bottom of the shafts. This was to get intimate contact between the water and the rock so that we could drain the water. That's not to say that it wouldn't be there in the original construction, it would be; but later, we'd remove it for post-operational considerations.

Our thoughts had evolved a little bit since the original concepts, and that's what this figure on the right represents. Rather than have a massive concrete structure which we'd have to design in some major way, an engineering design, we felt it better just to cover up the entire shaft and restore it to its original condition, using a similar type of design that's being used at low-level radioactive waste facilities, where we have a capillary barrier theory, which is one that has been used by the NRC, or at least the concept has been adopted by the NRC at least for the last decade.

And here we have riprap, a rooting medium. We have a sand, low permeability clay, which would divert any of the water that might get down to that point, general compacted fill, and basically the rock itself. So our thoughts had changed. We no longer wanted to have this massive structure at the surface. We wanted to restore it to its original condition, and I think this, in fact, actually achieves the objective of deterring human entry much better.

The second concept is the drainage enhancement concept. This one here is basically catered to small amounts of water inflow. It can be either in the emplacement drift or it can be in the non-emplacement drift, or, in fact, this lower figure could also be in an emplacement room. The intention was if any water comes into the underground facility, to drain it at its location, back into the rock, not have it migrate over long distances, and we felt with low inflows we could do that very handily by having a French drain concept, excavation at the bottom of the floor as shown in this bottom figure, or we could even have some drainage boreholes that could be located between waste packages if it felt that the volume of water was low enough so that it would not affect the quality or the performance of the repository and the performance of the waste package.

Benefit is this is a very simple way of controlling small drift inflow, and it also increases the drainage and storage capacity of a floor.

Here's a water control concept. Here's the development of a circular opening by a TBM. What I've shown here is perhaps an excavation at one side of the drift in order to control or to divert water away from the waste package into a non-emplacement area where the water could conceivably be drained if we decided not to go with the options that I mentioned on the previous view graph.

Here's another TBM, but in this case here, there is no excavation at the bottom of the circular opening, which would mean that if we wanted to control, let's say, the equipment moving in and out, we would probably want a flat surface for that equipment, so we probably would emplace the waste package at a slightly higher elevation. This would raise some problems, I think, for sealing program if, in fact, this design were to be implemented. What I've shown here is the potential for putting in a filter over the waste package, such that the water--if, in fact, there was any freely draining water that came into the drift, it would be isolated basically in this area, and this area of the drift would serve the same function as this additional excavation on the other side. But this also complicates things, because if there is any leakage along this interface, the first place that it goes, potentially, would be down to the waste package. So I just wanted to point, you know, that potentially difficulty out.

The other thing is in non-emplacement areas, we have the option of going back if we need to and control the direction of the water flow by simple excavation means if we decide not to do that while the facility's being constructed, or if there is no need, of course, we don't have to worry about that.

This fourth view graph is a view graph that illustrates the single embankment concepts. Option 1 was the first concept that we came out. We said, well, what if we had a fair amount of water coming in and we wanted to restrict the lateral migration of that water? Well, why not use a dam? You know, dams are very common features on the surface. It seemed a very logical approach. The benefit of the first one was to retain large amounts of water; again, increase the drainage and the storage capacity behind single embankment; and it also could be emplaced in pre-defined areas, in non-waste emplacement areas as we did our excavation. We could say, this area would be the best and therefore we would like to retain the quality of the rock upgradient from the dam in order to enhance the flow.

The Option 2 was to say what if we wanted to restrict the phreatic surface that would develop through this dam, and one concept is just to put an impermeable layer on one side of the dam. Basically, it has the same benefit as Option 1, but it also reduces the extent of the phreatic surface so that we don't have the phreatic surface perhaps breaking out of this side of the dam.

Option 3 was basically the same as Option 2. It, however, was just a single-layered system that will be placed at perhaps a two-to-one slope--two horizontal, one vertical--and the conductivities would be graded back to the coarse or high hydraulic conductivity, which is represented by the general backfill here. Okay, so we're looking at perhaps the material having a .1 centimeters per second saturated hydraulic conductivity, perhaps a 10^{-4} centimeters per second saturated hydraulic conductivity, and then an internal core of clay, which would be 10^{-6} saturated hydraulic conductivity, centimeters per second.

The final design was, what if we just contrasted that with two materials? We have a coarse material over here and a finer material over here, and we reduce the hydraulic conductivity by a couple orders of magnitude, two or three orders of magnitude. Well, that's actually a very simple type of concept to implement, and I think we're leaning, really, to one of these two designs, and in the third option, even though these bands are shown to be very narrow, it doesn't mean they have to be very narrow. They can be, in fact, very wide. From an emplacement standpoint, it's probably better to make them much wider as opposed to much thinner.

The final concept that I have here is for highly incredible type of flows. These are the more traditional type of seals that you had talked about earlier. In this case here, if we had a fault that was producing a fair amount of water, our intention would be to emplace large, rigid concrete seals, perhaps, with some sort of a grout curtain around it such that we could, knowing the geology, enhance or encourage the water just to bypass that particular area of the repository. It has the benefit of isolating large amounts of water from the waste, and it also potentially would preserve the integrity of the host media.

The second concept that's shown here is much similar to the upper one. It's just a matter of where it's located. Here we are at the shaft. This is the repository drift coming off. Here's our rigid seal, plus some sort of a grout curtain above and below, such as to restrict the water to drain at the bottom of the shaft or to reduce the amount that might get into the repository drifts. It basically has the same benefits as the previous concept.

The second part of this presentation is the approach that we're using in the sealing program. Our approach has always been to try to maintain an integrated program, a program that looked at the site characterization, the properties of the rock mass that we're having to seal. We're looking at the repository design. It's a maturation process. As we go from a simple design to a more complex design to the final design, it's important to iterate back and to know how that design might affect our sealing concepts; and finally, to look at seal performance evaluations.

Where we are right now is in the process of refining those requirements, whether they be performance requirements, design goals, or design requirements, and hopefully we'll be moving into the testing program in the very near future, looking at more sophisticated laboratory analyses and fuel testing, as appropriate. Our focus in the sealing program very clearly and succinctly could be stated as to reduce the uncertainties associated with the sealing components.

What are those uncertainties? Again, we can use those three same categorizations. We can look at the site, the design, and the seal component. The uncertainties for the site are hydrologic. What is the dominant flow mechanism at the site? What are the variabilities in the properties? What's the flux? And, very importantly, what is the location, frequency, and quantity of the amounts of water coming into the underground facility; where are they? It's a fundamental question that we really need to know.

Nevertheless, I think we can still proceed, obviously--and have proceeded--in the sealing program to try to come up with some estimates of what these water inflows would be, or some preliminary characterization.

The second bullet, the rock properties, what are the mechanical, thermal, and the geochemical properties? They're very important to the sealing program. What are, also, the future conditions?

For the design, what is the construction method that will ultimately be used? We've talked about drilling, blast, and TBM. It's important to know what method we'll be using. What's the grades and the dimensions of the drifts? They've actually helped refine our thought as we developed the SCP-CDR. Knowing what the design was, we actually made some modifications to the sealing concepts.

What is the number and the interconnection of the drifts? It's interesting to point out here if we don't have a modular system for the repository, there may be more "sterilization" of the repository that may occur, because if you have large amounts of water coming into the underground facility and you don't want that water necessarily to contact the waste package, you may have to isolate more of the system if you have designs that are not more modularized, and this is something that we learned and were able to apply in the SCP-CDR design. And finally, what are the thermal loads? Very important, particularly from a geochemistry standpoint, but also from a structural mechanical standpoint as well.

For the seal component, what's the required performance and what is the actual performance? What is the longevity of the sealing component? What is the design life of the sealing component? And finally, what are some of the emplacement concerns? And I'll talk about these and other people will talk about these in the course of these two days.

What is the approach that we're going to use in the current design phase? Again, the three categories: site, design, seal component. Well, our approach has always been to select a broad range of site properties in the analysis. Since we really don't know what those properties are--we know fundamentally what they are, but we certainly need more characterization--it's important to select a broad range of site properties; also, to estimate the anticipated and unanticipated conditions that we may have to seal to.

Under design, to develop constraints for the repository system, what things seem logical as far as constraints for the sealing program to impose on the repository design system that could enhance the performance of the repository eventually? The design also provides focus for seal location and size. One design, the number of seals might be very different than another design; and also, where you would locate those would be very different. So it's important for us to know where the design is for our design purposes, what the overall repository design will be.

Under seal components, the proposed alternative seal designs, you've seen those alternative seal designs for a small amount of water coming into the repository and large amounts of water coming into the repository. Select conservative design requirements. The third talk that I'll be making will look at the hydrologic design requirements, and those are based--those are truly conservative design requirements, as I hope I can present later.

Establish the general design requirements, and in this, there are certain things that just make common sense. For example, to locate seals in lower temperature areas to avoid trying to prove that a seal will have longevity over a certain period of time in a high thermal environment. If we can locate them out of those high thermal environments with simple materials, I think we've achieved an awful lot. So that's one of our design approaches, if you will, in the sealing program; and also, to use man-made materials. We have proposed to use earthen materials or non-man made materials for the majority of the underground facility. Only when we have to will we use man-made materials, such as cementitious materials.

As a very important point, most of the underground facility will have a rock fill, as we're proposing. Where we need to, we'll put earthen materials in those areas where we need to restrict the water flow, so that's a way of trying to accommodate some of these uncertainties.

I have two more slides here. I'll go through those very quickly. One deals with the constraints on a repository system. These are ones that are in the SCP-CDR or the requirements documents that the project currently has.

Under water control strategies, to control the drift grade, provide selected water storage and drainage capacities, to ensure reliability of the drainage areas. As I mentioned earlier, one of the benefits of going into an underground facility is to select or predetermine where these drainage areas should be and try to preserve the integrity of those areas in order to enhance drainage flow into the rock mass. And finally, to control and monitor the use of water, whether it be for construction or whether it be for testing.

Under design control strategies, a very obvious one is to locate the shaft collars above the flood plain. In one of the previous designs, it was really down in Coyote Wash, as you may recall, and it certainly is more preferential--as you'll see through some of the analyses--to locate the collars a little further away from the flood plain.

Do not emplace any grout in those areas where we think we might have grout emplacement or seal emplacement. It's very difficult to remove grout from a rock mass, particularly if that's where we want our seal to be. So we've tried to work with the designers in this particular area in order to achieve that particular constraint.

To limit the excavation damage is the third bullet. To control the grades to divert water away from the waste package, I've touched on that already. And also, to have stand-off distances between the exploratory boreholes and the drifts. John Case will be talking a little bit about this and some of the structural considerations associated with that bullet. And finally, to design liners for easy removal, as I pointed out in the sealing concepts, one of our ideas is to remove the liner at the bottom of the shafts in order to enhance the drainage capacity at the bottom of the shaft.

And finally, seal component uncertainties, what are they? I mentioned earlier performance and emplacement, two basic categories. Under performance, what is the hydrologic performance of the seal in situ? Another major issue is the drainage capacity of the rock. The three things that we're primarily interested in here is the geochemical alteration of the flow through the fractures. What happens if we have slightly heated water? What type of precipitation and minerals might we get in the fractures? Thermal effects on fractures, do they close or open? How much do they close and open, and can we still achieve our design goals? And finally, fines migration, we have a rock fill material in these drifts. We probably will have some fine materials in there. How does the presence of those fines in the rock fill in areas behind these single embankment structures, how much migration might we anticipate to occur, and does that migration influence the capacity of the rock to drain by the fact that the fines will get into the fractures?

Under emplacement, we have several items: liner removal, keyway excavation, grout emplacement, shrinkage and temperature effects due to the emplacement procedures, fines creation just from the emplacement operation--you have the material, you haul it out and have to bring it back in and you create a lot of fines, so we'll have to go through some sort of a processing of this material--and finally, the emplacement of the seal component itself, and that concludes my talk.

DR. ALLEN: Thank you.

Comments or questions from the Board? Staff?

MR. McFARLAND: One question, Joe. You indicate that these studies were done sometime late seventies, early eighties, your Sandia 83-1778?

MR. FERNANDEZ: It was done in the early eighties.

MR. McFARLAND: Early eighties?

MR. FERNANDEZ: Right; '82 to '84.

MR. McFARLAND: And this was done under the assumption that all of the candidate sites were in saturated geology?

MR. FERNANDEZ: No. It very clearly recognized at that point that this was unsaturated site, and one of the difficulties that we had is convincing people that we were in the unsaturated zone, because many of the regulations back then were written for saturated zones, as you know.

MR. McFARLAND: In looking at the USGS report, Circular 903 and the National Academy of Science report in 1983, these were the first recommendations to DOE, I believe, of the National Academy Committee and the USGS to consider an unsaturated geology. Up to that time, unsaturated geologies had not been part of the candidate site. If these studies were done prior to that, how was it that you were considering an unsaturated zone?

MR. FERNANDEZ: Well, when we first started the sealing program, we actually were in the saturated zone. In fact, I had some numerical analyses that looked at seals, the effectiveness of seals in saturated environments. We were transitioning into looking at a unsaturated zone site, so we started approximately around 1982. The thought back then was saturated zone thinking, and very quickly we moved out of the saturated zone thinking into the unsaturated zone thinking.

MR. BLANCHARD: Max Blanchard. Russ, maybe I can help. I think a lot of people were looking at radionuclide retardation and migration concepts during the early eighties, and it wasn't unique just to those two reports that the idea came out. I think some laboratory studies and hydrologic modeling that were going on by people studying potential sites in the southwestern U.S. also, that unsaturated zone concepts had some inherent benefits that saturated zones didn't have, and it's not surprising that laboratory and, say, theoretical modeling studies were being done at the same time by different people, and I think those who formulated the write-up that went into those two reports were drawing information from these laboratory studies and these modeling studies, so there were groups of both empiricists and, let's say, modelers looking at this at the same time.

Don, if I might ask you a question, I'm wondering if somewhere along the line in our briefings to the Board we've not been clear enough in our discussions about fracture versus matrix flow? You asked a few questions this morning about--perhaps I misunderstood, but it seemed to me like your question was something like have we shifted our strategy in understanding fracture flow, and if I understood the thrust of your questions this morning--well, first, have I?

DR. DEERE: I think our impression is there's been a great shift looking more at fracture flow, which we are in agreement with.

MR. BLANCHARD: Yeah. Okay. Well, I think the maturation in the project with respect to groundwater travel time and flow directions and how that ties to rock mechanics, hydrology, and geochemistry has matured a lot in the last ten years, but over those ten years, it always started out with an understanding that both fracture and matrix flow occurred, and you'll find a lot of modeling and a lot of empirical studies were done in the laboratory in the area of fracture flow, looking at the different concepts of the way fluids and radionuclides migrate in fractures.

And the dilemma has always been: What is the influx now that's infiltrating, and at what point do you switch from matrix to fracture flow as a dominant mechanism? And in the early 1980's, one thing that comes to mind is a report by Scott Sinnock, where he was calculating our early perception of groundwater travel times and his equation used both fracture and matrix flow and, of course, the groundwater travel time changed by orders of magnitude--getting shorter, that is--as the infiltration rate went up, and the flow regime changed drastically from matrix flow to fracture flow, and I think we've got a lot more people studying both matrix and fracture flow and maturing these concepts more, and the perception is maturing in fracture flow perhaps more than it was earlier because not a lot of studies had been done in fracture flow, even though there were lots of empirical observations in minds about how important fracture flow was. It was more readily accommodated in models to stay with matrix flow concepts, because they were more mature, I think.

But at this point, I think we've seen not a change in the program emphasis as much as a better understanding as a consequence of maturation in both some laboratory studies, some field studies, and some modeling.

DR. DEERE: I would go back and say that in June, 1989, in the first meeting of this Panel in Las Vegas, we were really overwhelmed by matrix flow by all the presenters, and every time we asked the question, "Well, how about fracture flow?, oh, no, it gets adsorbed by the matrix," and we had such in values and all kinds of things and the impression was very strongly, "Don't worry about fracture flow. The matrix will take care of everything." And we raised questions about this, and we always had, "Well, we're looking at fractures, too," but that was about all that was ever presented. We never really got much in the way of fractures. The emphasis was not on that.

MR. BLANCHARD: I think those questions were asked of our hydrology group.

DR. DEERE: Yes.

MR. BLANCHARD: And, of course, the people that are more concerned with the balance between fracture and matrix flow are those people who bear the responsibility to calculate groundwater travel time flow paths and directions, which are the people within the performance assessment group. And so I think those people would have answered the questions considerably different, wouldn't you say so, Tom?

DR. BLEJWAS: Yes, I think they would have. They would have answered it very differently, but I do think that we were immature in terms of our understanding of the flow mechanisms and how they may affect performance. I would say I don't think so much our perspective has changed that we think that Yucca Mountain will be dominated by matrix flow. I still believe that to be true, but I believe that if Yucca Mountain will fail, it's going to be because of the predominance of fracture flow; hence, we have to be looking at the potential for that failure; and hence, we see more of a effort to look at fracture flow and the capability for fracture flow at Yucca Mountain.

We still have calculations that we believe that would show that if you have a fracture flowing in the highly unsaturated medium, indeed, it's going to eventually get sucked into the matrix and you're not going to have a single pathway all the way down from the top of Yucca Mountain to the water table, unless you have a very strongly preferential path that we would avoid with our waste emplacement. Hence, it probably will not be dominated by that, but it may be, and we have to look at it.

DR. ALLEN: A comment by Carl Johnson.

MR. JOHNSON: Carl Johnson, State of Nevada.

Originally, when I was going to come up here I had only one comment. Now I have two. Let me start out with the second one that has to do with the remarks that Max, and the interchange between Max and Don on matrix versus fracture flow. I think if we go back in the program to the early eighties and development of the environmental assessment for the Yucca Mountain Program, fracture flow was hardly even mentioned. The emphasis was on matrix flow. We commented extensively on that in our belief that fracture flow dominated the flow regime; matter of fact, we still believe that.

The site characterization plan, while it recognized the possibility of fracture flow, did not consider it in any of their detailed plans or the performance assessments, and I think I agree with Don Deere in that we have still heard little through the last few years on fracture flow, and I think that's borne out in the study plans that we've reviewed so far. Still the emphasis is on matrix flow.

The other point that I wanted to make was I think there was one aspect missing from the presentation, and that had to do with the observations that have been made in the tunnels complex on the Nevada test site. The Desert Research Institute, which has been doing the hydrology of the Nevada test site for a number of years, the observations they have made in the tunnels is that there is significant increases in flows in the fault systems after major storm events. This needs to be considered in seal designs, that sort of thing, and almost importantly as the increase in flows, is the change in the hydrochemistry that takes place.

I think that when we get to Yucca Mountain, the same things will probably occur, that we'll have increases in flows and changes in hydrochemistry after storm events, and I think that needs to be considered in your design of your seals, especially in considering the potential of short duration major flows in your designs.

DR. ALLEN: Okay, thank you.

Joe, if it's all right with you, I think we might take the break now.

DR. PRICE: Let me ask a quick question for clarification here. On your shaft sealing concept overhead that you presented, changed from a concrete-type plug at the surface to earth materials, and just for clarification--you may have said something and I missed it--does that reflect, also, a change in your view of that barrier with respect to human intrusion?

MR. FERNANDEZ: I don't think it really does. I think it probably enhances the deterrence of human intrusion. It probably is more beneficial because if we restore the site, the detection, the obvious detection of some large structure being there is no longer there and I think it actually probably is better to meet that particular objective by restoring it to the original condition than leaving a massive structure on the surface.

And I think there are, also, other large, rigid bulkheads that are still proposed for the shaft, which will also deter human entry.

DR. PRICE: So the argument is that this is a camouflaged thing?

MR. FERNANDEZ: I think that's true, yes.

DR. PRICE: Well, could you not put the man-made material cement down deep and then camouflage it?

MR. FERNANDEZ: Well, I think the slide did show that. The anchor-to-bedrock seal has always been, the intention has been to place that close to the surface, between the alluvium if, in fact, there is alluvium there, and the first--the bedrock, the Tiva Canyon member. So I think the intention is still to do that.

You know, I would also say that it's important to look at the final requirements. This is by no means a final design. There may be multiple bulkheads in there and there may not be. There may be just a couple.

I would like to respond to the comment--I guess the nature of this presentation, I think Carl Johnson of the State of Nevada had raised a concern about me not addressing the aspect of water, the periodic water inflow into fault zones; in other words, we have some sort of large precipitation event at the surface and then we see that precipitation event at depth, and I think one of the slides--and the purpose in showing the slide on uncertainties had raised one issue, and that was the location and the nature, you know, of the inflow.

The purpose of that slide was to address that particular concern. We don't know what the interrelationships are between surface precipitation events and the potential to see, you know, water inflow at the repository horizon. So we are considering, or we are concerned with that uncertainty in the sealing program and we very much would like to get underground and find out what the site really is like in order to reduce that uncertainty.

DR. ALLEN: Okay, thank you. Is it all right with you if we take the break now?

MR. FERNANDEZ: That's fine.

DR. ALLEN: We'll take a fifteen-minute break.

(Whereupon, a brief recess was taken.)

DR. ALLEN: Okay, Joe. May we proceed with the next presentation, which is on progress to date in the repository sealing program?

MR. FERNANDEZ: In this presentation, I'd like to present some of the basic facts, you know, the reports, et cetera, that have been prepared as part of the sealing program over the last eight years or so.

The work that we've done can be classified, really, into two categories. We're a very small group and as a result, we have to--we split ourselves in the area of general support to the project, as well as specific topical reports; topical reports that deal with planning, performance, and materials. And as I go through my presentation, I will hit the general support to the project in one view graph, and then the rest of the presentation we focused around topical reports; the report itself or the study that was performed, the approach, and then the conclusions associated with that particular work.

The support to the project is really summarized in this view graph, the one that I mentioned that I'll present.

Through the course of the sealing program, we've provided support to the Yucca Mountain Project Office in coming up with or supporting the first concepts report that was prior to the SCP-CDR report. We provided input to the SCP-CDR, the SCP. There were several ES performance activities that sealing personnel were responsible for which were also included into the SCP, and some other topical reports, one of which I'll mention later.

There was the involvement with the exploratory shaft fault technical assessment review associated with the old exploratory shaft location in Coyote Wash. We had a significant amount of involvement with the NRC position paper on sealing, the one that was published fairly recently. There were a number of NRC action items that we had to respond to. They're not only action items from the NRC staff, but also from their consultants from the state; from, actually, the State of Nevada, State of California, all of which are time-consuming, but all of which are very necessary, I think, in the way in which we're progressing with our project in order to respond to some of their concerns, but nonetheless, they have been very time-consuming.

We had one very significant interface with the NRC back in August of 1985. I thought that interface was very good. It's good to hear what other groups are thinking, and

I certainly would personally encourage interactions with technical staff in different organizations.

There was the ESF alternative study that was completed recently, eventually settling on Option 30, and then there was the other ESF, exploratory studies facilities activities, Title I design, Title II design, and a number of unpublished work to support other activities. And the additional thing that I don't have here and it's important to raise is the level of effort that it takes to do a high quality project; the checking of calculations that we perform, having an independent person take those calculations and review it, defining the work that we have to do in problem definition memos or design investigation memos, and have that description of work prior to actually doing the work be reviewed by a technical group, as well as the peer review process that we go through internal to our project, all of which are very time-consuming and all of which add to the quality of the work, but it's important to raise that issue.

I'd like to go through the second part of the presentation now fairly quickly. I think we might be able to make up a little bit of time here. I mentioned before that I've divided this talk into three areas; into planning, into performance, and into materials and studies associated with those three different areas. There are two that fall into the category of planning.

At the beginning of the project we had developed the sealing concepts, Sandia 83-1778, and I've talked about that a little bit this morning so I really won't go into, you know, all of this, but I will just re-emphasize the conclusion. We felt that effective sealing concepts are possible to meet performance criteria that we had developed, preliminary performance criteria. We also felt that the lower shafts and the drift floors were capable of dissipating the amount of water that we would expect to see in this particular geologic environment.

The second report was a program plan that we had prepared, and the basis of that program plan was developed around performance-related questions. There was a six-step approach that was proposed to answer these performance-related questions. These six items are listed here. The first is to assess the need for sealing; to define the design requirements; to measure the material properties; assess the performance of sealing design; perform laboratory analysis and field testing; and reassess the performance of those seal designs.

Now, where I think we are right now is we've basically touched on the first four steps here. That's not to say we won't go back and look at additional, or develop or refine new additional design requirements, measure material properties, but I view number five and six as being more of the very high quality level field testing, laboratory analysis, assessment of sealing designs that were required for the license application when we get to that point.

The second set of topical reports involved performance. The first of those was the hydrologic calculations to support sealing concepts. We had two reports that we had worked on. There were two different numerical analyses, analytical groups that were used to model basically the same problem to see if we would come up with the the similar results, and in fact, they did; one at Sandia, and one at Pacific Northwest Laboratory, and the results from both of those works are included in these reports indicated on the view graph.

The approach was to look at flow in partially saturated porous media, and we, as I mentioned earlier, looked at the flow past the waste package and flow to the shaft. Again, the conclusions: backfilling the shafts and drifts does not significantly influence the flow past the waste package. In fact, it was only a matter of a reduction of about 10 per cent of the actual matrix flow reduction that we were able to achieve by using a sand backfill as opposed to a clay backfill. The second bullet is: if the backfilling is required for other reasons, a coarse-grained material is more desirable. Again, we considered only matrix flow at that point.

DR. CANTLON: Before you take that off; Cantlon.

These now are all based on modeling. How much of this has an experimental underpinning?

MR. FERNANDEZ: Well, the actual values that were used, or the material properties that were used in the analysis were based on laboratory results that were performed by PNL, Pacific Northwest Laboratory.

DR. CANTLON: But there's been no test in G-Tunnel or elsewhere to look at an actual model, an actual experiment in tuff to test these conclusions?

MR. FERNANDEZ: Well, in the sealing program there has been no field testing, that's correct.

A basis for many of the performance-related calculations that were performed in the sealing program started with the development of the modified permeability zone model. This model will be described by John Case in one of the subsequent presentations here, but I just very briefly wanted to indicate what the approach and conclusions were.

The objective was to develop a modified permeability zone around a shaft resulting from stress redistribution and blast damage. The approach was to calculate the stresses around the shaft, establish the relationship between the stress and the fracture permeability--some information that actually was obtained in the field by Roger Zimmerman, and some of the laboratory work that was done by people at Sandia and Pacific Northwest Laboratory--calculate the rock mass permeability, and estimate what the permeability changes would be due to blasting.

The conclusion was that we were able to develop an equivalent permeability of the modified permeability zone, averaged over one annulus out from the excavation face itself, such that the disturbed rock mass was 15 to 80 times the undisturbed or the original conditions of the rock mass. Now, this is a very important conclusion that was used for densely welded, highly-fractured tuff, and that's what this model was developed for.

DR. DEERE: The previous one stayed at--Don Deere here. It was a matrix only under performance, flow through partially saturated porous media, then you have the conclusions. Then the next page was a modified permeability zone with the shaft stress redistribution and blast damage. Now, was that of a uniform matrix permeability without fracture permeability, or was it with--

MR. FERNANDEZ: No, that considered fracture permeability as well. The reason for coming up with this model was to actually look at the bulk rock performance, okay, so it included matrix and fractures, and predominantly, as John will talk about, it included the fractures and what will be the effect of excavating in a densely welded tuff on the fractures.

DR. DEERE: But it's not a follow-on, then?

MR. FERNANDEZ: No, it is not a follow-on. These are discrete reports that are done, you know, over a period of time. Okay, the first one was done at the very beginning of the--the one that I just showed up here, that was done in order to support the sealing concepts. This report came out several years later, and it probably would have helped to have a time diagram to see where these all fit in.

DR. DEERE: Well, no, I can see it, because Sandia 84, where you used the matrix flow, and then Sandia 86, we're dealing with fracture flow.

MR. FERNANDEZ: The numbers don't always correspond to the years, however.

DR. DEERE: I know how that goes.

MR. FERNANDEZ: The modified permeability zone was used very extensively in the technical basis report, and John and I will both be talking about the results that are included in the technical basis report in subsequent presentations.

The approach was to evaluate the need for sealing. This is a difference between design groups. Our approach was to say: How well do we need to design the seals to? There are other groups--some of the European communities--where they try to seal the best way they can, okay, so there is a distinct difference here. Our purpose was to come up with design requirements. How well do we need to seal? And that was one of the primary aspects of this report.

We had proposed performance goals. We had computed the design requirements, and we had postulated scenarios in order to give us a better feel for how well we should design these seals, based on what we knew about the site at that time.

Conclusions were for anticipated conditions, sealing is not required to meet hydrologic performance objectives. The second bullet: The nominal sealing of shafts and ramps is required to meet airborne and human intrusion objectives. Third point: More comprehensive sealing is proposed to deal with highly improbable scenarios, and you'll see more of this when John and I discuss the results of the technical basis group report.

DR. DEERE: A question again; Don Deere.

I have to take a look at the report again. Sandia 84-1895, now are these conclusions based on a fracture model or not? Because you're going back again--you went from '84 to '86 and now we're back to an '84 report.

MR. FERNANDEZ: Well, again, I have to stress the numbers, you shouldn't follow the numbers, you know. They were assigned at a particular point in time. It's just internal to the Sandia system at that time, we were required to get numbers on reports when we first started to do the work, okay?

DR. DEERE: Yeah, but the first report--

MR. FERNANDEZ: I think maybe I should have had on here, to avoid any confusion, is to say what the publication date was, and I think that would have helped a lot, but this was based on a fractured model.

DR. DEERE: Yeah, because conclusions for anticipated conditions, sealing is not required to meet hydrologic performance objectives, that's a pretty broad sweeping statement, and really--anyway, that statement does take into account the fractured welded tuff?

MR. FERNANDEZ: That is correct.

DR. DEERE: Okay.

MR. FERNANDEZ: We were also requested by the Yucca Mountain Project Office to evaluate the performance of the exploratory shafts, the old location for the exploratory shafts that were shown in the SCP-CDR. This particular report, Exploratory Shaft Performance Analysis Report, published, I believe, in 1989--even though it has a date of '85 on it--the approach was to evaluate what the influence of exploratory shafts would be on performance.

We looked at water flow, three different scenarios. We looked at air flow, two different scenarios; convective and barometric air flow. We looked at chemical interaction between the liner and the groundwater was evaluated. In this particular instance, we were concerned with the possibility of water dribbling down a shaft, the shaft liner, dissolving some of the minerals from the liner itself and then precipitating out at the bottom of the shaft liner and potentially clogging up the fractures at the base of the exploratory shaft. So our concept was to try to maintain good drainage at the base of the shafts, or adequate drainage, whatever that would be determined. That was another element of the study. Remediation efforts were also proposed in the study. We also looked at the ability to remove the liner and different techniques associated with that.

The conclusion for the study was that the design and construction of exploratory shafts are not expected to significantly influence the performance of the repository. I might also add that even though the exploratory shafts, the current design, current thinking is not the same as the SCP-CDR thinking, or how we evolved in this project. A lot of these studies are still very appropriate for a location for new shafts, and a lot of the thinking is still very appropriate.

In the third and final category, we looked at materials. One of the first evaluations that we had performed was to look at the contact between large concrete pours and grout pours at the Nevada test site--in particular in G-Tunnel--and to look at the quality of the concrete in the rock at that interface, okay, for concrete plugs and grout plugs that were very old--on the order of 17-years-old --and to see if there was anything we could learn from the quality of that material as it had resided in a tuffaceous environment.

We measured the standard mechanical and hydrologic properties of concrete, and we also observed the quality of the concrete and the grout pours. The conclusions is that typically, the mechanical properties of the materials exceeded the design specifications, although it was very difficult to define, in some cases, what those design specifications were. But when we were able to tie the two together, they were able in these very old pours and some of the more recent ones, able to achieve the original design specifications.

The hydraulic conductivity of the concrete and the grout was very low. There was no apparent degradation of the concrete at the interface or in the rock mass, and in some cases, we observed some poor interface bonding. I might also add, this doesn't reflect--one other thing that we found out, between the subsequent lifts of concrete, we did observe some segregation of the aggregate in the concrete, which is something else which we found to be a very good qualitative piece of information.

We had looked at several different areas. This is the portal for G-Tunnel. We had looked at one of the main seals, the 75 psi gas seal door at the entry to the facility, and several other locations; a total of five locations that we looked at in G-Tunnel, and cored numerous cores in those areas through the interface into the tuffaceous rock.

Here's just one slide to show the bond. This is the concrete here and this is the tuff over here. You can see that due to the operation or the actual drilling of the tunnel, there was quite a bit of fracturing near the wall, as what you would expect. In this case here, the bond was very good, so I just want to illustrate that even though we did observe some poor bonding at the interface, there was many other areas in which we did have good, high quality bonds between the two materials.

We did an ancient concrete study, so when I said, you know, starting with our concepts back in--

DR. CANTLON: Go back to that slide, the colored slide again. What's the nature of that sealant that's so dark in color there?

MR. FERNANDEZ: Which material? I'm sorry. What's the nature?

DR. CANTLON: Yes. Is that cement-base concrete?

MR. FERNANDEZ: This is concrete here, that's correct.

DR. DEERE: Yeah, but it doesn't look like concrete.

DR. CANTLON: Why is it black?

MR. FERNANDEZ: It's probably because of the photograph itself, probably didn't have enough illumination. I really don't know. There are a lot of additives they use in the concretes at the Nevada test site, and I would have to--probably to answer your question realistically, I'd have to go back and find out what the mixture was on this, and it could have been due to the aggregate.

DR. DEERE: It may have been a fly ash in it, even.

MR. FERNANDEZ: We did have an ancient concrete study and while we really didn't go back in our sealing concept design back to the 1800's, we did actually go back in the ancient concrete study to evaluate some concretes that were very old. This work was initiated under the old ONWI, Office of Nuclear Waste Isolation, and we picked up the work at the end of it and basically, Penn State was doing the characterization of that material, together with Los Alamos National Laboratories.

What they did was to extract some mortars, plasters, and concretes that had tuffaceous material included as a coarse aggregate, exposed to surface environments for 2,000 years from different areas; these areas being Rome, Ostia, and Cosa. A basic material characterization was performed and the conclusions were that the changes in the cementitious matrix and the matrix aggregate were observed, but the change of these materials seemed to have appeared to be very slow over time, but there were alterations to the concrete. It also was shown that the 2,000-year-old cementitious materials were actually quite durable. This had more application if we were to place large concrete structures in a surface environment and I just wanted to point that difference out, the motivation for doing that work originally.

A tuff concrete study was also initiated. The one question that was asked at the beginning of the program back in 1982 was, we're going to have a lot of material that we're going to be extracting from the repository. The thought back then was similar, at least in many peoples' minds, to placing large cementitious seals in any repository, whether it be in the saturated or unsaturated zone. We had a lot of material to get back into the repository, so the obvious question was, what will be the characteristics of this tuff concrete; a concrete that would utilize the tuff aggregate as the aggregate in the concrete.

We formulated the mixtures using this aggregate. We measured some basic properties, the physical properties of the concrete, and measured the crushing and grading characteristics. The conclusions were the mechanical properties, expansion behavior, and processing was good. The crushing of the densely welded tuff that we used was very splintery, but nonetheless, the mechanical properties were good. We were able to achieve a 10,000 psi concrete, for example; very high quality concrete, very low permeability, on the order of a microdarcy range, so very low permeability material.

The absorption of the water in the tuff was very high. It was on the order of 10 per cent. It's something that you would need to know if you were to design a high quality concrete mixture, to know how much water will actually be sucked up into the material that you're using. So for design purposes, we needed to know what that absorption was.

We observed that there is a potential for reactivity, and that reactivity between the alkali-silica reaction that normally would occur in a concrete and create some problems later, we were concerned by that but had done no geochemical evaluation in this particular study to evaluate what that impact would be. And finally, we felt the welded tuff could be used as a coarse aggregate in concrete.

DR. DEERE: Question; Don Deere.

That's a pretty sweeping recommendation or conclusion if you haven't evaluated the potential reactivity, because that is the one thing that would throw it out and you would imagine that your welded tuff could have a great potential for reactivity.

MR. FERNANDEZ: Well, because we recognized that there might be some highly reactive silica in the concrete, perhaps one of the conclusions in this slide--although I think it's included in one of the other slides from a geochemistry standpoint--is we need to selectively--if, in fact, we're going to use this as a material, we need to very carefully evaluate the quality of the material that we're using. It was just an observation. It's an observation that's been a concern for people involved with concrete design for a long period of time.

DR. DEERE: There's a recent--a better understanding, I would say, in the last five to maybe eight years of very, very slow reactions that can take place on aggregates that are considered to pass all tests; in fact, they have passed all the tests, the existing tests that are specified by ASTM or Corps of Engineers or the Canadian groups, and yet, after ten to twenty years, they're starting to expand and forming lots of reactive gels and deterioration. So I think we'd have to be awfully careful with that "welded tuff can be used as a coarse aggregate in concrete," until you evaluate potential reactivity.

MR. FERNANDEZ: That's true, and we have done some geochemistry studies, work that has been done in the past, and it has shown that that could be a problem. I concur.

One has to be a little bit careful, and one has to come up with a good degradation model, and Tom Hinkebein will address some aspects of geochemical considerations, which is what we're talking about here.

DR. DEERE: Thank you.

MR. FERNANDEZ: As far as the tuff concrete, Penn State was tasked to look at the quality of this material from a geochemistry standpoint. Again, one of the primary directions in the program is to basically try to understand the physical system that we're dealing with, whether that physical system be the mechanical properties or the geochemical properties. What are we dealing with? What's the quality of the material?

They had looked at the tuff concrete that was developed at Waterways Experiment Station, and they looked at the reactivity of the tuff concrete, the inclusions that were in the concrete using the standard type of testing that was done at that time and still is used, actually, looking at crushing the samples and using intact samples, doing static experiments, elevated temperatures, elevated pressures, and also agitated experiments, trying to accelerate reactions in the concrete material.

The conclusions were that the alteration of the glassy component in tuff is observed, something that we had expected. It wasn't a surprise. The concrete disc appeared sound microscopically. Obviously, through all the electron microscopy work that they had performed, there was other things that they had observed. They also recognized that an understanding of the relationship between the alteration of the material and the physical properties, mechanical properties, properties of concern for a sealing material were very important. This spun off into the development, eventually, of the degradation model that Tom Hinkebein will discuss a little bit later.

DR. DEERE: If I may again; Don Deere.

Just to elaborate a little on this reactivity, the ASTM and most of the tests are accelerated tests, so they will test them in an environment, and usually it will run for one year to two years. They will make a bar out of it and then measure the amount of expansion, but they accelerate it under higher temperatures and then their experience has shown that--maybe with a factor of four or a factor of ten--they can predict the prototype behavior of a large concrete.

Well, where they've got into trouble is where it's one-to-one, where they did the accelerated test and it looked like it was going to make the norms because the prototype was going to be only one-fourth or one-tenth of that behavior, when in reality, it really swelled it exactly the same rate as the accelerated high temperature test in the laboratory, and that's what threw everybody off, and there are a number of projects around the world that are very severely impacted by this, where they have actually had to lower the reservoirs and--but most of these have been because of a small amount of silica that was able to get into the concrete.

MR. FERNANDEZ: There's always been the concern about how well these laboratory experiments, you know, accelerated reactions really model the actual behavior, particularly in the long term. There's no doubt that there are problems with these type of reactions, but there's also the concern in the community about some of these tests and how well they actually model the performance of concrete, or just the change in mineralogy of the concrete with time. It's a technique that certainly is used and it has some validity to it, but I just wanted to just make note that there's a lot of people who--there's still a large uncertainty, I think, as to how well we're modeling the kinetics or how well these things actually can model things in actual time, as opposed to accelerated time.

DR. DEERE: And it appears that it's a process that can be slowed down or almost eliminated if you have the right low alkali-type cement or have additives such as fly ash. So it's not something that you necessarily have to live with. If you think you have a problem, you can do a lot of things that will really ameliorate that problem.

MR. FERNANDEZ: Well, I just also wanted to point out, at this phase in the sealing program, we had a lot of historical involvement with cementitious materials. In the technical basis report, one of the previous slides that I presented, I failed to mention that we had gone through a material screening process and it was our intention to get away from using these man-made materials. I did mention it in the first talk, and I think there is good reason for it because if we use these man-made materials, we can get away from a lot of these discussions that we're having right now and they're a lot easier to validate the performance, I think, of the sealing components if we use those natural materials versus the man-made materials.

This was a mechanical evaluation associated with, again, cementitious materials in the early phase of the program back in 1982-1983 time frame. The work was started under the ONWI program and it was transferred over to us, and basically, that's what the Phase I, the short term study referred to. The Phase II was through our evaluations, we looked at the quality of eight of the grout and mortar mixtures, half of which were expansive, half of which were non-expansive, and said we're looking for, in general, materials that have certain properties; low permeability, high compressive strength, low porosity. We were interested in those basic properties.

And from looking at all the properties that were obtained in the short phase, the Phase I study, we started the Phase II study, looking at two materials; a material called 82-22 and 82-30, one of which was a mortar and one of which was a grout.

The basis for the original mixtures of these materials was to get away from the calcium-rich typical Portland base cement and to get into more of a silica-rich type of cement/mortar. The purpose for doing that was to try to match the bulk chemistry of the rock that we would potentially place these cementitious materials in, so we added, deliberately, reactive silica that would--in the form of silica fume, silica flour, and we had subsequent problems with some of that material--and I think Tom will address that in more detail later.

It did seem that we can control the material properties a little bit better, and we did select certain types of materials. Now, there is a Phase, if you will, a Phase II-A or II-B, and what that looked at was one other material called an 84-12 material, and that material, the intention for modifying that material was to reduce the sulfate content in the cementitious material. It was thought that the release of sulfate could be a complexing agent which would enhance the release of radionuclides, so we wanted to lower the sulfate content of the material by using a Class H cement, which had a lower sulfate content, rather than the

Type K shrinkage-compensating cement which was used in one of the previous mixtures.

This was a companion report to the previous one. We wanted to assess the geochemistry reactivity of the cementitious materials. Again, we wanted to model this material close to the bulk chemical composition of the Topopah Spring member; high silica content. We did the same type of experiment; static, agitated, and simulated vapor experiments. We felt that cementitious materials could be developed to be compositionally similar to the tuff.

The last slide that I have here deals with some work that was done on rockfill material, some crushed tuff consolidation. Consolidation may not exactly be the correct term here, but what it was was basically to take a look at a rockfill with different gradations, subject that rockfill to a pressure, and then look at the hydrologic properties of that rockfill after it was compressed to a certain state, predetermined state. We looked at seven different mixtures and looked at multiple hydraulic conductivities.

The conclusions from that is we had some problems in doing that. We had a large testing apparatus, but we also noticed that for many of the samples, that piping had occurred and we were concerned by that. The results were sometimes contradictory between the types of hydrologic tests that we were doing, whether it be a falling head test or whether it be a constant head test, so we had some concerns and we reached the conclusion that we really need to do additional developmental work in order to better understand what some of the problems were.

That, in a nutshell, kind of summarizes all of the major topical reports or activities that we've completed in the sealing program. There are many other reports that we've issued to support other groups, or just progress reports that we made in the sealing program and they're not included, obviously, here because of time. But we reached a conclusion after all these studies that we feel that cementitious materials are possible--if we understand a little bit more about the quality of these cementitious materials--they are possible in order to achieve, potentially achieve some functions as sealing components, but we also felt that it was prudent to minimize the use of cementitious materials in the sealing in the repository environment, which may include elevated temperatures, in order to alleviate some of the concerns associated with thermodynamic equilibrium, for example, and that concludes my presentation.

DR. ALLEN: Questions or comments?

DR. DEERE: Don Deere again.

I think I should point out that in one project that suffered from the potential problem with a siliceous aggregate, the solution was actually to go about 80 miles to a site and use a tuff, and the tuff was ground as a pollozonic material together with the clinker from the cement, so when they got through, they had a pollozonic cement, which is very good at reducing reaction from the sodium and calcium that's left over from the hydration of the cement. So I think there are available--and you obviously have looked at a number of these methods of using special cements, perhaps using additives to overcome any potential that there might be for aggregate alkali reaction.

DR. ALLEN: Bill Barnard?

DR. BARNARD: Have you conducted any tests where the temperatures have been above the boiling point of water?

MR. FERNANDEZ: Yes, we have. We've gone up to, I think--correct me if I'm wrong, Tom--up to 300°C.

DR. BARNARD: What sorts of differences do you see at the high temperatures?

MR. FERNANDEZ: Well, again, I should state I'm not a geochemist, and Tom will address those issues later, but I guess in a nutshell, the calcium silicate hydrate as the mineral class is what we're really looking for in these cementitious materials. We did observe that when we have elevated the temperatures, we went from a mineral--well, actually, went from ettringite, which is a mineral which created the expansion in some of the materials. That actually decomposed--I'm not sure if that's the correct word, but that would have formed--subsequent to the ettringite, it became unstable at roughly 90 to 110°C. There was the formation of another mineral which seemed to be quite stable up to, in proportions, up to 300°C, and that was tobermorite, but there was alteration at the very high temperatures in some cases to two other materials called gyrolite and truscottite, and what the implications of all those are was really the reason why we initiated the degradation level, and Tom has all of the minerals that he's considered in the cementitious phases, and what really occurs when you start mixing these materials together and start accelerating some of these reactions, and I think he'll give a more complete description of what the implications of the formation of those minerals would be for our sealing systems.

DR. BARNARD: I've got another question. Were you implying in the one slide on ancient concretes, that all 2,000-year-old cementitious material is durable?

MR. FERNANDEZ: No.

DR. BARNARD: Okay.

DR. DEERE: Only that that's still standing.

(Laughter.)

DR. DEERE: Don Deere again.

For those who will be working on this particular problem in the future, I would like to give the name of David Stark, who is with the PCA, Portland Concrete Association in

Chicago. He has developed a new test that, on these various projects that have had trouble, we've been able to go back, or he has been able to go back and look at the reactions and his test is a simple cell test with one normal sodium hydroxide in one side, and a osmotic layer made up of simply a mortar, just a wafer of cement, of concrete as the osmotic layer, and then they put the aggregate in and then get, in a question of about three weeks he can detect and separate potentially reactive from the non-reactive aggregates, and it's the fastest way that we've come up with to be able to get a handle on potentially bad aggregates, so it's something worthwhile to keep in mind. You may know about it, but it's developed in the last three or four years, so I would put it down.

DR. ALLEN: Max?

MR. BLANCHARD: Thanks, Clarence. Max Blanchard with the Department.

Joe's next talk is about hydrologic goals, and I think it might be worth a point to explain how the hydrology fits into the Yucca Mountain characterization program so that we correct some misunderstandings that perhaps have developed over the last year or two.

The hydrology program that the USGS has is to develop a general understanding of the characteristics of the saturated and the unsaturated zone at the mountain. It's not related to radionuclide release, groundwater travel time, or retardation of radionuclides. The hydrology program at Lawrence Livermore, the goal is to understand the hydrology of the near-field around the waste package and link the degradation of the waste package and the rock and the water and those interactions in temperature changes as they occur with time. Livermore does that work.

Los Alamos does the hydrology of the far field and the radionuclide retardation mechanisms. Sandia links the hydrology to the seals and the hydrology to groundwater travel time calculations, and the hydrology used for long-term radionuclide releases to the accessible environment, which are the EPA limits.

And so, perhaps in earlier meetings as we were talking about characterizing the Yucca Mountain site, we've allowed the perception to develop that the hydrology program is basically that of the USGS, and please accept my apologies if it's turned out to be that way, because we have an applied hydrology program specific to each one of the disciplines, given the dilemma that they must confront themselves with in the use of hydrology to the waste package near field or the far field, or the groundwater travel time, or sealing, or whatever, and so perhaps in the future we'll have an opportunity during 1992 to try to more clearly show the associations with those.

The concept that we've had was that the general characteristics would be elucidated, discussed, and modeled by the USGS hydrologists, and that would be an information base for those who are doing more specific hydrologic studies, for them to draw from, but they're not limited to accept their models or their concepts; that we hope in the end all of these different hydrologic studies will come together with a coincident set of views with respect to the processes that are working at the site.

I don't know if this helps, but I wanted to try to point that out so that people understood that each of the disciplines--and in particular, Joe, in his need for developing a sealing program--had to link to hydrology, and he is burdened with determining how the hydrology and the sealing program links together and how he's going to describe how well it will work and act as a retardation or a sealing mechanism against potential radionuclides when they migrate, and he may draw on the USGS information, but he also may draw on a lot of his own information that he's obtained from laboratory studies or elsewhere, and so he doesn't have, at the outset, a preconceived input which is to accept the results from the characterization work, but to use what he can, it may or may not apply to his particular condition.

DR. DEERE: Okay, thank you very much, Max. Don Deere here.

I think this is an interesting topic to pursue perhaps later next year, to get together on this particular topic with the different laboratories to say, where are we at the present time?

DR. ALLEN: I think we better move right ahead. Joe, why don't you proceed?

MR. FERNANDEZ: I think we can probably reduce the time here a little bit. This presentation, I think, will only take about 45 minutes.

As Max had pointed out, the critical link in the sealing program, in the design of the sealing components is an understanding of the site. You'll see some speculation, engineering judgment presented in this next presentation. Actually, it's in many of the other presentations as well. Our strategy has been to try to postulate, try to estimate the water inflows into the underground facility using the best engineering judgment so that we could provide focus to how well the sealing components need to be. So we're kind of making a little bit of a leap here in some areas, just saying this is what we believe could be the case; either in anticipated conditions or unanticipated conditions, so I wanted to preface this presentation with that thought.

There are a number of design goals and design requirements that we have developed in the sealing program to give us some direction. They fall into these categories.

There are three hydrologic categories, and there's one airborne category. The subject of my presentation will be the first box; that is, the hydrologic design requirements for the majority of the sealing components. We have other requirements that we've also prepared and presented in the technical basis report, which is Sandia 84-1895, and all of this information, basically, is included in there and one other subsequent document, the exploratory shaft performance analysis report.

The second category, the hydrology in channels and sumps will not be discussed because of its brief nature in this presentation or any other presentations today. The hydrologic borehole design requirements will be addressed under the exploratory borehole presentations later today, and John Case will address the airborne design requirements associated with shafts, ramps, and drift fills, and there's no sense, really, in going through some of the rest of this slide here.

The presentation that I'll be making is structured as shown here. We have a performance allocation process that I'd briefly like to describe. I'll go into what our radionuclide release model was back six years ago when we tried to establish a basis for the sealing program. We compared how much water was allowed to contact the waste in order to meet the NRC criteria that I'll talk about in a minute, compared that with the actual amount of water that we would expect to see, the anticipated amount of water, and we also postulated what the unanticipated amount of water might be, considering some of the incredible scenarios.

In all fairness, I've added a section here which looks at a more realistic computation of flow into the underground facilities and into the shafts and ramps, primarily associated with fracture flow, flow into the shafts from that fracture flow, and flow in drifts itself and how well we can control that flow. And finally, I'll present the development of the design requirements.

For the first bullet, the performance allocation process, I've tried to come up with a quick slide that shows the entire process. It's a very complex concept to get across, and I thought it was worthwhile to go through in very simple terms and try to present that. We have the establishment of the performance goals; in our particular situation, how much water can we allow to pass the sealing components and subsequently contact the waste package? What is that volume of water? That represents the total goal here. So many cubic meters per year is allowed to contact or pass the sealing components and contact the waste packages. We have some associated anticipated flow that's represented by this line at the bottom of the first rectangle.

The second step is to allocate performance. We have a total goal here. Now, if we're trying to restrict the amount of water past the sealing components, are there different systems that we should consider? Well, we did consider two different systems; the shaft and ramp seal subsystem, and the underground facility seals subsystem, and the reason for doing this was really based on the criteria, the NRC criteria, 10 CFR 60, where they actually address seals as a separate category for shafts, and seals in the underground facility or the engineered barrier system as a separate system.

The third step is to establish the design goals. For each one of these goals associated with a particular subsystem, how can we further break that down for an individual seal component?

And then the fourth step is to say, using this design goal of so many cubic meters passing through this particular seal, what must the design requirements be in the form of hydraulic conductivity for this seal system, whether it be for the shafts or for the underground facility. It's depicted as the shaft here. I'll go through each one of these with a slide.

The first step, establish performance goals. We've developed a curve called a maximum allowable performance goal. That's represented by this upper curve in this diagram. This is the amount of water that we feel could contact the waste package and still comply with 10 CFR 60 criteria of one part in 10^5 of the 1,000-year inventory. I'll show a more detailed curve for that later and some other models that were used in order to develop this curve.

There's another curve that we're calling here the design basis performance goals curve. Is there logic that we should select another design basis curve for our seals? And we're calling that the design basis curve. We have the anticipated flow represented by this horizontal line, and we also have little spikes along this line as a function of time where we might get larger inflows into the underground facility or into the shafts and ramps.

The second step is shown in a very simple form out here. This is the allowable amount of water passing the sealing components and that could contact the waste. At some time after closure, we have a performance goal of 1,000 cubic meters, for example. We allocate or proportion that between two different systems; the shaft and the ramp system, and the underground seals system. So in this case, we've selected 900 m^3 and 100 m^3 to give a total of 1,000 m^3 for our overall performance goal.

At Time 2, we're allowed to have perhaps a little bit more water that can actually contact the waste package through bypassing the sealing components, so we have another allocation where we say so much can pass the underground seals subsystem, those seals that comprise that subsystem, and so much that can pass the shaft and ramp seals subsystems.

There comes a point where this number here, really, we don't need to allocate any more to that because we don't anticipate, even under an unanticipated situation of water flowing into, let's say, the underground facility, to have more water than that. It's the same as saying, you can have total seal failure and all of the water under an unanticipated situation that would enter into the underground facility can contact the waste and there would be no unacceptable release of radionuclides. So at that point, you can start allocating, as shown here for Time Period 3, let's say the allowable amount of water passing the sealing subsystems collectively would be 100,000 m^3. The split would be the amount under unanticipated case for the underground facility, and then the balance would be applied to the other subsystem. So it's just a matter of allocating performance and how much water can pass through the seals associated with one subsystem versus the seals associated with another subsystem.

DR. CANTLON: Before you change--Cantlon--that first column totals, those are rates, aren't they? 1,000 m^3.

MR. FERNANDEZ: These would be so many cubic meters per year.

DR. CANTLON: Per year, okay.

MR. FERNANDEZ: It's just, at this point, meant to be a schematic.

DR. CANTLON: I understand.

MR. FERNANDEZ: The third step was the establishment of design goals. This is just a very simple concept that we use. We have so much water that can enter and pass through seals collectively in one subsystem, the shaft and ramp subsystem versus another subsystem, the underground subsystem. This is strictly the number of components of a particular component in this subsystem. For example, this could be the anchor-to-bedrock seal. If we had, for example, ten anchor-to-bedrock seals, the design goal would be one-tenth such that this term here was equal to that. So we're just proportioning. We're saying if we can only have 1,000 m^3 of water in any one particular year passing through this portion of one of the subsystems, we're saying that only one-tenth of that--if we have ten anchor-to-bedrock seals--one-tenth of that can pass through that particular seal.

This other term here is a storage term that we added as a part of our development of these design requirements. It's a storage of water in the underground facility--or, excuse me--in either one of the facilities. For the underground facility, we basically said that will be zero. We don't have any place to store the water. In the shafts and ramps system, we felt because of the design of the underground facility, we would be able, if water did in fact enter either the shafts or the ramps, we would be able to store that water in the low point of the repository and drain that water so we'd have a storage capacity and, subsequently, a drainage capacity for that particular subsystem.

And that's the only difference that you see here between the allocation or the design goals for one system versus the other subsystem, is this storage term here.

I'll get into the next part of the presentation associated with the radionuclide release model. Approximately six years ago, we felt we needed a little bit closer tie with performance, so we made the decision to try to determine how much water can contact the waste package and still not exceed the 10 CFR 60 criteria of one part in 10^5. We also recognized at that time that we were not applying the EPA criteria because we felt it was very, very premature to look at radionuclide releases to the accessible environment and tie that to the criteria defined in 40 CFR 191, so we stopped it basically at the waste package itself in order to meet the 10 CFR 60 criteria.

We looked at two different models. One was to assume that all of the radionuclides were contained within the uranium oxide matrix. The other model was to look at perhaps something that was a little bit more realistic. It was thought that had developed a decade or so ago, a lot of work that was done by Lawrence Livermore Laboratories on the characterization of the waste package and the fuel rods, and other people in the field as well.

So we tried to encapsulate all that information and to say, can we make a slightly more realistic model to look at what the radionuclide release would be? This was our attempt to try to tie that to the sealing program. I should also point out that that's one very active area in the Yucca Mountain Project. It's not our intention in the sealing program to continue this work, but nevertheless, at that point in time we needed to focus the sealing program, and that's why we came up with this radionuclide release model, using results that were obtained from Lawrence Livermore Laboratories and other groups.

So we looked at a second model, and we recognized, or they recognized that there were differences in the matrix where the radionuclides were located, in the matrix, in the cladding and structural parts, in the gap, in the grain boundaries. We then looked at what type of mechanisms can we postulate, or can we use from these other laboratories to come up with some sort of a model. We incorporated the congruent dissolution concept in the first model, as well as corrosion of the zircaloy cladding, the rapid gas release that would be associated with some of the radionuclides such as ^{14}C, ^{129}I, and some of the cesium radionuclides, and also looked at preferential dissolution, particularly for ^{99}Tc.

We coupled these assumptions with a number of other assumptions, quite a few of which are described in the technical basis report, which looked at the failure of the waste package and failure rate. We also looked at the failure rate of the fuel rods themselves under different conditions; once the waste package was breached, and once it was intact.

We put this all together and came up with a cumulative release. What we did is looked at how much water can contact the waste package and still be within the 10 CFR 60 criteria. We did that for a number of radionuclides, for about 40 radionuclides. Particularly, there was eight that we were primarily concerned with from the second model, which had different release mechanisms. We've included those eight here, as well as the radionuclides that will be contained exclusively in the matrix itself. These represent, for each radionuclide, what the allowable amount of water would actually be that can contact the waste package and still not exceed those criteria. That's what all these curves are up here; ^{129}I, ^{90}Sr, ^{137}Cs. This was Model 2, the results for Model 2, everything above here.

There were actually many other curves, but I'm not illustrating them here just for simplicity. It shows several things. It shows that some radionuclides, such as Pu-238, actually could dominate our performance goals in a short period of time, whereas others would dominate it in a longer period of time, such as Pu-242, because of their half-lives. If a particular radionuclide had a very short half life, you would expect, if it was available to be dissolved or to be released early because of failure of the waste package, it would control the release of radionuclides and, in fact, this is what was observed.

So we have a series of curves that represent how much water can actually contact the waste package from Model 2. There was Model 1, which assumed all of the radionuclides were contained within the matrix. Now, the distinction to point out here is in Model 1, we assumed that all of the waste packages had failed at the end of 300 years after closure. On the second model, we assumed a systematic release or failure of the waste package and a systematic failure of the fuel rods contained within the waste package. We assumed that 1,300 years after closure, all of the waste packages would be failed, and then we would have some continuing failure of the fuel rods contained within those waste packages. So those were the basic assumptions that we used for Model 2 and Model 1.

It turned out after we did the analysis, that the more restrictive model was Model 1, where we assumed that all of the waste is available to be dissolved via water, and all of the waste packages are failed at 300 years after closure, and so, as you may recall, in the previous slide we saw a line that kind of came down here. It was called the maximum allowable performance goals, and then we had another curve called the design basis curve. Well, we figured for conservatism, we should select the lower curve, and that became our design basis for subsequent analysis.

The next phase of my presentation deals with the water flow into the repository. As I mentioned before, we wanted to couple how much water we could allow to enter through the sealing system, through the sealing components, and then migrate to the waste packages and all of that water contact the waste package. How much water are we really dealing with? And that was a very difficult part of our problem. Not having a lot of information on the site, we had to make many generalizing assumptions.

We considered two types of flow; anticipated flow and unanticipated flow. We assumed that we would have matrix flow over the entire repository area. We assumed that there would be annual, limited, and localized fracture flow that would occur at the repository horizon, as well as in the shafts. We also limited the surface flow into the shafts; in other words--well, I'll get into more of the assumptions associated with these three here.

Under the unanticipated case, we considered continuous fracture and matrix flow over the entire repository area, and we also considered extensive surface flow into the shafts from major flooding events.

For the flow into the shafts, the assumptions were as follows: For the anticipated case, we assumed there was no restriction of the flood waters near the shaft; in other words, we didn't have some sort of an embankment downgradient from the shaft location that would inhibit movement of water from a probable maximum precipitation event or some other flooding event to occur. There was freely flowing drainage past the shaft portal.

Again, the reason why we did this study is that the original design, the SCP-CDR design and the designs before that actually had considered placing those shafts in drainage areas, and so that's why we explicitly considered these scenarios. As far as the shaft fill, we assumed that we had a granular rockfill that had a 10^{-2} cm/s. We assumed the same thing for the unanticipated situation. We actually, in this case, this became our base case, but we looked at a broad range of rock properties as well as shaft fill properties that you'll see in a second here, and it'll also be presented in John Case's presentation for air flow.

There was no seals in the shafts in either case. By seals I mean no rigid bulkheads that were placed in the shafts. I still consider backfill to be a seal, however. It's a matter of semantics.

For the anticipated case, we assumed that the water supply was four thunderstorms, and we would have sheet flow that would occur over the shaft locations. Once the precipitation exceeded .5 inches for an event--some work that was done several years ago, in observations that were made by the USGS and the Bureau of Reclamation indicated that this was probably a little bit low. They noticed sheet flow when the precipitation was actually over an inch or two. So this is still a conservative assumption at this point, and the four thunderstorms was based on some meteorological work that was done by Sandia by--well, I can't remember his name, but it was work that basically had looked at--oh, Tom Englington. It was work that had looked at the precipitation that had occurred at the test site over periods of time, and that's what the basis for this four thunderstorms became.

The water supply for the unanticipated was probable maximum flood, and also, the 500-year flood occurring at each one of the shaft locations, so there was four shafts at the time of the SCP-CDR.

The final assumption was that the duration of the thunderstorm would be one hour. Sheet flow lasts for one hour over the shafts and the faults; whereas, in the unanticipated case there was no restriction of water flow into the shafts. If there was water there--as you'll see in the models that we used--it was allowed to drain into the shafts for a period of 100 years in order to get the quantity per each year.

These were the two models that were used for flow into shafts for unanticipated conditions. This one here assumed four different types of flow. It assumed alluvial flow, basically flow that would occur downgradient from the wash--and this is the alluvium, this stippled area here. That was the first flow. It also allowed vertical flow into the Tiva Canyon bedrock. It allowed Dupuit flow, and this was assumed to be fully saturated, this alluvium, so we had flow occurring into the shaft, and we also had a fourth type of flow, and that was the flow that actually would be contained, would actually flow down into the shaft itself and into the modified permeability zone, the assumptions or the model that we had created in one case to support this study.

The second model we used, it was a theory that was presented by some other hydrologists in another publication which looked at the zone of capture. Any of the water basically coming down this particular alluvial area, would that basically be captured by the shaft? And all the water outside flowing in the alluvium would not be captured by the shaft and would be able to continue down flow.

The reason for doing this, again, was to have two different techniques to see if we would basically come up with the same results. When we did that, the results were quite comparable.

The results are shown here. For the anticipated case, we looked at water flowing over the top of the shaft that was filled with a basic rockfill material and had 10^{-2} cm/s saturated hydraulic conductivity. We used the Green and Ampt Solution to model the amount of water that actually could enter into the upper portion of the shaft, and we also assumed that basically it would be able to go to the bottom of the shaft and subsequently contact the waste. So we basically said it could enter into the top of the shaft and all that water can contact the waste, so that was the assumption, the implicit assumption that was made in both of these situations.

The results from that is that for different shaft sizes--we had anywhere from 13.4 m^3 per year to 100 m^3 per year entering into the upper portion of the shaft that could potentially contact the waste package, assuming no bleed-off into the rock itself as it made its way to the waste packages. The total for the four shafts was on the order of 270 m^3/yr.

The unanticipated case, again, we looked at the model considering these four types of flows. We assumed that the Tiva Canyon saturated hydraulic conductivity, bulk rock conductivity would vary because we really don't know. We don't know at different locations what it would be, so we said, well, let's just vary it by three orders of magnitude anywhere from 10^{-5} and 10^{-2}; assumptions taken from work that was developed by the U.S. Geological Survey and Scott Sinnock and others.

We assumed that the saturated conductivity for the alluvium varied over seven orders of magnitude; 10^{-5} to 10^{2} cm/s, and we assumed two modified permeability zone models, one at the upper portion of the shaft and one at the lower portion of the shaft.

The results from that is that, as you might anticipate, for the probable maximum flood, a situation where all of the water is either Dupuit flow, alluvial flow, Tiva Canyon flow, or the MPZ and shaft flow, this MPZ and shaft flow is the amount of water that entered into the shaft that was computed, and that ranged anywhere from 200 m^3 to 83,700 m^3 for a probable maximum flood occurring at each one of the shaft locations and the impedance of the water from the floods would occur because of some sort of a landslide that would occur across or downgradient from the shaft locations.

The flow into the underground facility is shown here. We have anticipated and unanticipated case, upper and lower. The approach was to look at matrix flow. We coupled this particular--the work we had done before with this evaluation. We found out for the work that was done by PNL and Sandia that the flux through a drift per depth of drift of one meter was on the order of 1.3 x 10^{-12} m^3/s. If we were to assume for the entire repository--we used, I think, a number of 100 miles of drift for emplacement drifts--all the water were to go in there, it would come only to 5 m^3/yr, just to give you an idea; fairly small. For sand, it was even lower. It was .1 m^3/yr. So the amount of water that would potentially contact the waste package via this mechanism was considerably lower than the other mechanisms that we had evaluated through those analyses that we had performed.

For fracture flow, we assumed, again, that the Green and Ampt Solution was applied where we would have water that would pass over a fault zone. In particular, we were looking at the Ghost Dance Fault. We looked at the surface expression of where the Ghost Dance was relative to the location of the emplacement drifts at the repository horizon. We had X number of drifts--I think it was 24 emplacement drifts--that were intercepted by the Ghost Dance Fault, and then we also looked at the supply of water at the surface.

The Ghost Dance Fault had crossed nine drainage channels, so we figured that it was reasonable to assume that 50 per cent of these emplacement drifts would actually experience some sort of fracture flow at depth for a first cut, and this number here represents the maximum amount of water that we computed--considering the variations in the porosity of the fault zone, variations in the hydraulic conductivity of the fault zone, and variations in the saturation state.

Okay, so this was again a sensitivity study and this number represents the maximum amount of water that would occur under partially convergent flow for 12 emplacement drifts. Fifty per cent of the emplacement drifts were assumed to have some sort of a water inflow due to some sort of precipitation event; water subsequently going over the fault zone, being communicated down through the fault zone to the emplacement drifts.

We also assumed that there would be two water-producing fault zones in each ramp, and that was, again, the original design. The result of this is not shown here because it was not considered part of the underground facility, but because of the nature of the mechanism that we were looking at, this number came out to be 21 m^3/yr, still very low.

For the unanticipated flow, we worked really hard at trying to come up with a logical model, and we really felt that there was no logical model that was believable by a "large" number of people, so we decided arbitrarily, frankly, to assume that freely draining water that could enter into the underground facility, whether it be by fracture or matrix flow, was assumed to be one millimeter per year infiltration. We assumed that all of that infiltration that passed a cross-sectional area of the total floor and total ramp areas would be available to contact the waste packages. That number came out to 5,600 m^3/yr.

As I mentioned, when we did this study, we recognized that some of these values were incredibly high and we wanted to get a more realistic estimate of what the water would be because we are in the unsaturated zone, so we did several analyses in the area of trying to refine what that inflow would be.

We looked at several different scenarios. This is the location for the exploratory shafts, the old location for the exploratory shafts. They used to be down over here. This is Coyote Wash, which is the northern drainage area shown here. This is the G-4 pad for reference. These were the "final" locations for ES-1 and ES-2.

We looked at three different scenarios. We looked at rainfall scenario, sheetflow scenario, and channel flow scenario. This shaded area represents the sheetflow, the area over which sheetflow would actually accumulate and go over the exploratory shaft's pads.

The conditions considered were two probable maximum floods. We considered a general storm, which was a storm that was postulated, I believe, by the Bureau of Reclamation. They had assumed that this storm would be 14 hours in duration and it would have a total precipitation of six inches. There was a second storm, which was actually a more severe storm. It was a storm that had lasted for six hours, but it was 14 inches of precipitation would fall over these areas. So those were the two probable maximum storms that we considered.

We also looked at varying rock properties, you know, over this area here because we were trying to compute the actual flow that would occur through the fractures--as you'll see in a second--into the shafts themselves. We looked at the varying saturation states for the Tiva Canyon member, as well as different porosity states. We looked at the average properties which are contained in the reference information base for the project, which gave a saturation state of 67 per cent, a porosity of 11 per cent.

We wanted to maximize the amount of water that would be included into the fracture or imbibed into the fracture, so we selected a low saturation state and a high porosity, and we also wanted to minimize the imbibition into the rock matrix as the water had percolated down into the fracture, so we looked at another set of conditions here.

This is the model that was developed by Tom Hinkebein, and it basically shows what we tried to do. Here we have water. Here's the exploratory shaft, and water entering into a fracture system over here. We assumed an average fracture. Then we looked at that particular fracture, and we had so much water coming into the fracture at this point; some of it being imbibed into the matrix, the balance of which would go down into the next element, some of which would be, again, imbibed into the matrix going down further.

At some point, all of the water is imbibed into the matrix, so it has a certain limitation over which a fracture --for example, here, the water might migrate down this far, but then there would be no more water that would actually be imbibed into the matrix because there's no more supply of water. So what we were trying to do is determine what this "r" distance would be. What would be the maximum distance in which water from these scenarios would actually be able to enter into the fracture and make it to the shafts and subsequently, potentially, down into the lower portion of the shafts.

What this shows is varying zones of influence for different rock properties. Remember, I mentioned the three before. These are what the concentric circles represent. Here's the maximum extent of water entering into the shaft, as shown here. It's interesting that that extent is pretty consistent with the outline for the exploratory shaft pad, and I'll mention why that's significant in a little bit here.

The other thing is, here we have the general storm, and this is the flood width of the storm shown here. The second line represents the PMF water, plus the debris flow, which was assumed to be 50 per cent. It was a number that I had obtained from Pat Clancy of the U.S. Geological Survey. So this was the maximum extent of water flow in the channel, and what we had tried to look at is if water entered into a bare fracture network here, as it did here, how far would that water migrate down? So these two lines, the north line and the south line, became the zone of influence for that probable maximum flood.

It's interesting to note that that probable maximum flood in this instance, looking at the geometric relationship between the current drainage channel and the shafts, that this zone of influence was actually never--the water never did reach the shafts, and the water that did enter, theoretically, into the shafts was the water that would drain over the exploratory shaft pad within these concentric circles here for the rock conditions evaluated.

The reason why this is significant is because, as I mentioned earlier, we went from one design, we went from having a large concrete structure at the surface of the shafts to a capillary barrier theory. If we were to restore this whole area, we would have a series of, or layering of materials that basically would be able to minimize these circles here, because in our analysis, or in Tom Hinkebein's analysis, it assumed that all of the water passing over the pad was in direct contact with the fracture. There was no buffering capacity associated with some sort of a layered material, like an alluvium. So having a capillary barrier theory or a layered system actually, I believe, can reduce that zone of influence.

We looked at, also, the drift flow. How can we actually restrict the lateral migration of water in a drift? There are some recent calculations that were done by Sandia and GRAM, Incorporated, and they'll be presented here. We had a constant influx of .3 gpm occurring in a drift, which is indicated by these two lines sloping at a 6 per cent grade. And then we have the TSW2 Topopah Spring member above and below. We had an infiltration rate of .1 mm/yr.

The question that was asked that we tried to answer is: how far does this water go in before it's actually imbibed into the rock below? What is the distance from this source point to some point in which we would expect no more lateral diversion of water?

Well, the answer to that was for this material here, which is a glacial outwash--and what I need to do is perhaps show this slide first. The question that we asked ourselves is: what would be a reasonable material to have as a rockfill? So what we did is we looked at several different crushed rock curves.

The first set here, 1 and 2, was derived from some work that Sam Wong and I had done at the Waterways Experiment Station, crushing some partially welded to densely welded tuff, and we came up with a gradation curve in a jaw crusher, and that's what this three-inch opening and the 1.5 inch opening represents.

We also looked at some of the recent work that was done by Colorado School of Mines, looking at the characterization of what would be the gradation of two different types of disk and pick cutters going over a welded material, and that's what Curves 3 and 4 represent; at least partial curves.

We also looked at a TBM, the results from a gradational analysis that was performed at the Nevada test site, Little Skull Mountain, in a bedded tuff. They had a very low compressive strength, approximately anywhere from 600 psi to about 1500 psi, as I recall, and that's what these two curves represent here.

We then tried to categorize these materials. We said, well, basically, using the soil classification system, these would fall under the gravelly, well-graded materials with few fines. And using that system and doing the proper analysis for the coefficient of uniformity, et cetera, we basically categorized these in that one category.

Then we went to two categories for soils, one of which was prepared by Desert Research Institute of Reno, Nevada, and another one by Mualem's Catalogue of Soils back in 1976. We looked through those soil catalogues to say, is there a soil or is there a material that would be comparable to these materials here? We found two materials, one of which was the glacial outwash. It had a very high conductivity. It had a saturated conductivity of .1 cm/s.

Then we looked at another one, which was the gravelly sand material, and that had a saturated hydraulic conductivity of 10^{-4} cm/s, as I recall, and those were used in the analysis. So let me very quickly go back to this figure that I showed before.

The glacial outwash was one of the materials that we used. It isn't truly glacial outwash. It's the analog that we used in our model and that had, again, a saturated conductivity of .1 cm/s. The maximum extent, lateral extent of water flowing into this drift, imbibed into the matrix underneath, was on the order of 220 meters. That was consistent with some back-of-the-envelope calculations that we had performed. Now, this rock was assumed to be the bulk rock saturated conductivity of 10^{-5} cm/s, which was an important parameter in this study.

Then we asked another question. We asked ourselves, fine, this water goes now 220 meters under those model conditions. Can we somehow restrict the lateral extent of that water? Perhaps, would a simple material, two materials in a drift, could they achieve something very effective? Well, in fact, they did.

We looked at two different bulk rock saturated hydraulic conductivities, 10^{-5} m/s or 10^{-3} cm/s. Here's our glacial outwash material over on the right-hand side. This line here represents the break between the glacial outwash material and the gravelly sand material, which had, roughly, a 10^{-4} cm/s, still, both of them being very easily achievable materials as far as conductivities.

We looked at the phreatic surface here. We assumed that this whole modeled area which goes above and beyond this drift--and that's all you see here right now, is the drift--we fully saturated the whole model and then allowed it to de-saturate until we reached steady state conditions. Okay, it was just a simpler way, numerically, of doing it, and what we show here was that the maximum phreatic surface would go from the previous case--excuse me, this would be the comparable one.

For the previous case, for a bulk rock hydraulic conductivity of 10^{-5} cm/s, we found out that we can reduce the lateral extent down to 23 meters, from 220 meters. So this led us to reach a preliminary conclusion that just a simple contrast of materials in a drift can be very effective in reducing lateral migration and controlling water flow if, in fact, we have these large amounts of water.

I'd like to conclude the talk here by going through what we had learned in the refined computation of the water results and then conclude in the overall presentation.

The fracture flow into the shafts--remember when we were talking about the fracture flow models--the amount of water entering via the fractures into the shafts was shown to be anywhere from zero to 50 m^3/PMF. Remember, the waters we were talking about before were incredibly large. For the probable maximum flood, for damming across--assuming that the shaft itself was located in the drainage channel--there were values that ranged up to 83,700 m^3 for all four shafts; incredibly large numbers. And here, we're showing that for these analyses, the flow into the shafts was on the order of zero to 50 m^3.

Add to that the consideration of putting a capillary barrier on top of these locations, we can substantially, I think, reduce the amount of water that could enter potentially into the upper portion of the shaft.

Again, I wanted to point out, there was no buffering capacity associated with that analysis. The water was allowed to enter directly into the fracture matrix. It was a clean surface, basically; rock surface. The extent of the zone of influence is potentially limited even for a probable maximum flood.

Locating the shafts out of the alluvial areas is very effective in reducing the water flow into the shafts. A layered soil, capillary barrier, may be very effective in reducing the flow, and for the drift inflow, basically we reached the conclusion that drift inflow in the drifts can be controlled by the material change between those two materials that we had modeled, as I showed before.

I have two more view graphs here. Getting back to our original discussion, we had looked at the maximum allowable goals and anticipated conditions, the amount of water that we would expect. This was on the order of 64 m^3/yr, as I recall, for the underground facility. We looked at the anticipated conditions, which is represented by this horizontal line. We looked at the unanticipated conditions, climatic change, assuming 1 mm/yr represents the vertical spike ;, the unanticipated 500-year flood, represented by spike 3.

For 4 and 5, we looked at the unanticipated probable maximum flood, and as a worst case, looked at the unanticipated condition 2, which would be the climatic change, 1 mm/yr, and coupled that with the unanticipated condition of the probable maximum flood, the worst case.

Now, what has to be understood here is that we assume this to occur every year, the probable maximum flood to occur every year. It's not just a spike here in time. Our assumption was that we will design for that particular scenario that would occur every year.

In conclusion for the analysis, we felt that nominal sealing is only needed for the anticipated conditions. Sealing measures are proposed, however, to provide a greater assurance that performance goals can be met.

I actually had forgotten a few view graphs here. There was the last bullet here, which is really the most important, I suppose. What are our design requirements? It's just the bottom rectangle there.

It was important to discuss several concepts here. We came up with our performance goals that we mentioned. We had two curves--and I'll get back to that in a second. We have design life, design goals, and design requirements. Looking at the amount of water that we would see under an unanticipated condition, we had arrived at the fact that contrasting that with the allowable amount of water that can enter into the underground facility, that because the underground facility seals would be subjected to a much higher thermal load, that we would like to allocate all of our performance to allow as much water to pass those seals as possible first before we allocated anything to the shafts and the ramp seal components.

Remember, it was a little diagram that I showed earlier with the large volume of water allocated between the two subsystems. Using the analysis, we basically said we could allow water to pass through the underground facility seals from zero to 500 years after closure, but once we reached 500 years, the allocation was on the order of 5,600 m^3, as you may recall, per year.

We also had assumed that the maximum amount of water to enter into the underground facility was also that same amount. It just so happened to correspond to 500 years. That became our design life, so what we're looking at here is our design life for the underground seals, or for the seals associated with the underground facility was on the order of 500 years up to this point here. Beyond that point, you could have all the seals fail and it wouldn't make any difference, because all of the water in the unanticipated condition can pass the seals--whether the seal was good or not--and contact the waste, none of it being absorbed into the rock. So this time point here became 500 years when we looked at our radionuclide release model, and looking at the unanticipated amount of water that can enter into the underground facility.

We had a similar design life for our seals and ramps subsystem. This turned out to be a little bit higher, on the order of 84-90,000 m^3/yr. It was based on the unanticipated flows from water entering into the four shafts that I mentioned earlier.

As far as the design goals, you may recall I talked about the storage capacity. For the shaft and ramp systems, we assume that 10,000 m^3 would be able to enter or pass through the seals associated with that particular subsystem--the shafts and ramps subsystem--and be accumulated in the low point of the repository and drained at that location. So for the first 500 years, even though we weren't allocating any performance directly to the shaft and seal components, we were actually doing that. We were allowing water to pass through based on this storage capacity term. None of this water contacted the waste package in our analysis.

However, we also assumed that any water passing a seal in an underground facility could contact the waste package. It could pass through the seal component, not be absorbed into the rock, contact the waste package.

We had to establish a similar number of sealing components. For example, we had anchor-to-bedrock seals. We had six of those, for example. We had a repository station seal. There may have been nine of those. And we went through and, with the SCP-CDR design, figured out how many similar components there would be.

So what we did is we assumed that that single component for that single subsystem would be responsible for controlling the water passing that seal. In effect, if we were to have multiple seals in the shafts, we'd have a redundant seal system. We have the same designs for, or a design that would restrict so much water past an anchor-to-bedrock seal, but we're also assuming that we have a shaft seal, and we're also assuming that we have this repository station seal. So we have a redundant design system.

We came up with, finally, the design requirements. We established a flow model. This is the same figure that you saw in one of my earlier slides. We had a head of water, water passing through, in this case, the anchor-to-bedrock seal. We assumed fully saturated conditions for the entire life of the seal, totally an exaggerated assumption. We said, what are the basic hydrologic requirements for that particular seal component?

The next two slides summarize what those design requirements are. Getting back to our reference design, we talked about the anchor-to-bedrock plug seal. These were our design goals through the analysis that I mentioned. Coupling this design goal with the model that we assume of water passing through, a certain head of water up to the surface--on the order of 30 feet, as I recall--we came up with an effective hydraulic conductivity of 10^{-5} to 10^{-4} cm/s.

We did the same for the shaft. This one was a little bit different. We looked at the water potential, the water entry and also the airflow out, so on this particular slide we've combined a couple of thoughts, and John will talk a little bit more as to why that should have been 10^{-2}. As it turned out, our reference condition for water flow was also 10^{-2}, so the airflow and the water flow analysis matched up pretty well. And on the bottom one here, the water flow through the station, through either a repository station seal or a seal located in the lower portion of the shaft, our effective hydraulic conductivity was a little bit more restrictive. It was 10^{-6}, 10^{-5}, and that was because we assumed that there was a full column of water from here all the way to the surface, so certainly, a more severe condition.

Here we see the single embankment dam. We assumed that there were so many of these within the underground facility. Applying the design goals and coupling that with the simple models that we had used for flow, we basically had an effective hydraulic conductivity of 10^{-5} to 10^{-4} cm/s.

We looked at massive bulkheads. We looked at how many large inflows might we expect from the Ghost Dance Fault, and came up with a number of bulkheads and, in this case here, we had a fully saturated column of water all the way to the surface to provide the pressure head on either side of that seal, which would be intimately in contact with the drift. That effective hydraulic conductivity was fairly severe. It was on the order of 10^{-8} to 10^{-7}.

And finally, we assumed what happens if you have a bulkhead that perhaps settles or, by design, you decide that it's not really a pressure bulkhead, but just a bulkhead. And here we have the head of water only being the amount of water that basically would occur in this portion of the diagram, the upper portion of the lower seal down to some portion of the upper seal here, and that effective hydraulic conductivity was 10^{-5}, 10^{-4}.

What this illustrates is that there may be some preferred sealing components. If we apply these models, assume that we have fully saturated flow, it's better to have a seal, for example, further up in the shaft so we can avoid a high column of water. If we have to design pressure systems like this and if, in fact, water conditions exist, we have pretty severe conditions that we have to design for. I don't really anticipate we're going to have those, however. So we do have some preferred sealing components.

Now, this really is the conclusion here. We felt that our design goals are fairly conservative. We came up with, using Model 2, we came up with the maximum allowable performance goals, a realistic radionuclide release model. In the second model, we assumed that the matrix was fully exposed to the water. All the water that would pass the seals in the shafts and ramps or in the underground facility, all that water was allowed to contact the waste package. We also have here the same scenarios that I described before, one through five, as well as the anticipated conditions.

We feel that these designs also are conservative because we considered only one subsystem at a time. We didn't assume any benefit from the water being imbibed into the rock mass, for example. This one system, whether it be the shaft and ramp subsystem or the underground facility subsystem was responsible for doing the job, of restricting all of the water that got to the waste package. That's why we feel that's a very reasonable design goal.

We also reduced our performance goals. At some point, basically at 1300 years after closure, all of the waste packages failed in either model, and what we see here is a convergence and at this point here, we have no benefit of the waste package. We felt by reducing these goals, we were able to achieve a more conservative design basis upon which we used our design goals for this one here. You can see particularly for the early years, that is very conservative. All of the water contacts the waste, the point I've made several times in the presentation. None of it's imbibed into the rock and goes someplace else.

We assumed the models that we used to come up with our design requirements were fully saturated models--

certainly not the case. Unanticipated flow conditions became our design basis, not the anticipated flow conditions; again, assuming that that unanticipated flow occurred every year. And finally, the actual flow is lower than the design values that we used. That was shown in the refined computation of flows.

The design goals--I have to make this point very clearly--these design goals are iterative. We will evaluate these design goals, but as I mentioned at the beginning of my presentation, it's important to provide focus in the sealing program. That's why we took a bold step, if you will, in making some assumptions here, to say we felt, based on our engineering judgment, that we have to make some assumptions of water flow. We have to make some assumptions on the radionuclide release model, and they will, however, be evaluated through a total system performance analysis, and when we get our new baseline repository configuration, they'll also be reevaluated at that point in time, and that concludes my talk, particularly now that I'm losing my voice.

DR. ALLEN: Thank you, Joe.

Are there any questions from the Board? Questions more important than lunch, that is. From the staff? Any comments from the audience?

(No audible response.)

DR. ALLEN: So why don't we break for lunch, but let's have an hour and 15 minutes, so it'd put us about one-twenty or so for reconvening.

DR. BARNARD: Yeah. They have a pasta buffet out there out by the registration desk where everybody ate breakfast, so they should be able to accommodate all of us.

DR. ALLEN: Okay. Thank you. Thank you, Joe.

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

DR. CANTLON: We're going to make one program switch, moving the last speaker, John Case. Ian Hynd isn't here, I understand, so we're going to move the John Case presentation on technology to tomorrow at 8:45, and bring forward from tomorrow's agenda, Archie Richardson, Technology to Seal Underground Openings, move that to the anchor position today at five. We're running about 30 minutes behind, so we're well into the cocktail hour.

Our next speaker, then, is John Case.

MR. CASE: Good afternoon. The title of my presentation is "Airborne Goals and Requirements for Selected Sealing Components." The scope of my presentation is that first of all I'm going to discuss the modified permeability zone that Joe had touched upon earlier. This is a model that is used in many of the performance calculations that we have done, both hydrologic and airflow. Then after I have presented the modified permeability zone model, I'm going to move on to looking at radionuclide release mechanisms, airborne radionuclide release mechanisms, both convective airflow and barometric airflow.

The outline of the modified permeability zone presentation is that I'm first of all going to discuss the technical approach to developing MPZ modeling assumptions in fractured, welded tuff. I'm then going to present some elastic and elastoplastic stress analysis. Following that, I'm going to present stress permeability relationships, and then, finally, I'm going to present the modeling results.

As is shown by this schematic right here, and as Dr. Deere had pointed out earlier this morning, one of the important things with respect to sealing is the host geologic formation. We've known that we can select concretes or earthen materials that have very low conductivities, of the order of 10^{-10} cm/s, and yet we can have situations where flow would be occurring around the seal, through the modified permeability zone--which is right here--or possibly, also, as is suggested by laboratory testing at Terra Tech in the Bell Canyon test, possibly through the interface zone at this location right here.

We can look at stress relief mechanisms when an excavation is created. Basically, what I do here is to show a shaft right here, and as we have excavated, if we had some lithostatic state of stress, then we will induce a stress redistribution around this excavation. The radial stress at this point will go to zero along the face of the shaft, and in turn, if we have an elastic response of the rock, the tangential or boundary stress will increase, as suggested by this plot right here.

We can envision that we could have different systems of fractures that would evolve from the shaft excavation. We can envision a series of radial fractures, as is suggested right here, and we can also envision a system of perhaps "onion skin" fractures, and these two systems of fractures would be affected by the radial stress relief that would occur, in the case of the "onion skin" fractures, and the tangential boundary stress that might increase or decrease in the case of the radial fractures.

Some of the evidence that exists for modified permeability zones comes from the STRIPA permeability test which was done at the STRIPA mine in Sweden, where they had a room that was isolated and they had flow that was induced to the room, and they noted that there was a zone of reduced permeability which they thought was due to increased boundary stress for flow that was being conducted towards the excavation.

Of course, in sealing we have the situation that is of most interest is for flow parallel to the axis of the shaft, which would occur along the system of "onion skin" fractures where there has been radial stress relief.

So basically, what we have here is that we could possibly have opening and closing of existing fractures. We can also be creating new fractures, which would possibly be due to blasting, but also possibly could be done due to the stress relief in the sense that the stresses would exceed the intact compressive strength of the rock, and thus, induce fracturing.

Based upon these simple schematics and for a preliminary model, we developed this technical approach to the modified permeability zone. Basically, we would go and we would calculate the stress changes that occur around the shaft. We would then relate permeability to stress through field and laboratory tests. We would then calculate the rock mass permeability as a function of radius away from the shaft, and then, on top of this, we could estimate the effects of blasting if the shaft was excavated by drilling and blasting methods.

Just briefly to talk about blasting mechanisms, when we are doing drilling and blasting, we would have a series of holes which would be loaded and, as detonation occurs, then explosive gases would form and they would form a system of radial fractures like this. Shock waves would be propagated through the rock mass, and we might form another system of fractures like this. And this figure at the bottom here suggests the kind of pattern that might develop for conventional blasting.

It turns out that the Swedes have been involved in controlling blasting for some time in granite, and they've developed several techniques. One technique for controlling the extent of blasting is to have a set of peripheral holes that are more lightly charged and more closely spaced together. These holes, in smooth blasting, would be detonated after the main detonation. The fact that the holes are closer together will result in some super position of stress and a resulting smooth fracture at this point. The fact that they are lightly charged will result in a smaller blast damage zone.

Just to give you an idea of the scale of the systems of fractures that we're talking about here in welded tuff, I basically show a schematic that shows the exploratory shaft right here with a radius of 2.2 meters, and I superimposed upon this fractures that are spaced at 6 centimeters, other fractures that are spaced at a maximum of 50 centimeters, and so we get the concept from this that if we had excavation occurring and stress relief, that the stress relief would occur across a system of fractures as opposed to a single fracture.

Just to summarize, some of the modeling assumptions that we've made in terms of developing this preliminary model--and I should say that our purpose in developing this model is to use it for performance calculations, and so some of the assumptions that we've made are consistent with coming up with a conservative model for performing hydrologic or airflow calculations.

Basically, in this model we assume that the in situ state of stress is isotropic at depth. We orient the fracture normal to the direction of the maximum stress relief, which is in the radial direction. We calculate stress relief using closed form solutions, either elastic or elastoplastic solutions. And finally, in these calculations we're ignoring the effects of shaft liner support.

In order for us to make an analysis, we have to estimate the rock mass strength, and there was a study that was done by Brenda Langkopf and Paul Gnirk in which they did some rock mass classifications using the Bianewski system, and basically, they found for fractured welded tuff unconfined compressive strength ranging from 110 to 230 MPa, joint frequency varying between two to 16 fractures per meter, joint condition, lower bound slightly rough fractures, upper bound very rough fractures; and essentially, for purposes of classification we assumed that we have dry water conditions. Based upon these assumptions, then, they came up with a rating of 48 to 84, or average to very good rock.

We can then apply some scaling relationships that were developed by Evert Hoek for scaling the effects of the fractures on rock mass strength, using the empirical method that he developed. And what I show here is basically the triaxial compressive strength of the rock mass; in other words, minor principal stress plotted against the major principal stress at failure, and over the range of rock mass conditions that we looked at, you can see that we have considerable variability in strength.

I've also included in this plot some information and data that was developed by Fran Nimick, which shows a similar range in terms of strength. I should say further on, Joe Fernandez is going to present some information about oversized boreholes that tend to support the notion that some of these boreholes will develop plastic zones and fail.

Based upon the assumptions that I've talked about, we performed an elastoplastic analysis. We also performed an elastic analysis. The elastoplastic analysis, which was the upper worst case assumption assumed a lower bound strength for the rock mass, and upper bound in situ stress. And then we used some elastoplastic solutions that were developed by Evert Hoek to essentially develop tangential and radial stresses as a function of radius, and we did this at two different depths.

We did this at a depth of 100 meters, where the in situ state of stress is perhaps about 2 MPa, and then we looked at the in situ state of stress at the repository horizon. From this, you can see that a zone of stress relief in the case of the shallow 100 meter calculation was fairly shallow. This almost agrees with elastic stress distribution, and as we went deeper, we found that the elastoplastic zone was larger.

The next thing I'm going to discuss is laboratory studies of single fractures in welded and non-welded tuff. There were some laboratory studies that were done using a constant flow rate permeameter that is reported by Klaveter and Peters. Basically, what they would do would be they would take samples of core that had single fractures with various roughness. They would emplace these into this permeameter. They would then raise the pore and confining pressures to about 3 MPa. They'd then raise the confining pressure from a range of 3.5 to 16 MPa, and under these conditions, they would induce flow through the sample, through the fracture, and then they measured the flow and they knew the pressure differences, and on the basis of using the smooth wall fracture aperture relationship, they could calculate a fracture permeability.

Some of the data from their experimental work is shown right here. What we see here is that there's a fairly broad range of fracture permeabilities as suggested by this laboratory data, which is not inconsistent with some of the ranges that Joe had reported earlier. If we have a highly-welded rough fracture, permeabilities were up in this range, on the order of 3 Darcies. If we were talking about a non-welded planar fracture, permeabilities were much lower, of the order of approximately 1 Darcy.

The other thing that's interesting from this data is as we increase the effective normal stress, which is equal to the difference between the confining pressure and the pore pressure, we see that these appear to approach an asymptote. The fractures close up.

DR. DEERE: A question; Don Deere.

MR. CASE: Yes.

DR. DEERE: Just a comment, perhaps. It's interesting to note, though, that there is much greater difference between one fracture and the other than there is the effective stress on any given fracture.

MR. CASE: That is correct, that is correct.

DR. DEERE: And I think this is what we'll find in nature.

MR. CASE: Yes, that is correct.

What we did here was to take the permeability that was observed of high effective normal stress, essentially normalize this data as the ratio of permeability to that at high effective normal stress. So we can express the relative permeability for these different samples, and what we note here is that the rough sample is showing not much change in permeability as a function of normal stress, and the planar fractures are showing a much greater sensitivity of permeability with normal stress.

Concurrently with the laboratory studies, there was field studies that were done on single fractures in welded tuff at G-Tunnel that is reported by Zimmerman. Basically, a fracture was isolated. Slots were created around a block. The flat jacks were grouted in. Heaters were placed around the block, which allowed the simulation of various conditions within the repository. The flow tests that were done were to inject water into one hole into a near vertical fracture, and then to monitor flow rate in two observation holes. Again, what we can do is we can apply the smooth wall fracture aperture theory, take the flow rate and pressure data, and we can calculate changes in fracture permeability.

And I present that in this figure right here. It turns out that the load paths that were followed in the field testing were somewhat more complex than those that were followed in the laboratory testing, but it's interesting to note that with this data, that again we see that at low effective normal stress we have a fairly high permeability, and then it appears to come down and at greater than about 3 MPa, the fracture is closed up and there doesn't seem to be much variation in permeability.

Just to compare the field and laboratory measurements, what I show here is sort of the upper and lower bounds for a planar fracture right here. For a rough fracture, I show the relationship here of relative fracture permeability, and then I've plotted two simple lines to represent the more complex load paths that were followed in the field testing, and as you can see from this data, there is some general agreement between the field data, field measurements, and the laboratory measurements, and it appears that the laboratory measurements actually bound the field measurements in terms of sensitivity of permeability to stress.

What we then can do is to estimate what the changes in permeability would be as a function of radius away from the shaft. As is shown here, basically combining the stress analysis with the stress permeability relationships, and plotting those as a function of radius, and we've done this at two different depths again, and here I show an upper estimate at 100 meters, a likely estimate--which is using another set of properties which we think are the expected case, average rock mass strength, average in situ stress--and we've done this at 310 meters.

And the thing that's interesting to note here is that at 100 meters, there isn't a great deal of difference between the upper bound and likely estimates that occur in terms of changes in permeability, but when we go to a greater depth, because the analysis suggests that we have a larger elastoplastic zone that would develop around the shaft, we, hence, have a greater degree of permeability enhancement that would occur.

Briefly, I'd like to touch upon blasting. Basically, this figure right here shows the relationship of distance with peak particle velocity for a charged drill hole for different charged densities that would occur, and as we can see, as we get further away from the borehole, there is attenuation of the peak particle velocity. The peak particle velocity can be translated into strain, and then compared with strains that would cause incipient rock fracture, and from this type of analysis here, for incipient rock fracture, fracturing of welded tuff, we can come up with some estimate as to what the extent of this damaged zone would be.

Further case histories that have been done where people have tried to go out and measure changes in permeability due to blasting, they have found that they have measured some permeability enhancement and they think that the number of fractures that may have been induced by blasting would be increased by approximately a factor of three.

Summarized, some of the conclusions from our analysis, our preliminary model of the modified permeability zone would suggest that for the expected case of average rock mass strength, in situ state of stress, that we might see an increase in permeability of 20 over one radius. If we take upper bound-type calculations, high in situ stress, low rock mass strength, we might have permeability enhancement of the order of 40 to 80, and the analysis suggests that there's a significant contrast in elastic and elastoplastic response.

Just to show this more graphically here, this shows basically the relationship of permeability against radius, and shows the relative changes in rock mass permeability for the exploratory shaft which is of a radius of 2.2 meters.

The next part of my presentation is to discuss radionuclide release mechanisms due to convective airflow and barometric airflow. Just to summarize these two release mechanisms, at the repository horizon, after we emplace waste, there'll be a temperature rise that will occur and temperature gradients that occur within the repository or within the ground might look something like this. They'd reach a peak at the repository level, and then they would return back to the geothermal gradient.

With the high temperatures that exist in the repository, there is the potential for airflow to be induced through the rock and through shafts and ramps that would be within the perimeter of the repository since hot air rises and cooler air would be drawn in from the ramps. So this is the convective airflow mechanism that would occur.

Similarly, we can envision that the repository, at the ground surface there could be changes in atmospheric pressure, some atmospheric event that would cause airflow rate to be either induced inward or outward from the repository. This is suggested by this right here, where if we had a sinusoidal change in atmospheric pressure, then the repository air pressure would lag in terms of changes due to the change, and the amplitude of that pressure fluctuation at the repository would be smaller. And because of the differences in atmospheric and repository pressure, we could have flow rate that would be occurring through the rock, through the shafts and ramps, away from the repository.

DR. CANTLON: Before you move that, Cantlon.

Is this assuming that it isn't closed and sealed?

MR. CASE: It makes no assumption with respect to whether it's sealed. Clearly, if it's not sealed--actually, the next slide that I present maybe clarifies this a little bit.

DR. CANTLON: Well, the lag would obviously be much greater if it's sealed.

MR. CASE: That's correct, that's correct. The lag is dependent upon what the conductivities are of the backfill and the rock.

Some of the modeling assumptions we've made in this analysis is for backfill shafts and ramps, we assume that Darcy's Law is valid for airflow. We assume that the rock and the air are at the same temperature at the same location; hence, the flow rate is small and we don't have a heat engine developing that would expel air at a high temperature. The calculations that we've done assume that the air is dry and the flow is incompressible. Further, we assume that air circulation occurs along specified paths.

And in the case of the air for the barometric airflow case, since we have to know something about the compressibility of the air within the repository in order to perform an analysis, we're assuming that the ideal gas law is obeyed.

This slide here shows mechanisms for convective airflow, and we can sort of see conceptually that there might be two cases here. If we had a very coarse backfill, or if we'd left the shafts open, then what we would have is the shafts and the ramps would, of course, become the dominant flow paths, and we essentially would have air that would be drawn in, say, through the exploratory shafts in this area. And in the older design, air would be drawn in from the emplacement exhaust shaft and the waste ramp and the tuff ramp.

If we have a lower conductivity, then there would be flow that was occurring through the shafts and ramps, and also through the rock, and it's this mechanism here where we're looking at flow that would be induced both through the shafts and the rock that is of interest.

Just some of the assumptions that we made in the convective airflow modeling, we have looked at calculating draft air pressures based upon the temperature differences of two columns of air, and using the density method, and assuming that the repository reaches a peak temperature and the surrounding shafts or ramps where air would be drawn in is at a lower temperature, we calculate 1.4 inches of water gage, or .35 kPa. And again, we're saying the flow is occurring along specified paths.

And what we have done here--to address the comment that Dr. Deere made here about the variations in conductivity within the rock being quite large--we developed three different combinations of rock mass conductivity. We looked at non-welded and welded units above the repository. Combination 1 was 10^{-5} cm/s. I'm expressing these conductivities as hydraulic conductivity, although the analysis was done assuming air conductivity. Combination 2, which assumes that the welded units are much higher in conductivity, but the non-welded units are low, 10^{-5} and 10^{-2}, and then, finally, the third combination was higher conductivities in both non-welded and welded tuffs.

What we then did is to apply this convective airflow model, and we calculated what the total flow rate would be out of the repository as a function of the shaft fill air conductivity for the three different rock models that I've described here, covering the range of conditions that we think are applicable. And basically, we see that in some cases here, if we're dealing with the high model right here, you can see that when we have a high shaft fill air conductivity, airflow rates are high. As we reduce the airflow rate down, then this approaches a constant value, which is reflecting the modified permeability zone; in other words, at some point in this process, for different combinations of rock conductivity and the assumption of a modified permeability zone model, the total flow rate is occurring through the modified permeability zone and not through the shaft fill.

What we can do is express the percentage of flow that's occurring through the shafts, or we can express the flow that's occurring through the shafts as a percentage of the total rock flow that's occurring, which is shown here. And so, basically, if we had a very high conductivity shaft fill, then we would have 100 per cent, or nearly that occurring through the shaft fill. But as we reduce the shaft fill air conductivity downwards, then the percentage of flow that's occurring through the shafts and ramps becomes smaller.

And in the technical basis report that Joe had referred to earlier, there is some discussion of establishing a percentage of airflow that we could allow to occur through the shafts and ramps as an airflow performance goal, and I think that was to restrict the flow to a percentage, perhaps

1 to 5 per cent of the flow that would occur through the rock, and so from this we can see that we can achieve that goal by selecting a low shaft fill air conductivity.

Just to conclude some of these points here, for convective airflow, air flow occurs dominantly through backfilled shafts and ramps for seal conductivities that are greater than 10^{-4} cm/s. Airflow occurs dominantly through the MPZ for low seal conductivities, as intuitively we would expect. The analysis is conservative in the sense that we are looking at the maximum temperature differences between the areas where flow is being drawn in and the hotter parts of the repository. And finally, we can satisfy the performance goal by selecting a permeability, a seal conductivity of 10^{-2} cm/s.

Let me move on to the case of the barometric pressure model. Just to re-familiarize yourselves with this, we can have some atmospheric fluctuation occurring here, inducing airflow through the rock, through the shafts, and through the ramps.

On the basis of this, we can derive a simple, ordinary differential equation that describes the changes in flow, which is shown up here. This should be dPR/dt. And basically, we can derive this equation right here, and we have this constant here, which is a function of the properties of the gas, the temperature of the air in the repository, and the conductance paths. Further, if we assume for the term right here that the air pressure varies as a sinusoid, then we can develop a formula that calculates for the displaced air volume as a function of this constant C, the amplitude of the pressure event, and the frequency of that pressure event.

We looked at several cases here. One was a thunderstorm. We developed a sinusoid like this with pressures that would be--and it's difficult to read, unfortunately--of the order of maybe 30 millibars over a period of perhaps a week. So that would be a weather front that would be moving across Yucca Mountain.

We looked at a tornado event, which would be an event that would be of several minutes, and we calculated very high pressures that would develop within that tornado event of perhaps as high as 150 millibars.

And then, finally, we looked at a seasonal fluctuation which was a much lower pressure fluctuation, on the order of one to several millibars over a period of a year.

On the basis of this, then, we can calculate what the ratio of the displaced air volume to the total volume within the exploratory shafts would be for these various events, and again, the equations that I showed there included flow paths in terms of air conductance for the shaft fill air conductivity, and also the surrounding rock. And we can use the same three models here--low, intermediate, and high--in terms of conductivity.

And so what we see here is, again, the ratio of displaced air volume. When the shaft fill air conductivity is high, we have a fairly high amount of air that could be displaced out of the repository from the shafts, and as we reduce the shaft fill air conductivity, this approaches a constant value which again is constrained by the modified permeability zone model that we've assumed; in other words, there's a point of diminishing returns with respect to the selection of the low shaft fill air conductivity if we have a modified permeability zone.

This shows the same analysis applied for the high frequency, high pressure tornado event, and again, I think what happens here is we have less air that would be actually displaced outward, but we see similar trends, and I interpret that as the tornado event is finished before the air has a chance to respond, as actually one could show by a response spectrum analysis. But again, we see a similar behavior here as we lower the shaft fill air conductivity. Then the ratio is approaching a constant value.

Finally, for the seasonal event, we again have relationships that develop. Interestingly enough, at high conductivities, this approaches a constant value, which is interesting in terms of it approaching a constant value, and you can actually show that by looking at the basic equations that have been developed.

To summarize the barometric airflow analysis that we've done, we've calculated the displaced air volume out of the repository could be 1/10,000 to 1/10 of the shaft air volume during a thunderstorm. Airflow occurs dominantly through backfilled shafts and ramps for high-seal conductivities. Airflow occurs through the modified permeability zone for low-seal conductivities. The seasonal and tornado events appear to be of less significance, and for high-seal conductivity, displaced air volume approaches an asymptote.

The conclusion from this is the displaced air volume due to an atmospheric event can be controlled by emplacement of a backfill with a low conductivity. We can restrict the amount of air that's displaced out of the repository, and with that, I conclude my presentation.

DR. CANTLON: All right. Questions from the Board?

DR. DEERE: I'd like to go back to the table that you had before you presented all the graphs that showed the permeabilities in terms of three combinations of rock mass conductivity, Combination 1, 2, and 3.

MR. CASE: Yes.

DR. DEERE: Now, the Combination 1 is the one that in all of your other graphs you call low; am I correct?

MR. CASE: That's correct.

DR. DEERE: So the Combination 1 is low, and the Combination 2 is our intermediate?

MR. CASE: Yes.

DR. DEERE: And then the high. Now, all of the figures that you show, or a number of them, have the modified permeability of the damaged zone.

MR. CASE: That's correct, yes.

DR. DEERE: And if these were raised bored shafts, or if we're talking about a machine TBM-mined incline, then we'd be looking at something considerably different.

MR. CASE: Possibly. From the standpoint of drilling and blasting, I would agree with you. From the standpoint of stress relief, I think that our calculations show that we would have stress relief. Of course, it would depend upon the degree of support. The thing I would say about the calculations, they were done as a worst case assumption in terms of coming up with a model that we could use for calculating airflow; and, hence, we've neglected liner support or artificial support that would have a tendency to restrict the amount of flow that would occur through the modified permeability zone.

Further, in these types of calculations, we also are not looking at, you know, if we have a seal that we place in the ground which develops expansively some stress across the interface zone--which I will discuss subsequently--we're neglecting any recovery or any reduction in permeability that might occur due to that. So the purpose, the objective of this particular model was to use it for coming up with conservative analysis for flow that would occur through the modified permeability zone.

DR. DEERE: But when you talk about the stress-related fracturing, you certainly are not going to get these ring-type release fractures.

MR. CASE: We used that as an idealization. We assumed that the fracture--again, conservatively--the direction of maximum stress relief would be in the radial direction, and we assumed that if we had fractures oriented that direction, then there would be maximum stress relief and change in fracture aperture. In point of fact, again, we may have the fracture oriented in some oblique angle to the state of stress, and therefore, we might not expect quite the same change in conductivity occurring. It may be less.

On the other hand, the analysis is looking at changes in normal stress and is not looking at the effects of shear and dilatancy that would occur. Shearing stresses across fractures could cause some dilatancy. So, on balance, we think that for the purpose of the calculations that we're doing, that this is an appropriate preliminary model.

DR. CANTLON: Other questions? Yes, Russ?

MR. McFARLAND: Russ McFarland. You made a statement a minute ago that the fracture or the modified zone due to stress relief would be a function of the ground support. Would you amplify that, please?

MR. CASE: Well, if we were able to go in there as the face is advanced, and we were able to put support up, then as the face is advanced further, there may be some interaction between that artificial support and the surrounding rock that would result in perhaps some beneficial effect; in other words, the degree of loosening that would occur in fractures around the excavation would be smaller, but in this analysis, we've neglected the effects of support, liner support. And, of course, it would depend upon the timing with which that support was emplaced when the face was advanced.

So our purpose here was, again, to neglect the effects of artificial support to come up with a model that could be used for flow calculations. Does that answer your question?

MR. McFARLAND: That's fine. Thank you.

DR. CANTLON: If you're looking at this question as a fundamental aspect of whether the repository could retain, let's say, Carbon-14, in looking at flow rates, that needs to be looked at in terms of the total pool or mass of gas that's in the system. Do you have any perception of what per cent of the total enclosed mass of gas in there would move out in these pressure events?

MR. CASE: That's an interesting question. It would, again, be dependent upon a sort of an overall performance assessment and the conductivities that would be appropriate for flow to occur. We have not performed any analysis that looked at that effect, although I think we could easily use this model to evaluate those effects.

DR. CANTLON: Well, pursuing that, since there is thought to be some exchange between the radioactive carbon and the nonradioactive carbon in the rock mass, then the lag time moving out would influence the amount of exchange that would take place so that the actual release, really, this is one of the ingredients out of two other major variables; the total mass, the time that it resides in there, and the interaction with the rocks. Has that calculation--

MR. CASE: I would have to say that we have not done that calculation. I think our purpose with these calculations was to investigate the shafts and the ramps themselves as part of our sealing purview here. We have not actually looked at some of these issues, although I think they could be looked at from the standpoint of using this simplified model. Our purpose here was to see if, given that we backfilled the shafts with properties, engineered properties, what do we think we can achieve in terms of restricting flow.

DR. CANTLON: Right.

MR. CASE: And so, unfortunately, I can't answer your question.

MR. FERNANDEZ: Can I add a little bit to that? Joe Fernandez.

The purpose of doing this calculation was to try to address the 10 CFR 60 criteria dealing with preferential pathways. In the sealing program, we're concerned with, as you know, the shafts, ramps, boreholes as preferential pathways. This wasn't intended to be a total system performance analysis.

We used some of the values on Carbon-14 inventories, and we said using the results from Lawrence Livermore's laboratories and others as to what fraction of that might be released as Carbon-14, we made an estimate of how much could be released in a gaseous form, assuming no transfer at all. And we said there was X number of Curies that could be released based on the other people's work, and we would like to restrict the amount of Carbon-14 release via the shafts to one Curie per 1,000 metric tons of heavy metal. I think the regulations are 100 Curies per 1,000 metric tons of heavy metal, if my memory serves me correct. So we were trying to control the release of Carbon-14 to one per cent, one per cent of the total allowable release.

Now, on the second point you talked about travel times. We did do some calculations on travel time for the entire area of gaseous flow up, and that's very much dependent upon the quality of the rock, you know, the air conductivity of the rock, and since there is, as you've seen in these calculations and the previous calculations, many orders of magnitude that we varied these calculations, there is equally many orders of variation in the travel time calculations that you might expect.

And I've also noticed recently there is another report that I just read where they actually assumed multiple travel times because they also, in this report--which I can't remember the title--acknowledged the fact that travel times could vary considerably depending upon the rock quality. So we have to keep that in mind, too. We need to know a little bit more about the air conductivity of the rock mass.

DR. CANTLON: Other questions from the Board?

DR. DEERE: Yes. In the--I think it was probably the in tunnel at the test site at a depth of about 1400 feet. I recall a number of the boreholes that were made from the tunnel walls would change from circular to elliptical in just a question of days, and it was really a shear failure because as it became elliptical, you could actually see the intersecting shear zones at the two sides and just take them out with your hands, and so you got a nice elliptical thing;

not concentric simply because the state of stress was not one.

MR. CASE: It was an anisotropic--

DR. DEERE: Yeah. It was probably .8 or something like this, and then with the stress concentration from the opening itself, we had a higher stress than we had strength, and that formed.

But I'm still very worried about the relative permeability. I just can't believe that a permeability of 10^{-2} cm/s can't bring about drastic changes in the behavior of the movement of gases.

MR. CASE: You mean in the sense of it--

DR. DEERE: Well, in the sense of your--where I have found this in other projects where we've had gas under pressure which moves, it's almost always when we have a impermeable layer, a less permeable layer over the permeable layer, and therefore, it's restricting any loading and unloading of the gas pressures or the movement of the gases, and we do have here the Pah Canyon.

MR. CASE: Yes.

DR. DEERE: And I guess this was the one in your combination that was called the non-welded tuff?

MR. CASE: I think that's Combination 2, I think, was the one that you're referring to where you have a low non-welded Pah Canyon thin zone, and what we would do in these calculations, we would calculate the harmonic mean for the permeability and equivalent permeability for flow that's occurring in series through these units. And so, I think your question is--let me see if I understand.

DR. DEERE: Well, I can't see how you can come in and short circuit a bedded layer that has greatly different, by a factor of 1,000 or 10,000, permeabilities and not say that a shaft backfilled to 10^{-2} is not a short circuit. It seems to me like it is.

MR. CASE: Well, it depends upon the conductivity of the shaft fill, is what our analysis would suggest.

DR. DEERE: Yeah, but you have other presentations earlier today that said 10^{-2} is great.

DR. HINKEBEIN: Tom Hinkebein. The answer to your question deals with the other part of the term. You have a resistance factor, you know, k_x/A. The area term for the rest of the repository is so much larger than the area that's involved in your shaft alone, that when you consider the resistance factor for the shaft and the resistance factor for the rock, the resistance factor for the rock, because of the large area, gets to be--that's the term. The area term is the one that swamps out the rest of it, simply because the numbers of pathways that you actually create are so small.

DR. DEERE: I see your point and don't agree with it. I see your point, and certainly, it is--I guess it's relative, but I've seen it on a large scale and all we do is poke a hole through the lining and we blow gas.

DR. HINKEBEIN: Again, the question here is one of absolutes and relatives. The mountain itself breathes, and the total amount of gas that can come out of the shaft can be large. It can breathe, especially if you have nothing else in there but a 10^{-2} conductivity backfill. However, you can't feel, simply because it's so diffused, you can't feel the air coming out of the rock mass as a whole, but it's a large number, especially when you consider--

DR. DEERE: Yes. It might go from 10^{-5} to 10^{-7}, and you may well have some bets at 10^{-7}. You're going to get a lot different answer in the effect of your large repository.

DR. HINKEBEIN: Yeah, you're right; absolutely. The point to be made here, again, is that in the meeting of 10 CFR 60 requirements and the way that the requirements have been apportioned for these analyses, you know, we say the total answer is going to be one per cent. We're only going to let one per cent go up. So when you spread this number out, you've got a small number over a very large area. I think you understand this--good--a small number over a very large area still gives you a lot of mass of gas getting away. So you can allow some reasonable amount of gas to go up the shaft and not interfere with your overall performance assessment. The mountain itself can stand on its own merits, and the shaft won't add to the complexity of the problem.

DR. DEERE: I think we can see by your difference of Combination 1, 2, and 3 in several of the graphs, it really does have quite an effect on some of the properties.

DR. HINKEBEIN: Indeed.

DR. DEERE: So if you're off by a factor of ten, then the answer we're coming out with is off by a factor of ten.

DR. CANTLON: Other comments from the audience?

(No audible response.)

DR. CANTLON: If not, then, Tom, I think you're up next; Tom Hinkebein.

DR. DEERE: I would like to mention that in a later discussion when we have some more time, I'd like to go back and talk a little about the blast damage permeability, because this is an argument that is now going on very violently in Sweden, and we were just there three weeks ago and had a chance to go down and see where the argument was coming from, so I think it's of great interest. The blasting, even in Sweden, does damage the rock. The only comment I guess they didn't like from me was, I thought we were going to come to Sweden and see some good blasting.

(Laughter.)

DR. HINKEBEIN: I'm going to talk about geochemical considerations. In particular, what I'm going to focus on are materials.

A little history: In the starting of selection of a material for the repository, we started out by saying we need to have some way of doing this. The plan was to develop some functional and design requirements. Those Joe talked about this morning, and I'll show those to you again briefly. After you've developed those design requirements, then you need to develop some specific design requirements in particular--I'll talk about those--and then you evaluate materials and say, well, how well do our materials stand up? Are they good or not? Then, as time goes on, you repeat the process as you learn more and more about the repository, so I'm talking about a process that's ongoing, but there's a lot of history here.

The initial material screen started with functional requirements; containment and isolation, human intrusion, longevity, and cost. From those, we developed some specific criteria that materials that we would select for sealing would have to meet; permeability, strength, and so forth. We then developed a large matrix of candidate seal materials and said, which of these are going to meet these initial design criteria that we've selected? And we ended up throwing away the ones below the dotted line, simply because of their relative performance relative to the ones at the top; in other words, when you've got materials that look superior in a lot of ways, you don't consider the inferior ones.

The organic materials were thrown out because of their likelihood of allowing complexants to get into the repository; ceramics because of their difficulty of emplacement. So we ended up then with four materials being carried forward for a closer evaluation. Again, these design requirements were developed for strength, emplacement considerations, groundwater chemistry, and environmental conditions.

In the case of strength, these are general requirements, so we didn't really look at specific things. We looked at more, generally, how well do the materials meet what we expect is going to be required. In terms of strength, we can imagine areas where you would need high strength, areas where you would need low strength. In terms of the emplacement considerations, there are some that would require bulk emplacement, some that would require remote emplacement. Groundwater chemistry, in this case our concern is that we don't want to introduce such a large perturbation to the system that we interfere with the waste package. The environmental conditions, temperature, in situ stress, and so forth, had to be evaluated, and specific design requirements were developed for the hydraulic conductivity.

Joe showed you previously some design options, and he showed you how you develop design requirements for those design options. What I've done here is show you that in this early stage of development, we said, well, let's pick some candidate materials and see how well they perform. For the anchor-to-bedrock seal, we selected standard concrete. For the backfill and the shaft, we selected a crushed tuff, and the hydraulic conductivity required for those options is shown. For the repository station seal, we didn't highlight one material, but we said there's two that could possibly meet that application; both standard concrete or compacted earth. I'm not going to show you all of the options. The point here is that we've got a mechanism for selecting the candidate materials.

Given that we've got the mechanism for selecting them, how well do we expect that materials will perform under these circumstances? I skipped a view graph. So what we did at this point was to examine the hydraulic conductivity that was shown in the literature, you know, what kinds of things could we expect from the materials?

For cementitious materials, you typically can get values 10^{-8} to 10^{-6} cm/s. We actually worked with Penn State some years back and developed material whose hydraulic conductivity was 10^{-10} and held that value for a period, an accelerated test of three months, at 90° accelerated. We did it at 38° for two years, and also it held value that was less than 10^{-10} cm/s.

What this really indicates is that if you've got material that has a very low hydraulic conductivity requirement, we think we can probably meet it, but there are some other considerations which we'll get to.

In terms of the stability of cementitious materials, there is a great possibility for chemical alteration. We'll talk about that some. Groundwater chemistry, if you look at cement, the big one, the first one that stares at you is the increased pH. The other increases are shown. We also did an initial evaluation and looked at some interactions with the cements and the surrounding tuff, and in this case, our concern was are pieces of the cement coming off and are they going to interfere with our sealing system? Is the water going to be so perturbed, so different that it interferes? And what we find is that there is a stabilizing effect of the tuff.

That's another way of saying that, yes, we realize that there is going to be some tuff/cement interactions, but those tuff/cement interactions are going to bring the water composition back to a norm that's close to the J-13.

In terms of strength, I want to say two things about it. One is that you can certainly get high strength, but maybe an even more important aspect of strength is that with cement, it's very controllable. By adjustments, you can control the cement strength.

For earthen materials, again, we've got a relative goodness in how well did the materials perform relative to what our requirements were. For typical values of earthen materials, you can get down to 10^{-5}. If you add some clays to it, you can get down even lower, although we have less confidence in being able to maintain those real low hydraulic conductivities at this time. It may be possible.

In terms of the stability of earthen materials, one of the things that's going to happen is that they're going to tend to dehydrate as the temperatures go up. That dehydration can cause volume changes and potential cracking problems, and those need to be addressed very carefully.

In terms of groundwater chemistry interactions, you don't see quite the pH effects, but you sure do see a lot of other ionic increases as you increase the temperature of the groundwater that the clays are sitting in. However, we also feel that those increases are controllable through a judicious choice of the clay composition and, as a matter of fact, we've played some modeling games with EQ3/6, and what we find is that by judiciously altering the composition of the clays and the types of clays, you can get compositions that are right on top of the groundwater composition.

All right. Given that this is where we were, this is the piece of history that said, yes, we've got two really good types, general types of candidate materials that we should evaluate further. Those are cementitious materials and earthen materials. What we have done at this point is we've carried the characterization further for cementitious materials. In particular, we've done that out of the realization that these materials are going to be used in the repository for other applications and our desire to be supportive of the other parts of the program, and also that the cementitious materials have a huge history, and so they're certainly worth evaluating further.

So let's talk about how cementitious materials will tend to degrade as a function of various environmental conditions. I've highlighted four types of environmental conditions that I found that I think require some more talking about. These interactions can cause your cement to have change in volume. That change in volume will be manifested in changes in porosity, and through the modeling efforts, to a new permeability. I'm not going to talk a whole lot about fracture flow modeling, matrix and fracture flow modeling, but in the overview, in the long term, big picture, that has to be considered.

Thermal-mechanical interactions. All right, I want you to imagine that the thermal-mechanical interactions that I'm talking about are taking place 100 years from now. They are not emplacement considerations. They are considerations that derive from the repository heating, and that repository heating will cause the seal materials to expand and generate stresses. So what we're looking at are the magnitude of the stresses generated, and comparing those to--well, the way we calculated the stresses were based on just a textbook analysis out of Timoshenko and Goodier. Our intent here was not to be sophisticated with the analysis, but really to learn something about the materials. So the application of this in terms of a global application is limited only insofar as how it applies to the materials.

All right. Further on, we'll also talk about microscopic effects. If you've got little pieces of aggregate or something, minerals that are included within the cement seal, we'd like to find out what stresses those have on the matrix, the cement matrix. So I'll be talking about both effects here.

In terms of the microscopic analysis, if we look at the kinds of inclusions that we've considered, they are typical inclusions. Without trying to get too geochemical in talking about cement minerals, ettringite is the expansive mineral that is proposed as a way of getting a good, tight bond. Some people would add extra sulfate to the material to get that tight bond, give you a better contact with the tuff. Hydrogarnet is an alumina phase; Portlandite, silica, gypsum. You can also have some unreacted cement phases and then, of course, aggregate, and what we want to do is, through the textbook-type analysis, is look at the stresses that are generated and compare them to the confining stress.

For gypsum, microscopic analysis, what we've done is looked at the stresses that would be generated at a station seal location, so 100°, roughly, C increase in temperature, and how do those stresses compare to the tensile strength and the compressive strength. We find that they're greater than the tensile strength. What does that mean to our seal material? It means that we've got a tendency for a gypsum inclusion to crack the C-S-H matrix that's surrounding it. That's a tendency that can be viewed as an instability. It should not be viewed as saying, oh, then if you've got any gypsum in your cement, then that's going to cause it to fail. No. It's a tendency. It just is an indication that this is a material whose presence is to be watched.

If we do the same kinds of calculation for other materials, we find that Portlandite and unreacted cement phases are also materials that will tend to expand at a greater rate than the C-S-H matrix can fight.

If we now shift our attention to a macroscopic analysis, you look now at the big seal, in this case we find that there are a couple of parameters that are very important; water/cement ratio, and aggregate fraction. As you increase the water-to-cement ratio, you tend to reduce the stress within the cement plug through the thermal-induced stress. Also, as you increase the aggregate fraction or the percentage of aggregate, you also reduce the stress. These stresses, however, are always so much less than the confining stress that the effects are relatively minor, and our conclusion is the thermal-mechanical or thermally-induced stress inside the seal is not going to particularly cause a failure.

Let's turn our attention now to mechanical interactions. In this case, we examine, again, through textbook kinds of solution. We look at the stresses generated inside a plug under confining stress, and we ask ourselves, are those stresses large or small relative to the confining stress or the tensile strength of the cement.

In the case of maximum principal stresses observed, we find that those stresses are less than ten. When compared to the tensile strength and the confining stresses, that's viewed to be a minor effect. These are all compressive strength.

When you have a situation where you have a low horizontal stress--and that's modeled here as a zero--you start to see that you do get some tensile numbers. The numbers out here that are negative are all tensile, and that could be a situation if you had--I guess the zones where you might have low horizontal stress would be something like a faulted zone. It wouldn't necessarily be a faulted zone, but it could be there, and under those circumstances, what you see is that if you increase the water-to-cement ratio and reduce the aggregate fraction, you tend to avoid some of the problems, and I think the reason that that's happening is that your seal tends to be more plastic.

Another aspect of mechanical interactions deals with concrete creep or cement creep. We used a method just based on handbook, and we computed the creep as a function of water-to-cement ratio and aggregate fraction again, and what you see is that for high water-to-cement ratios, you can actually get creep that is so large that it relieves all of the generated elastic strain, so you can actually make a seal that's plastic enough so that it doesn't have any residual stresses in it. That should probably be viewed as a positive for a lot of applications.

DR. DEERE: But doesn't it also, when you go to a high amount of water for the cement, give you a less--a more permeable cement?

DR. HINKEBEIN: That's exactly right.

DR. DEERE: It's one that you can leach out the cementitious material with water flow much easier than if you have a dense cement.

DR. HINKEBEIN: That's exactly right. The permeability and the porosity do--the porosity goes up, the permeability goes up as a consequence of adding more water. So, yeah, that's one of the drawbacks. But you've got a game that you can play here. You've got a trade-off analysis that needs to be considered. If your goal is to make sure that you've got a seal that maintains its integrity, and whose properties are several orders of magnitude less than what you could actually get if you put in the tightest possible seal, then you should do it. Do you follow the comment?

DR. DEERE: Right.

DR. HINKEBEIN: Okay, shrinkage effects. In this case, what we were considering is that relative humidity does shrink and swell cements, and we were wondering, are those effects significant enough to be considered important. For in situ saturations between .4 and 1.0, it can be shown that the relative humidity is going to be maintained at a very high number, and based on that, I conclude that saturation variations will have very little effect on shrinkage and swelling.

The last part of this overall presentation deals with geochemical interactions. In this case, I want to talk briefly about a little modeling exercise that we did where we combined J-13 water and cement and looked at the effects. I also want to highlight--and I'm only going to do it right here. I'm not going to do it later. I want to highlight cement-tuff-water interactions. We have done some of those. They're not presented here, but they do show that your water comes back to a baseline condition. It's not exactly J-13, of course, but it is of moderate pH.

I'm not going to talk very much about kinetic effects. We do have a program ongoing to investigate how does the kinetics of these operations affect, or how are they determined? This is an experimental program. The validation effort is also experimental. Leaching effects, we'll also be looking at. What we're going to be examining here is how fast do external minerals pass through a cement boundary. How fast do they get in there and start to cause the geochemical changes to occur?

EQ3/6 model is a very difficult model to run. It requires more assumptions sometimes than you think that are reasonable. In order to get anything, you need to propose surrogates. You need to make assumptions about equilibrium. In this case, we tried to do worst case assumptions.

In terms of a closed system, open system, you need to make a decision. Are you going to allow gases to be replenished when they're consumed? Are you going to allow materials to be replenished when they're consumed, or are you going to shut the system and close it off? You get different kinds of minerals formed depending on the assumptions you make.

I'm going to be reporting on a closed system assumption. That would be more appropriate for the consideration of an internal portion of a seal, and you also need to suppress minerals in order to get the Code to work. A good example is quartz. If you don't suppress quartz, every piece of silica in the system will thermal-mechanically be favored to go to quartz, and that isn't a very likely occurrence if you consider the mountain and kinetic limitations, kinetic favorably.

We looked at three different kinds of concrete; the first of these--again, and this can be viewed as supporting the rest of the program--is an ordinary cement. I'm wrong. Hang on. All right, this cement or concrete is one with a balanced amount of calcium and silica. The way that you would achieve that balanced amount is as you had suggested, Dr. Deere, is by adding excess fly ash or silica fume or silica flour or something. This particular formulation has a nominal amount of ettringite, which is the expansive phase, and a nominal amount of hydrogarnet. It's a very simple cement. Our intent here was just to get a handle on the process and say, well, let's put in what we think we probably ought to be using.

Another formulation we looked at was an ettringite-rich concrete, twice the amount of ettringite as was shown in the first one. It has a huge excess of silica. The third cement, which is the one that I thought I had up the first time, was an ordinary Portland cement. In this case, you've got a huge excess of Portlandite. By the way, this huge excess of Portlandite shows up here because we presume that we're 100 years down the road and not right now, and that a lot of the C-S-H gel, the matrix material, has transformed, and it's transformed to a preferred chemical species, and that preferred chemical specie is tobermorite.

DR. DEERE: Excuse me a minute. Is the active alkali there, the 1 per cent, in the original cement?

DR. HINKEBEIN: That's in the original cement.

DR. DEERE: Yeah, but wouldn't you want to specify a low alkali cement, .5, .6?

DR. HINKEBEIN: As a matter of fact, you would.

DR. DEERE: Because almost every one of the expansive aggregate problems that have developed with major structures have had .7, .8, one per cent free alkali.

DR. HINKEBEIN: Right.

DR. DEERE: And the testing has shown if they had been .5 or .6, there would not have been enough alkali to give the alkaline reaction; acceleration.

DR. HINKEBEIN: Right. That's exactly right, and the reason that we put the alkali in there is that our intent here is to look at cements that we saw. We could foresee the results and go right to the answer, but then you don't have the analysis to show that you made the right decision.

DR. DEERE: But might you go to--

DR. HINKEBEIN: Yes, you certainly would want to go to a low alkali.

DR. DEERE: Low alkali.

DR. HINKEBEIN: This is the water composition that we used. I'm not going to say a lot about it. It's a low solids, calcium carbonate, sodium carbonate, buffered water.

All right. The EQ3/6 Code works the following way: If you imagine you have a beaker and you put in little pieces of rock, and you follow the water composition as a function of adding the little pieces of rock, this is the kind of change that you get. Log z are the moles of cement phase added, so if you've got three phases that you're adding, the Log z will be, say, 10^{-6} moles of all three phases, and we follow, then, the concentration of species as a function of reaction progress, not time. pH you've got plotted on the right.

Up until a relatively large addition of cement, the properties of the water remain unchanged. It's very well-buffered. As you get above about Log z of -4½, you start to observe the first precipitation of calcite. That consumes about all the carbon dioxide in the system and there's where one of your first assumptions starts to happen.

At -3½ you have the first precipitation of tobermorite, the first real cement mineral. pH rises rapidly. Aluminum concentration rises rapidly. Silica drops and you start to have the rapid change in the system until you get to the precipitation of the hydrogarnet phase, the aluminate phase, and then lastly, up here you get gypsum precipitating.

If you look at the same kind of plot for the high silica, ettringite-rich cement, it sure does look the same. You could lay the two plots right on top of one another, and they're so close. The only difference observed here is that you've got a little higher elevated sulfur content, all of it deriving from the decomposition of ettringite.

For ordinary Portland cement, again, you get the same kind of plot. So in terms of the groundwater chemistry, you don't notice a whole lot of difference between any of the cements. This is the chemistry of the water very close to the cement.

All right, and what are we going to do with this? Through these runs, we've identified some new stable products. In fact, we've been able to pull out just a few chemical reactions that describe 95 per cent-plus of all the mass change that takes place. From those changes in mass and the phases that have left the system and new ones that have been formed, it's possible to create a new volume. That volume is, again, based upon an equilibrium assumption and no leaching yet. This is then translated to a porosity change and a permeability change.

I want to show you the last few results here just to give you an idea of what we're trying to do. In terms of projected volume change for the balanced cement--that's the real short one up here close to the origin--your alkalis caused this early decrease in volume. They dissolve out just right away. They're the first to go. Ettringite is another mineral that's very unstable. It tends to revert to hydrogarnet, and you get some gypsum precipitated. Remember, we talked about gypsum earlier. It's a definite problem component, and you also produce water which leaves the system, and that's what opens up the structure.

With the silica-rich and also ettringite-rich EPC cement, what you observe is that the initial volume reduction is a lot larger because you had a lot more ettringite. And then, after you get all of the ettringite consumed, hydrogarnet then reacts with some of the excess silica to give you some zeolites and also tobermorite back, and what happens there is that it incorporates water into the structure, tightens it back up again, so you actually get some improvement in performance.

The third cement that we look at has the same kind of initial performance, except the ordinary Portland cement has the Portlandite reacting with the tobermorite, finds a new phase in there and that's where we get a new cement phase that's getting rid of water and the cement structure opens up a whole bunch. So in terms of observations here, what we see is that a high silica cement is a much-preferred combination over a high Portlandite cement; calcium cement.

In terms of projected changes in permeability, the way we infer this graph from the previous one is by taking half a dozen different experimental data that relate porosity to permeability, and look for those porosity changes to be translated into a permeability, and we've computed them in terms of a ratio. The ratio shows that the changes are all less than a factor of two, so even in the case of the OPC, ordinary Portland cement, high calcium, the changes are fairly small. However, a word of caution is that the permeability/porosity models that we used showed wide variability and you could get factors probably increasing by two orders of magnitude greater than this. Is that a problem? No.

The reason it's not a problem is that for many of the sealing applications, we've still got six orders of magnitude between the performance that we actually expect and the performance that's required.

In summary for the geochemical interactions, we make the following conclusions: Calcium-rich and silica-rich concretes have very similar responses in groundwater. At very small concrete addition, the solution is strongly buffered by J-13 water, and at large concrete additions, you do have concrete dominating.

In terms of alteration for the conditions considered, the mass and volume changes can be described by the reactions that I showed you on the previous view graphs. The important ones, ettringite and Portlandite, tend to open up the concrete structure and so for long-term performance, those materials are not, to my mind, a preferred option, forgetting high initial tightness. Excess silica tightens the cement structure or the concrete structure, and I look at that as a very strong positive.

In conclusion for everything, I think that the material screens indicate that we can have either cements or earthen materials perform suitably in this application. Further, for the cementitious materials, we find that high-quality cementitious seals are very likely achievable, at least from the results shown. Important factors to be considered are the ones shown, and the calcium-to-silica ratio is very important. I think high silica is valued. Water-to-cement ratio, that just shows that the strength is tuneable. The aggregate weight per cent falls into the same category as the water-to-cement ratio. Gypsum, Portlandite, and unreacted cement phases ought to be avoided.

I'd be glad to answer some questions.

DR. CANTLON: Any questions from the Board? Staff? Audience?

MR. VERMA: Teek Verma from M&O/MKE.

I would like to make a comment and it relates to the material. Again, it is based on my trip to Soviet Union and looking into the technology they have. They use a combination of cement and clays, and the grout they come up with is in the form of a gel, and that remains gel forever. This is what they claim, and we have seen applications where they applied 15-16 years ago and it's still fairly pliable, and they showed us samples where they took some samples from it when it was applied, and there's not any change. We couldn't see any changes between the form when it was applied and how it was in in situ applications.

It also offers some obvious advantages over cement or other material. A, it doesn't swell or shrink. B, it doesn't lose its water content. Once the equilibrium is reached in the gel, it maintains its water content. And C is that it's very easy to apply because it could be injected. It has obvious advantages in terms of controlling the fracture permeability. You could well have fractures there or they are newly-created fractures. You could inject that grout and reduce the fracture permeability to almost next to nothing.

DR. HINKEBEIN: I guess I don't have enough knowledge of the material to comment on that, on the application to Yucca Mountain. I do have some concerns, and maybe I'll take them up with you after the meeting, but what I'd like to--I guess one of my main concerns would be in Soviet application, my presumption is that they're in a saturated environment and so it's very easy to maintain the gel in a saturated state, and it's in a medium where it's surrounded by a lot of water.

In an unsaturated environment where you can have some migration or loss of water as a consequence of the difference, the contrast between the medium and its surrounding, I guess I'd have some concerns, but it sounds like an interesting material.

MR. VERMA: I had asked that very question, because I was also thinking about its application to the Yucca Mountain and other environmental restoration-type of applications in unsaturated zones above the water table, and the response I got repeatedly, that as long as it's emplaced where relative humidity is 50 per cent or more, it maintains its water content.

MR. HINKEBEIN: Okay, so a very fine pore structure. What you're saying is it has a very fine pore structure, and that fine pore structure retains water.

MR. VERMA: Right.

DR. CANTLON: Other comments?

DR. DEERE: With respect to that particular application, I think the thermal environment here is something that is unique, also, that they probably don't have in Russia where they're using this.

MR. VERMA: That's true, but again, that question was also posed, and they said they have used this for a control of underground fires, and where the temperatures are fairly elevated after the fires were put out, and they went back and took samples to see how its structure or its consistency was, if any of those things were altered, and they say up to the temperatures that they had the data on, going up to as much 200° C, it didn't change. And again, these are their claims and we didn't look in the data that they had.

Thank you.

DR. CANTLON: Fine. Rather than going to the next speaker, let's take the break now. It's near the 3:05 schedule, and we'll take a ten-minute break. Let's kind of cut it a little shorter.

(Whereupon, a brief recess was taken.)

DR. CANTLON: We've got Joe back on if his voice is recharged.

MR. FERNANDEZ: The remaining presentations from now until tomorrow really relate to new work that's been done in the sealing program. It's new work, ongoing work. The next two major presentations deal with the development of strategies of, first, how to seal exploratory boreholes; and second, how to seal the underground facility shafts and ramps.

We've tried to temper our strategy development with an understanding of available technologies. Looking at the available technologies, reasonably available technologies is something that's stressed in the SCP under several information needs; in particular, I think it's Issue 4.4, and then Information Need 4.4.10 for the sealing program.

What I would like to do right now is talk about our approach to develop a strategy for sealing exploratory boreholes. The basis for this strategy is really derived from the 10 CFR 60.134 criteria, where boreholes should not be preferential pathways for release of radionuclides. Our assumption is to tie the performance to the rock properties. Again, understanding these rock properties becomes very important, the bulk rock properties. Our design goal is our judgment, basically. We're trying to restrict the flow through the boreholes to less than 1 per cent of the total flow through the rock. That's true for either airflow going up from the repository, or water flow going down to the saturated zone.

The approach to develop this strategy is, foremost, trying to define what the borehole system is. What is the physical appearance of this system? Can we learn anything from that to try to understand the location, the number, and a quantity of boreholes that we have to seal. The second is certainly critical. I will not be talking about defining the environment. We have done work in that area as far as what is the thermal environment that the seals will see, and John will be talking a lot more about that, particularly as that relates to borehole stability or to casing stability.

The second box here is establishing the significance of boreholes. I'll be talking about this in my presentation right now. We're looking first at air, and secondly at water, and trying to understand from the air standpoint the significance of the borehole considering dispersion up from the repository to the ground surface; and secondly, trying to understand the conductance of a column of rock and seal throughout the entire repository area, and what is the significance in that regard, the relative significance.

Underneath water, what I will present is a what would happen if you had a flooded drift. What would be the significance of the borehole with respect to a flooded drift in the repository; in particular, a drift at the edge of the repository, as in the perimeter drift; and secondly, to understand the significance of the boreholes with respect to their proximity to flood plains.

The presentation that John will make will address several other questions, the how, when, and where to seal exploratory boreholes. Later, Malcolm will review the available technologies for us, and then I'll present the final strategy as we currently see it developing. We still have a little bit more work to do, but we feel we're probably the majority of the way to developing the strategy. The reason it becomes very important is in the not-too-distant future, we'll be starting our site characterization program, and I think there may be some ideas that we might be able to pass on to those people associated with the site characterization program that would enhance the performance of the repository, just by some careful, perhaps drilling operations, things that we feel could decrease the performance of the repository. So it's a very opportune time for us to develop this strategy and to pass on our ideas to those people who actually will be involved with the site characterization program.

As I mentioned, the first very important thing to do is to understand what is the system we're dealing with, what is the physical system we're dealing with. Well, these are a list in the column to the left of all the various types of boreholes that we have or will have out at Yucca Mountain. We have unsaturated zone holes, water table holes, hydrology holes, geology holes, a series of other miscellaneous holes, paleozoic holes, unsaturated zone neutron holes, refraction holes, and seismic holes. They all vary with depth, and sometimes they vary from being very shallow to very deep, as in the case of the unsaturated zones; 57 feet to as much as almost 2,000 feet.

We have some very deep holes, as in the case of the geology and hydrology holes, down to 6,000 feet, as well as one or two paleozoic holes. Many of the neutron holes are very shallow. We have a lot of shallow holes, and one of the questions we're trying to address is, how significant is a shallow hole with respect to performance relative to one of the deeper holes that will penetrate right through the repository horizon, and we'll touch on that briefly.

The other thing I wanted to point out by this slide was the variation in the diameter of the boreholes. We go from anywhere from a 48-inch hole at the surface, to as small as a three-inch hole at depth. Typically, the construction of these boreholes, or if they're a very, very deep hole, will have a casing that's grouted in at the surface. Then we'll have a freestanding casing going down to some depth, perhaps 1500 feet or 2,000 feet, and then we'll have an open hole beneath that. In some other instances, there's no grouting at all, so we have a broad range of exploratory boreholes to deal with at our site.

The other point is they also have been drilled with various techniques, either with air foam, bentonite mud, polymer mud, and air, so we have not only differences in their physical characteristics, but also differences in the way in which they were drilled.

This is just a snapshot in time, perhaps nine months ago. These were a listing of all the boreholes. I have to explain the chart here a little bit. We have existing and proposed boreholes. We have categorization of the boreholes here. We have three columns underneath each one of these major columns, existing and proposed, and we have those within the repository boundary--and at the time we initiated the work, when I say the repository boundary, that reflects the SCP-CDR design, but there really wouldn't be too much difference. There would be some, but not much.

My point in illustrating this slide is just to show the number of holes that we have, the number that are proposed and the number of existing boreholes, and the categorization of these holes. You'll find out if you look at the types of holes over here, we have large numbers, but typically, those large numbers are associated with very shallow holes. For example, here's the neutron holes; 37 of those that will be outside of the repository boundary.

I meant to explain what this middle column was. When we did our airflow analysis, we had an extended boundary, which is just a little bit beyond the repository boundary, so that's what that refers to. It is really appropriate for discussion purposes, really, to talk about those within the repository boundary, and then the rest of them. So we have, up to this point, a considerable number of boreholes, and we potentially will have up to 400 boreholes out to some pretty broad limit outside of the repository boundary.

Again, a lot of the holes are shallow in nature. Here's the unsaturated zone neutron holes, 99 of those, basically, and then there's these LPRS holes, which in some cases are fairly deep but there are many shallow ones as well. So it just gives you an idea of the magnitude of exploratory boreholes that we have to consider in our strategy.

What are some of the conditions? These certainly are not all of the conditions in the exploratory boreholes, but I did want to present some of these. There's the presence of PVC tubing, screen, and casing in the boreholes that potentially would have to be removed in order to obtain a high quality seal. We have some eroded zones and some sloughing holes. You'll see that in some of the photographs that I'll show in a minute here. In some instances, there was notation in the drillers' logs of lost circulation in various portions of the exploratory boreholes.

There's uncemented steel casing in deep and shallow holes. There's steel casing, as I mentioned before, that is grouted at the surface. We have perforated cemented casing and uncemented casing. We have steel casing grouted at the bottom or at selected areas along the casing. Usually when we do see the really deep boreholes, typically they're only spot-grouted at the lower point. They're not grouted up and down all the way. And also, we have cement on the wall, so these are some of the conditions that we have to address.

In trying to get a better handle for the physical system, we felt it was worthwhile to take a look at a number of the video logs that were available for some of the boreholes. We decided to go ahead with taking nine boreholes, two to the north, some internal to the repository boundary, and some external to the repository boundary and try to come up with some categorization scheme that we could apply to the quality of these holes.

The categorization scheme, after looking at these nine boreholes or ten boreholes, and after looking at about 40 videotapes, there seemed to be a pattern developing. And I came up with this scheme of having four categories of hole condition. The first category was very excellent. Typically, the wall was smooth, had a very smooth surface. There was very few lithophysae cavities, and there were no fractures.

In Category 2, it typically was good. It was a smooth surface with small but consistently spaced lithophysae, and there were none to few fractures.

For Category 3, it was a poor hole quality, typically very rough surface, intermediate lithophysae, frequent fractures, but the hole, it was enlarged, but it usually was symmetrical.

The worst category that I noticed on the film was extremely poor category; rough, very irregular surface, large lithophysae cavities, many fractures, and the hole was enlarged and non-symmetrical. In some cases, there were very large voids, ten-foot voids for example, that would be several feet in diameter along the axis of the exploratory borehole.

Just going back for a second, when I say intermediate lithophysae, that would be those lithophysae that would be on the order of half of the diameter of the borehole. Large lithophysae would be those that would be greater than half the diameter of the borehole, and sometimes they would consume the entire borehole.

I'd like to show some of the photographs from the video logs. These are, unfortunately, very poor quality because the video log, when we look at it--and it has a number of frames per second--we don't realize how poor the quality is for each frame. Given that concern, as well as when we're out in the field there's electrical storms running off of a portable generator, and you don't always have the consistent electrical supply that you would like to have. So these are absolutely the best that I was able to get, and I think they do illustrate some points that I would like to show.

You have, I think, five pictures of these in your packet there. I would like to go through three of those because they're really the most telling of those, and I'd like to summarize all of the 40 or so videotapes that I reviewed.

The first one is taken out of G-4, which is close to the old location of exploratory shafts. We're down about 60 feet. We're in the Tiva Canyon member, and this is a densely welded, devitrified tuff. You can see there's large hole irregularities. It's not very symmetrical. You can't see some of the fractures here, but there are some fracture sets that run through here. I would categorize that as a C-4, Category 4, very poor quality.

Conversely, this UZ-6S, which is up on the crest of the mountain, it's marginally a Category 4, but the reason I gave it a Category 3 was because, in general, it was symmetrical. It could be debated that it wasn't. This is certainly a qualitative categorization scheme that we're using. This was, again, a Tiva Canyon member, a densely welded, devitrified tuff, approximately 150 feet down.

On the second view graph, we're in the Yucca Mountain member. This is in the Paintbrush non-welded tuff. We're in partially welded, non-welded, vitric. We're approximately 80 feet down and we're in UZ-1, and you can see this category I would classify as a Category 1 borehole as far as the hole condition.

This is also in UZ-1, down at 95 feet. It's a bedded tuff below the Yucca Mountain member, with several inclusions, as you can see; the dark spots around the hole. This is two of the piezometers that go down to the lower portion of the hole that would eventually have to be removed for sealing purposes.

The third and final view graph I would like to show here are in the Topopah Spring member. Here's a Topopah Spring member, the caprock, the upper portion of the Topopah Spring member in UZ-6, close to UZ-6S; densely welded, devitrified tuff, approximately 520 feet down. I would classify this as a C-4 category mainly because of some sort of abnormal structure that runs through here. It's also quite enlarged, and you can see that some of these pockets here are greater than the diameter of the borehole. The diameter of many of these boreholes that we're looking at are around nine to twelve inches in diameter, and some of the more dramatic openings, unfortunately, the quality was so poor that I couldn't show those in this fashion.

Again, this is in the Topopah Spring member, densely welded, devitrified tuff, a little further down in the stratigraphic column in UZ-6. This is 850 feet down. I would classify this as a C-4. You can see a major fracture running down the length of the borehole, a high-angle fracture. You can see part of it spalled off here from basically somewhere around here where the borehole was, down to this surface and coming on up.

The results of this study are summarized on this slide. In the densely welded, devitrified tuff in the Tiva Canyon and Topopah Spring, there was comparatively a high percentage of Category 3 and Category 4 boreholes. We're concerned because of these differences in the hole quality because we don't know how the grout or the sealing material will be accepted in these regions. If they are, in fact, highly fractured, very irregular, we may end up pumping an awful lot of material--if it's a cementitious material or grout--into these holes in order to obtain a better quality seal, so we're concerned from that standpoint.

In the second bullet, the Paintbrush non-welded tuff, typically it's a Category 1. This included the majority of Yucca Mountain member and the Pah Canyon member. The upper portion of Topopah Spring typically is Category 1 and 2. We go through a transition phase as we get underneath the Paintbrush non-welded tuff, and we experience some fairly good quality rock going down into the much larger lithophysae cavities of the TSW-1 and TSW-2, TSW-2 being where the repository potentially would be located.

And the Calico Hills non-welded, vitric and zeolitic unit typically is Category 1 and 2, very high quality holes. So what we see here is generally the "softer" material, the low strength material seems to behave more favorably to the drilling operation. Where we have a densely welded material, highly fractured, you go in there with percussion, rotary percussion or some sort of a drilling operation that tends to beat up the rock a little bit more, it responds in a like fashion. It just starts to fall apart. It's also indicative of the highly fractured of the material we're dealing with.

I'd like to move into the second part of this particular presentation. We've defined our physical system. The next question is, can we use a common strategy to seal the exploratory boreholes? In order to get a better feeling for that, we developed some very simplistic models on the CALMA system we have at Sandia. We looked at the thickness models. How does the thickness down to the repository horizon, how does that vary over the entire repository area? Do we see unusual areas where it's particularly thin or particularly thick?

We looked at travel time models, again using those same models that we've been working with before, varying the properties for the welded and non-welded materials over several orders of magnitude, and we combined those to try to maximize travel time and minimize travel time, and say, do we see any preferential travel times within this particular area over the repository.

The third is to take a look at the conductance model, to look at the equivalent vertical hydraulic or air conductivity above the repository to see, again, if we see any preferential conductance that might occur above the repository.

After looking at these three very simple models, we concluded that there isn't really a large variability in these models, and that led us to focus in developing a common strategy for sealing exploratory boreholes, acknowledging that there are variations of the rock properties. We're making the assumption here that usually in the Topopah Spring we would expect a certain hydraulic conductivity. In the Paintbrush tuff non-welded units, vitric, we would expect the same overall hydraulic properties. If we have variations within a unit, then there may be some other considerations that we would have to refine as far as this particular strategy. But we felt that there's little variability in the model, provided we stuck with one set of properties for a non-welded or welded tuff.

The next set of view graphs here deal with trying to answer the question of the significance of the boreholes. As I mentioned before, there's four questions that we wanted to answer. One is how significant are these boreholes in an absolute sense, and also in a comparative sense, with one another? The other three questions is how, when, and where do we seal these particular boreholes? And again, John will be talking about that.

But for the first question, the significance of boreholes, we wanted to look at it from a air dispersion up from the repository, and looking at it from a flooding; if, in fact, we were to flood a drift, how far out would the potentially contaminated water go, potentially intercept a borehole and then go down to the groundwater. Likewise, there was concern for air dispersion. We have, in the repository horizon here, we have some migration of radionuclides upward. Is it a possibility, as this were to go upward, to intercept a borehole and then be preferentially leaked through that borehole?

We have a second set of calculations, comparative-type of calculations for both airflow and flooding, and I'll go into each one of these in a little bit more detail.

I do want to add that these weren't the only calculations that we did; either those that were done in order to address this problem, or those that were done for design purposes. We're trying to present the ones that we feel are the most significant. In the final report, we'll address some other considerations in the area of design or in the area of significance of the boreholes.

This is a schematic of the first problem that we were looking at. Here is the repository, this stippled area here. Here is a deep borehole and a shallow borehole. What we're trying to do is observe--we were trying to model in a very simple model what would happen if we had a gaseous release of radionuclides from the repository. How far out would those gases be dispersed from the edge of the repository? We're trying to understand the magnitude of the problem. Do we seal all of the boreholes out 20 miles, or do we seal only those boreholes that are fairly close to the repository? Well, how close is fairly close? That's the question that we're trying to answer with these simple calculations. And again, we looked at a three-layer model here.

We solved the advection/dispersion calculation, assuming a constant source at the repository horizon of a contaminant, and we assumed no absorption of that contaminated in any form as it was migrating up. We also, recognizing the effects of anisotropy, we tried to consider if we would have preferential leakage to the north and south and to the east and west. We varied our dispersivity. We had two terms. We had longitudinal dispersivity and transverse dispersivity.

The longitudinal, if this was the repository horizon like this, the longitudinal was considered to be in this direction, and the transverse was considered to be parallel to the repository horizon. This was to mimic, in some sense, the gas going up perhaps being communicated or going through a fracture, and then either going up or going to the side. Well, we varied our properties here, and we recognized that these properties really are taken from groundwater transport books. They represent some of the information that's been presented in several different large scale studies. These were large-scale field tests; came up with dispersivities on this order of 100 meters, and some actually are a little bit higher than that, but from what we saw in groundwater contamination books, typically, it was around 100 meters.

And the transverse dispersivity could be anywhere from 1/20 to 1/5 of the longitudinal dispersivity. What this says, basically, is as the contaminant would move up, it would have more tendency to move upward as opposed to perpendicular to its path. So we would essentially minimize the lateral migration away from the repository boundary.

Nonetheless, we didn't use that. We did evaluate what would happen if we assumed those more realistic case, but for the purpose of coming up with the significance of boreholes for this particular release mechanism, we assumed that the longitudinal and transverse dispersivity were the same, so we were trying to maximize the lateral migration of this contaminant plume.

What this shows is, this is the centerline of the repository. This is the concentration relative to the initial concentration being released at the repository horizon. This little dashed line that goes up is the edge of the repository. What we were trying to do is find out how far or how fast that would taper off to a very low concentration, given a long period of time. And here we see this plume after 2,000 years, eventually reaching fairly close to a full concentration of what the original concentration is down here, but we also see a trailing off. So the concentration out 600 meters is fairly negligible from the edge of the repository, which would suggest by this model that you would get a very small amount of radionuclides being communicated from the repository horizon up through the rock, and then intercepting a borehole, and then out through the borehole.

DR. DEERE: Wouldn't that vary from the east boundary to the west boundary? I mean, near Solitario Canyon, for instance, where you have a lot more of your Topopah Spring exposed, which is overlaid by your less permeable unit.

MR. FERNANDEZ: I think it would vary. This was, again, a simplistic model. I think if you look to the north and if you had a more detailed model, you might expect as, let's say, the radionuclides reached the alluvium, for example, that this assumption here might be actually more valid. You would have the longitudinal and transverse dispersivity to be more or less equal, a very isotropic type of media. It was meant to give us just a first order cut, and there might be variations, as you're pointing out, in the broadening of this plume or the narrowing of this plume.

We basically assumed that since the fracture sets are north, northeast to southwest and northwest to southeast, that we would have more or less a fracture-controlled system, or airflow controlled system that would concentrate more flow going to the north and to the south, and that was our basic assumption, and that's as far as we've gone with the analysis.

The second calculation that we did was how far does the water actually migrate out from the repository drift if, in fact, it was fully saturated? What we've tried to do here is take a look at the dip of the fractures, trying to amplify this lateral spreading. The predominant dip of the fractures are vertical, but there are a smaller component that have a very low dip, on the order of 10 to 15, 20°. In the reference information base, they give the statistics on that.

What we tried to do is to maximize this water flow by assuming that the majority of the low-angle dip fractures, the most that we could assume as far as a standard deviation, we maximized the low dip fractures and then we minimized those vertical fractures so that we had fewer vertical fractures taking the mean, minus the one standard deviation, and then we took the low-angle fractures, taking the mean plus the one standard deviation, so we're trying to enhance the lateral flow out.

When we did that in our analysis, normalizing this to a three-dimensional or two-dimensional tensor, permeability tensor, we ended up with the results of this angle--or maybe that's on the next slide. I've probably gotten a little bit ahead of myself. Again, we maximized the frequency of the low-angle fractures, minimized the frequency of the high-angle fractures. The result that we had is the maximum extent out from the edge of the repository was 20° from vertical, so that if our drift is represented here by my hand, the angle between the vertical, this angle here and this angle here, is the predominant flow vector for that flow, 20° out.

If we were to take that 20° out and then find out where the water table is down here, that would give us kind of a halo around the repository for how far out radionuclides might be of concern, and those boreholes outside of that halo would be of lesser significance than those inside that halo.

We also looked at the significance of airflow relative to the borehole itself and a series of boreholes. We have, as I mentioned before, very deep boreholes and very shallow boreholes. What we did in this particular analysis, we have some shallow boreholes, and as the air migrates up we have--the airflow first would occur through some rock web or a column of air going up, and then it would hit the seal, and then it would go through the seal up to the surface. On the deep boreholes, the airflow would occur through a backfilled hole all the way up to the surface. So if here's the repository, the borehole has potentially a barrier pillar around it. We assume that it would go right up through the seal, from the repository, through the borehole seal up to the surface.

We used the equivalent of vertical hydraulic conductivity or air conductivity relationship, which is shown by this third bullet here, where this is the equivalent conductivity. This is the thickness for the first unit that we're going up, the second unit that we're going up--the geologic unit--the third unit that we're going up, and then if it goes through any thickness of the seal itself. And then we have the corresponding permeabilities and hydraulic conductivities for each one of these zones. Again, the design goal was to select conductivities to restrict the seal flow to one per cent of the total flow.

What resulted from this calculation, in our access here we have the flow versus the borehole length. Here we have the flow on this access, and we have the borehole length shown as a function of log. So here we're looking at holes that are 100 feet in depth, and here we have some that are some of the deeper boreholes.

What this shows is that the shallow boreholes which are represented by this cluster here and, actually, some perhaps even over here, the shallow boreholes have a very low comparative flow. It's on the order of--and I don't know exactly what these units are. I'd have to go back, but something to the $^{-8}$.

For these deep boreholes, assuming that the air would flow basically through the seal itself, we have a fairly high comparative flow rate. We have five orders of magnitude difference in this particular scenario between the shallow boreholes and the deep boreholes.

The last calculation that we did was to look at the significance of borehole relative to flooding. How many of these boreholes were actually located within a wash where they potentially could be flooded? That was our primary concern, flooded from some sort of a surface runoff event.

We had done a standard type of flooding analysis. We looked at the drainage basins, which are shown basically by this dark stippled area. We have plotted on where all the borehole locations would be, the existing and proposed borehole locations. We plotted on where the alluvium would be, and also the drain pattern, drainage channels, and then determined whether or not those boreholes were significant with respect to this particular scenario.

What we had concluded from the flooding analysis was that the existing holes can be divided into three categories. We have boreholes within very broad alluvial terraces containing incised channels. They are subject to flooding. We made some judgment call here. We recognize that in the future these incised channels will meander from side to side in the alluvium, so in our judgment, there were some that certainly would not be subjected to flooding today but would be subjected to flooding, potentially, in the future due to geomorphic events.

Boreholes within the steep areas are much less likely to flood, and there are another category of boreholes that are just clearly out of the flood plain, the flood zones, that are not subject to flooding. The proposed boreholes are not subject to flooding, was one conclusion from the analysis.

Deep boreholes are potentially far more significant in enhancing the water flow. Now, we say this because if we didn't do anything to the exploratory boreholes--not that we're going to leave them open. We certainly will not. We'll have to comply with the state regulations at a bare minimum, but if we were to do nothing to the boreholes and let it flood up, you'd find out that the rate or the quantity of water that can drain from the boreholes, assuming that it was potentially flooded for a deep borehole, is not a linear relationship.

For example, if you had a borehole that was ten feet in depth and was fully flooded, and it had 100 cubic meters of water leaving it, and if you take one that was 20 feet in depth, it would not be 200 cubic meters of water, it'd be much higher. So it's a non-linear relationship of the amount of water that we release from the boreholes. The deeper you are, the greater would be the release.

If we were, however, to place a simple sand material in the borehole--just as an illustration--we would be able to restrict the amount of water that would enter into the borehole to a matter of tens of feet, just by a simple sand backfill if you consider some of the Green and Ampt solutions that we had looked at before.

The summary from these performance calculations are shown on this view graph. Again, we were looking at two areas; four calculations, two areas. What is the limit of significance of the boreholes? For airflow, based on a simple air dispersion model, it appeared that the concentration over a long period of time of air being released from the repository, the concentration would be at fairly low levels if we were out approximately 600 meters from the edge of the repository. We feel, considering the simplifying assumptions, that if we were to go to the eastern and western edge of the repository, it would be perhaps more restrictive from the release primarily because of the direction of the predominant set of fractures in the mountain.

Water flow, I stated this already, that in order to maximize the lateral extent, we maximized low-angle fractures, minimized the high-angle fractures, and we found out that that was 20° from the vertical and away from the repository edge.

The significance of the boreholes, the airflow, the shallow boreholes are less significant by five to six orders of magnitude. This would suggest that in our site characterization program, the shallow boreholes are much less likely to create a problem from a performance standpoint than the deeper boreholes that would go through the repository horizon. Now, that's kind of an intuitive relationship or conclusion that you could make, but we just did the numerical analysis or the calculations to prove, in fact, that there was a broad difference here that we're looking at.

And finally, water flow, deep boreholes are potentially more significant than shallow boreholes, and also, there is a limited number of boreholes of concern. We don't have a whole lot, including both the proposed and the existing, but there are some that we feel are definitely more significant than the others, and with that, I'll take any questions.

DR. CANTLON: Okay. Questions from the Board?

I have one. You project that 20° angle down as sort of the curtain of flow of water down through the system, but you have bedding plains there that are much more porous than others, so you're not going to have a 20°, you're going to have a very jagged line.

MR. FERNANDEZ: You very well might, and it was also pointed out, because the dip of the bed is to the east, you might actually have a little bit of an enlarged halo to the east, and that's something in one of the other presentations that I'll make, the conclusions for the results for sealing exploratory boreholes. One of the first calculations we did was acknowledging the fact that we might have some sort of abnormal shape where the abnormal shape would be primarily either to the northeast, the east, or to the southeast because of the dip of the beds.

So we recognize that there are potential abnormalities in the system that might change that shape a little bit, and how much, we really don't know right now.

DR. CANTLON: Staff questions? Audience?

(No audible response.)

DR. CANTLON: The next speaker, then, John Case, selected design calculations.

MR. CASE: My next presentation is to present some selected design calculations that support some of the conclusions or strategies that we have developed for sealing exploratory boreholes.

The outline of my presentation is, first of all I am going to look at evaluating the stability of open borehole near an excavation. I am looking essentially at the near-field effects. We have a borehole that is penetrating near an entry. We are asking the question, what is the stability of this borehole, not only at the time that the excavation is created, but also during repository heating.

Then I am going to move on and look at evaluating a buckling of cased boreholes near the repository, some of the deep boreholes that go down are cased, and it is of interest to look at potential stability problems that might develop within those casings.

I am going to briefly touch upon casing corrosion as to whether this is an issue of concern with respect to developing an overall strategy. Then, finally what I would like to do is present structural hydration calculations.

The objectives of our open borehole analysis was to first of all to establish what we think the state of stress would be on a borehole surfaced near an excavation. We would like to sort of establish the minimum distance that we should have separating a borehole from an underground excavation, and thirdly we would like to look at some issues with the respect to the placement, the removal of the casing and the placement of the seal.

This diagram here just briefly shows the geometry of concern here. We have a tunnel right here (indicating), we have a heat source for vertical emplacement. We have a borehole at some distance and we have selected this distance here to be approximately ten meters. We can identify different levels here, the floor level which is near the heat source, the sidewall level and roof level. If we take a look of the borehole and plan, if it is an open borehole, then we can look at points A and B and we can use the Kirsch Solution to calculate stresses on that borehole.

What we did is we performed these calculations using the Kirsch Solution, and I might add, one assumption that we made was, that we took the far-field stresses that developed around the entry and applied those as far-field stresses for the borehole calculation. So we are assuming that there isn't much effect in terms of the borehole affecting stresses around the excavation because of its size.

What we did is basically calculate the state of stress and we did this at various times. We did it before waste emplacement, after ten years after waste emplacement, and then we looked at the issue of 100 years after waste emplacement. So this is basically a near-field analysis that we've done.

The state of stress given by the Kirsch Solution can be represented by these Mohr circle plots right here (indicating). We can see as heating occurs, then we have an increase in the state of stress, the shear stress that would exist at points along the borehole. We can then take and compare these states of stress to the strength to get some idea with respect to how stable that we think that that borehole might be. This is using a strength criterion at the Topopah Springs repository level that was similar to what we developed previously before I showed a triaxial compressive strength curve. Here I am showing a Mohr-Coulomb type plot.

What we note here is relatively low amounts of shear stress at the time of emplacement. After ten years it builds up fairly rapidly, and we see that the circle would be intersecting the failure envelope, and after 100 years, it is quite substantial.

DR. DEERE: That outer circle is 100 years?

MR. CASE: That is correct, yes.

Some of the preliminary results of these near-field analyses are that open boreholes appear to be stable for medium to high strength rock at ambient temperature. Open boreholes undergo plastic deformation for low strength rock at ambient temperature. That seems to be supported by some of the video pictures that we saw.

Heating significantly elevates stress because of the proximity of the borehole in these calculations relative to the heat source, and that open boreholes would undergo a plastic deformation, would possibly fail a short period of time after waste emplacement.

Some of the conclusions that we might draw from here; the first one is somewhat intuitively obvious. I guess we have had a number of calculations which would suggest this. We should not try to locate seals near the repository horizon. If we have a choice, let's locate them outside the repository horizon.

There is a potential for borehole instability at the repository horizon. This may require early sealing prior to waste emplacement. And for the lower seals in the Calico Hills, it certainly is important to backfill concurrent with or prior to waste emplacement.

Let me move onto the casing stability calculations that we did. This shows the basic geometry that we have. We have looked at different potential locations. One is up near the contact of the Paintbrush Member with the first unit, the TSW-1. Then we looked at the repository horizon and then we looked at the Calico Hills. There are many holes that are cased that go through the repository horizon like this. It just shows in planned view just the geometry of the casing. Basically we have in planned view, some thickness here and some external diameter.

The concern that we have is the buckling of the casing due to heating. The objective of these analyses would be to look at potential casing instability. The technical approach that we have done, used in these calculations, is that first of all assume that the formation contacts the casing, so the casing is performing its intended function in these calculations. We then calculate increases in thermal stresses due to repository heating that are averaged in the plan. We are assuming that the steel casing is comprised of J55 steel or H40 steel and we use standard elastic and plastic buckling formulas to calculate the buckling stress.

We've looked again at issues of repository heating and I show here basically some temperature histories that were developed for particular borehole H-5 hydrology hole, which is sort of on the eastern side of the repository. Basically, of course, our concern in removal of the casing is to look at time periods of perhaps ten up to 60 years. We are not looking at the longer term picture with respect to removal of casing in these calculations. But, basically, we have done calculations that show what the temperature histories are at the various sealing locations. And we note that the repository horizon is highest; the Calico Hills is less high and the upper contact PTn contact TSw-1 contact is lower.

Then on the basis of using those temperatures, doing a thermal stress analysis, taking into account the interaction of the casing with the surrounding formation, we then can calculate a stress. We can then take that stress and we can compare it with the theoretical buckling stress to see if we think that there may be any potential instability that may develop. This figure here illustrates our casing stability or buckling calculations. Basically what we have here is a theoretical buckling curve, it could be elastic or plastic buckling, that shows as the external pressure increases, the slenderness ratio which is the ratio of the external diameter to the thickness of the casing is reduced. What we have here is, I show a borehole, USW G-4; we have calculated the slenderness ratio for that as approximately 27. And we have done these calculations at the PTn, TSw-1 contact, TSw-2 and CHn-1. And as you can see for the shallow calculations for the unheated case, there appears to be considerable design margin. As we go deeper, the repository horizon there appears to be still a considerable design margin. As we get down to the lower units our calculations would show that there is less design margin for the deeper holes.

If we take into account the effects of heating, what will happen is is that it will develop a higher compression, thermal stress, which will have a tendency to increase the external pressure on the casing and we see this for USW G-4 which is in the center or near the center of the repository. So, we see at the repository horizon that there is a high, a large change in external pressure. At the Calico Hills, we see a smaller change. I should also add that these calculations are done after sixty years, that is the time period we looked at. And, interestingly enough, for the upper contact zone, we see a reduction in external pressure, and that is due to the fact that as heating occurs we develop a high compression zone near the repository and a slight tensile zone that develops away from the repository as can be shown by closed-formed solutions, I think, that goes back as early as Neville Cook and some calculations that he did for the STRIPA project.

If we consider USW G-3, which is outside of the repository to the south, and we look at temperature effects after 60 years, we see no temperature effects. So again, this is pointing to the conclusion that boreholes that are outside of the repository some distance away from the heat sources may not be seriously affected, but those that are within the repository may very well be affected.

Basically the conclusions here is, casing at sealing locations, the upper PTn are not expected to collapse. Cased boreholes within the repository interior, such as USW H-5 and G-4, are marginally stable during repository heating. Cased holes outside the repository boundary appear to be stable.

Let me briefly talk about corrosion. With respect to corrosion, one of the issues, of course, would be whether the formation contacts the steel casing. If we have no contact, we have an annulus of air, then we have something that we have atmospheric corrosion which would be appropriate. If the formation is in contact with the steel casing, then we might have soil/rock corrosion.

In the case of atmospheric corrosion, the composition and humidity of the air would be important in the free standing column. In the case of soil/rock corrosion, the host rock resistivity, ground-water chemistry and drainage may all affect corrosion. We can envision either local cell corrosion where anodic and cathodic regions are close to one another and we develop sort of a general corrosion. Or, we may have long cell action where the anodic and cathodic regions are separated by some macroscopic distance, and the host rock resistivity would be significant in affecting the potential corrosion that would occur. Again, one of the things is we are not aware of any site-specific metallurgical examinations. We are not aware that anybody has taken casing, pulled it out, done a metallurgical evaluation of this issue.

If we look at just some general corrosion rates, if we have atmospheric corrosion, we expect possibly a uniform corrosion of 1-7 mils a year. If we have soil and rock corrosion, again using values that are reported in the literature, we could have rates that go from 5 to 100 mils per year.

One of the things that we should point out here is we could have some synergistic effects in terms of corrosion. We could have the formation collapse on the casing; we could have stress corrosion occurring; we could have potentially staining, pitting and then failure; that is something that would need to be looked at.

We don't know the existence of collapsed zones. If we took the expected rock mass strength, we might expect those to be fairly isolated, but we do expect them to occur in some areas. And again, finally, I think we could gain a lot by just performing the metallurgical examination of the casing.

For the next part of my presentation, I would like to present some structural calculations. Most of what I am going to say is going to be structural hydration calculations, although I will touch upon other kinds of calculations here.

DR. DEERE: Excuse me. Perhaps we could have questions now on the first part because now you are changing into another type of analysis.

MR. CASE: Sure.

DR. DEERE: With your collapse, it is not a following force, in other words, once you go into a buckling, wouldn't that be self-limiting, because as soon as it deforms, I know there is elastic energy stored there, but it is not sufficient to buckle everything is it? Doesn't it just start to buckle and then since you don't have hydraulic pressure which can follow, it will just stop because it is no longer in contact with the rock.

MR. CASE: It is possible that that would happen. I guess the question is, if you do have--you know, if you look at most buckling phenomena, what typically happens is that as your loads increase it goes up and then your displacements become highly non-determinant with respect to load.

DR. DEERE: Because you have a following load, that load never disappears.

MR. CASE: Right.

DR. DEERE: If the rock squeezes in, the load is gone.

MR. CASE: Well, I would have to think about that. I am not so sure I would believe that. There may be some effect like that. It depends upon the condition of the formation at the place at which the buckling may potentially occur.

I think what you may be trying to say is that you may have some effect where loads are redistributed to other parts of the casing and it may be self-limiting. Again, I think we had done these calculations to come up with a conservative analysis, saying if there was some deformation of the casing that potentially could occur at some location, we may have some difficulty in removing that casing. And if we can't remove that casing, then we may have some serious difficulties with respect to sealing particularly in the areas of the Calico Hills or the deep boreholes if the casing were to be lost.

So, to answer your question, the calculations were done to conservatively evaluate some of these effects in terms of developing a sealing strategy.

DR. DEERE: I think certainly my experience has shown that when you have hydraulic loads that are following loads you get deformation if the load is still there.

MR. CASE: Right.

DR. DEERE: This can really take it right straight down to where that thing collapses completely.

MR. CASE: Yes.

DR. DEERE: But if we are going to have a load, and I have seen it in a hydraulic structure, where the hydraulic head varies as you go down a particular pipe, and when the buckling starts why that progresses a certain distance and gets smaller and smaller and smaller, and at the far end you have nothing, because the load doesn't follow and the load was never that high.

MR. CASE: Yes.

Well I would have to say that we are looking at this from the standpoint of a conservative analysis and we haven't fully evaluated all the load transfer occurrences that may be of some significances. Particularly with thermal loads, that is a different type of load than a mechanical load. Like in nuclear containment structures, if you have high thermal loads that develop, you could have thermal cracking which tends to relieve those high thermal stresses. Those are some effects that may be of some potential significance here with respect to how loads are redistributed.

DR. DEERE: I think you are right. The statement you made a little while ago, that if it does start to buckle then you are transferring either to the part that doesn't buckle up and down, or you are transferring it to all the rock around. Either that rock is able to hold it or it starts to fail and comes back in contact again.

MR. CASE: Yes.

DR. DEERE: Anyway, as you say it is probably going to stick the casing.

MR. CASE: Right.

MR. MCFARLAND: Question.

MR. CASE: Yes.

MR. MCFARLAND: In going back and looking at the concern, the concern is stated that the removal of buckled casing is difficult. Now we are looking at buckling of the casing, because if they buckle, it would be difficult. Now you are talking 56 years hence as you stated. I am having difficulty putting the concern to the problem. What is the problem we are trying to solve?

MR. CASE: Well the concern would be that let's say that we start, what is our schedule, what is the sequencing of events here? We go in, we develop the repository; develop the mains; develop the rooms; we emplace the waste; the temperatures build up within the repository. We have thermal calculations that have looked at this from a general rock mechanics point of view.

Now the point is that as loads builds up, what effects do we have of that heating on cased boreholes. I think the issue here is that we may have potentially--if for example let's assume that the borehole is open, there is an annulus of air that exists between the casing and the surrounding borehole near the repository horizon.

MR. MCFARLAND: This is after the repository has been commissioned, it has been filled, it has been back-filled, and decommissioned?

MR. CASE: Well the waste has been emplaced and temperatures are building up within the rock mass. So the issue is is that first of all could that open borehole fail? And I think the answer to that is that clearly it could, given the simple comparisons of strength to stress that we have made in these calculations.

MR. MCFARLAND: Now failure being a collapse?

MR. CASE: In the case of an open borehole, yes, collapse of the borehole around the casing.

MR. MCFARLAND: And what would be the impact of that collapse on the safety to the repository?

MR. CASE: Well I think the issue is how difficult would it be to seal, to remove that buckled casing, particularly, if we had to place a seal say below the repository in the Calico Hills, from the standpoint of hydrologic performance.

MR. MCFARLAND: But this is after the repository is decommissioned?

MR. CASE: That is correct, yes.

MR. MCFARLAND: Do you expect you wouldn't seal these holes before you would even start filling the repository?

MR. CASE: I think what our calculations are hoping to show is the importance of a strategy that says that we will seal those boreholes prior to the time that we have emplaced the waste. You know, the issues with respect to sealing design are not way out there in the future at some point in time. They are issues that occur with respect to the now. I think that is what we are attempting to show here with respect to these calculations.

DR. CANTLON: The message essentially is, get on with the sealing of the boreholes before you run into these problems?

MR. CASE: That's correct, yes.

MR. MCFARLAND: I understand. Thank you.

MR. CASE: Okay.

MR. FERNANDEZ: Just one quick comment if I may, Joe Fernandez. I think the other concern may be in the time framing that we are looking at here; John mentioned 60 years. I think there is a very rapid heat up of the rock once the waste is in place. It is that heating up of the rock that may affect the stability of the borehole itself if it is open or the casing of the borehole that really are concerned about.   And if it makes sense, if we think we are going to have problems, why wait until, until as Jon White had shown that little seal installation diagram, why wait until we may have a real, real problem of sealing on our hands. I think we should do it now rather than later.

MR. MCFARLAND: I think the block reaches maximum temperature at 800 to 1,000 years out.

MR. BLAIR: John Blair. I don't know if this is feasible or not, but what about reaming out the casing, just going ahead and drilling it all out and removing it that way and getting it all down, ream the hole out and get back and seal it, and don't even worry about--

MR. CASE: Well one of the things that we are going to address here is we are talking about timing here which is the when, we are trying to address the when. The next presentation by Malcom Jarrell is going to discuss some of the technologies that are available for cutting of casing, cleaning off of the hole, removing junk from the hole and issues with respect to seal emplacement.

MR. JARRELL: Yes. If you will please hold that question until the next presentation, I will address it.

MR. BLAIR: Okay. Thank you.

MR. CASE: The next part of my presentation will be to look at structural hydration affects in seals. And the reason why we are doing these calculations is if we look at the Terra Tech test that was done in basalt, large scale triaxial test, there was information that is presented in the site-characterization plan that shows that the interface of that seal, of a seal will behave like a fracture. In other words at low effective stress, we can have a high equivalent fracture aperture. If we increase the stress across that interface zone, we can reduce significantly the potential flow that occurs through the interface. So it is of some interest to see, to look, to evaluate situations which would result in developing high interface stress in cementitious materials.

What I am going to do is look at and present some structural hydration calculations, looking at different materials, different placement temperatures, and we have also done these calculations at different seal locations.

Just to conceptually show you what some of the macroscopic effects are in a seal, we start out, we will have initial mixing emplacement of the seal. And then the seal will be emplaced and the hydration reactions will start to occur and they are exothermic. We are developing heat. We may also develop expansivity depending upon on what is comprised with the composition of the cement, so we will have volumetric and thermal expansion.

Then as the hydration reaches, it never reaches entire completion, but it slows down I guess would be the proper way of saying it, we also will have thermal diffusion that is occurring out into the surrounding formation and we will have thermal contraction during cooling.

Another issue that we my have is primary creep in the sealing material that Tom Hinkebein briefly touched upon that would affect the interface stress. Then after that we would go in and we would backfill the borehole and thermal stresses would develop because of the post-closure repository heating environment and the interface stress might then increase.

There are other loads that may be of some significance with respect to sealing. Those might be seismic loads that might develop on the seal. There may also be the potential for perched water that would result in potentially high stresses on the seal.

Our current calculations are addressing some of those issues, although I am not going to say too much about that today.

The shaft seal analysis, we developed a computer program and basically it goes back to the ONWI sealing days, that looked at both thermal and thermal-mechanical affects that evolve within seals. Basically, we got the concepts for developing this model from some of the pipe experiments that were done at waterways experiment station and from other information. And basically, the shaft seal model takes the surrounding rock temperature, the emplacement temperature, the heat of hydration, and it calculates temperatures within the plug and the rock as a function of time.

Then, the temperatures are input into a second sub-program which performs the thermal stress analysis and what it does is it looks at Young's Modulus of the plug in the rock as they evolve; looks at unrestrained volumetric expansion, if we have an expansive cement; it applies the radial displacement compatibility and condition of static equilibrium at the interface zone. From this then we can calculate stresses within the plug and the rock.

Some of the parametric studies that we have done is to look at different sealing locations. We have looked at different sealing materials, a mortar with a type case cement which is expansion, a mortar with a type 2 cement which is not very expansive. And then we've also looked at issues with respect to placement temperature. In other words, below the ground surface, we may have a high ambient rock temperature. We might place a seal at that temperature. And then one of the things that we have found that might be advantageous would be to lower the placement temperature and I am going to present some results of what the effects of lowering the placement temperature to a value of 4 degrees centigrade would do.

This presents here, both a thermal and the temperature distribution in the plug and then the stress distribution. And fortunately I am going to show the temperature distribution first. Basically, on a plotting radius a temperature distribution at various times, after one day, three days, seven days, twenty-eight days, sixty days. What we see here is the temperatures within the plug right here, this is the plug rock interface rise, initially up to approximately 40 degrees centigrade. And then as hydration slows down and heat is diffused to the surrounding rock mass, then we see that the temperature gradients are reduced.

From that we can calculate what the development of interface stress would be within both the plug and the surrounding rock, and what I show here as the radial and tangential stress distributions at seven days and then 60 days. And we see some slight increase in interface stress from 5 up to 5.5 MPa. So most of what is happening is happening within the first few days after the hydration has started.

One of the things that we were interested in looking at here was the influence of placement temperature. We have also looked at the size of the plugs. In other words if we have a larger plug, we have essentially a much larger volume to surface area that affects the heat conduction through the plug and we have also looked at the effects of placement temperature which are shown right here. What I am showing here is the temperature of the plug near the center of the plug as a function of time. And in once case, we ran an analysis where we started out a temperature of 22 degrees, within several days, the temperature rises again up to 40 degrees, levels off and then starts to fall back to the ambient temperature.

We ran a second analysis where we lowered the placement temperature down to 4 degrees centigrade, so we started out at one day 4 degrees centigrade, we have a rapid change, a rapid rise in the temperature of the plug. But notice how the peak, the temperature here of that plug is lower than the previous case. The calculations then look at the effects at that lower placement temperature in terms of developing interface stress.

In the case of ambient temperature we see that the interface stress as a function of time again goes up, but then during the thermal expansion, but then as we have thermal contraction, there could develop according to the model and the assumptions made in the model, some potential tension that would develop at the interface zone. Because, as we are heating up the plug, it is in a more plastic state, less stiff, and then as it cools down it is more rigid and so these calculations are attempting to simulate some of the things that occur within plugs. We see this type of behavior at early-age temperature effects in cement slabs also. So there is some basis with respect to the development of tensile strains.

If we look at lowering the placement temperature, we can see that now we have the plug, it is expanding up into the formation, we have reduced the hydration temperatures and the analysis would suggest that we would then develop a compressive interface stress. This is suggesting at least in terms of trends that it would be appropriate to look at lowering the placement temperature relative to the surrounding rock.

Briefly, some of the conclusions from the seal hydration analysis is it is appropriate to try to select a design mix to develop interface stress. Now, of course one of the things that I think is important to recognize is, is that if we put in certain constituents that would be expansive they may not be favorable from the standpoint of longevity, so there may be some real tradeoffs here. On the one hand the interface is affected by interface stress, in accordance with the cubic law, if it models equivalent smooth wall fracture aperture. On the other hand, as has been pointed out, some constituents may pose some problems from the standpoint of longevity.

Nonetheless, with respect to reducing the placement temperature, that appears to have a very favorable affect on the seal in terms of developing interface stress. And finally, although I haven't discussed it very much, Malcolm is going to talk a little bit about lowering pressure injection. It may be desirable to develop compressive interface stress by a low pressure injection.

MR. CANTON: Questions for the Board? From the staff?

Thank you.

We will go on to Malcolm Jarrell.

MR. JARRELL: Hello, my name is Malcolm Jarrell. I am with IT Corporation. I am technical manager of our engineering office in Austin, Texas. I am going to talk a little about the technology to seal exploratory boreholes.

I was part of the team that was tasked to evaluate technologies, methods, sealing materials in current use in the oil and gas industry and in water well industry that might have application in the sealing of the boreholes at Yucca Mountain. In addition, our team had considerable experience with the plug and abandonment of industrial wells used for disposal of non-radioactive hazardous and non-hazardous waste waters.

I want to qualify our work a little to say that we haven't really fully evaluated existing operations related to the construction of the exploratory boreholes at Yucca Mountain, but I would suspect that the equipment that might be employed or the specifications for the equipment in terms of hook load capacity use and circulating equipment would be similar in the sealing operations as the construction operations.

I would like to talk about primarily the sequencing of operations which would include borehole preconditioning, seal selection and emplacement techniques, testing of the seals, and unusual conditions that are encountered generally and are very likely to be encountered at Yucca Mountain.

Preconditioning of the boreholes can be divided into two types of operations, what I would call routine operations, that is the removal of equipment that was designed to be removed for maintenance or whatever including tubing, packer and screens. And specialty operations, the removal of materials that weren't designed to be removed.

And that would include casing that would be cemented in or partially cemented in and also fish and junk. Fish and junk are terms used in the oil and gas industry to identify equipment inadvertently lost in the well if tubular goods, drilling equipment, bits will be referred to as fish. If that material became mangled beyond recognition, it would be referred to as junk.

In addition, many of the boreholes that were completed at Yucca Mountain were done so as Joe Fernandez indicated with a variety of drilling techniques including some polymer mud, rotary mud, airfoam systems that may have left wall cake deposits on the inside of the borehole. And if it were determined that these might compromise seal integrity, borehole pre-conditioning, might also include the removal of that wall cake by borehole scratches with air limitations or under reaming of a more heavy duty way to scour the inside of a well bore.

It is generally considered in oil and gas industry, injection well industry as well as here, that getting a good seal is going to require removal of casing from those wells that are cased. In hydrologic test holes, the casing typically extends to a depth of 1,200 to 1,300 feet with several hundred feet of cement at the base of that casing. And then the upper part of the casing would be free pipe unless it were bound up by a collapsing borehole.

One method of removing pipe above the cemented section, if this lower portion were cemented and the upper part of the casing free, would be to use a tool like this jet casing cutter which is a wire-line conveyed tool that incorporates shape charges developed for military purposes that will sever casing in a single shot and is activated by an electric signal from the surface. It's limited to 5 1/2 inch diameter casing and therefore other tools are used for cutting larger pipe. For example, this inside hydraulic casing cutter is one type of tool that can be used in saturated or unsaturated zone to sever pipe. It is anchored in the casing and then hardened steel knives are rotated and severs an 8 5/8ths type casing that might be found in a hydrologic test hole in about ten to fifteen minutes.

The free pipe then is simply pulled from the well with the surface equipment after removal of the cutter.

There are literally hundreds of specially designed fishing tools in the oil and gas industry for removing casing, tubing and other types of equipment that get inadvertently left in a borehole. This is just an example of a tool that might be used to grapple onto the top of a casing tubing or other tubular good from the outside and withdraw it from the well. This problem could also be attacked with an inside spear inside the inner tube here or the fish to pull it in order to remove it.

I would say that the operational challenges at Yucca Mountain are going to be more related to preconditioning or removal of fish and casing rather than the actual emplacement of seals. And at some point there will need to be a determination made onto what extent is it necessary to remove all of the man-made artifacts that are left in the well as opposed to removing those that will come with relatively practical effort.

This would be an example of a tool or scratchers that might be used to remove wall cake in relatively uniform concentric holes. And there are various types of scratchers that run either longitudinally or circumferentially around a work pipe that is either rotated or reciprocated to get a scratching or scouring action on the inside of the borehole.

It is also possible to use high pressure jetting to remove wall cake in an unsaturated well bore and also the use of mechanical under reamers that have large arms with coned bits on the outside that will remove in larger diameter holes wall cake, cement grout and rock, if necessary.

Some of the types of problems that are encountered in large sealing programs are listed here, these might include collapsed casing, large wash-outs and caved in areas. These can be particularly problematic if the top of a fish is located in a large washed-out area because it doesn't provide a good neck to latch onto or get inside. Lost tools and equipment in the hole are relatively common problems, and there are certainly those wells at Yucca Mountain where this problem may be encountered. There are also problems that fishing operations aren't always initially successful and that fishing tools become lost in the hole concurrent with trying to remove some lost drilling equipment or plugging equipment. And then there is the problem of getting stuck through collapse of the borehole during a plug and abandonment operation. And the "thief" zones which I would describe as highly permeable zones that might drink large quantities of sealing material that might be introduced in a slurry form, it may take much larger volumes than the discrete well bore volume in order to get sufficient sealing material to fill a given capacity of borehole.

In the oil and gas industry and the water well industry, cement plugs are the most widely used plugging materials. The advantages of the cement plugs to those industries are that it can be tailored to fit almost any need. There are literally hundreds of additives to the types of cement used in the oil industry to develop different characteristics and to use in specific geologic settings.

The plugs themselves provide structural support. They will hold fluid pressure. They can be made to be expansive and they are self-supporting after curing.

The disadvantages we talked about earlier include the cracking hydration problems, potential expansion and shrinking with thermal cycling. Some blends are very sensitive to contamination and therefore mixing waters have to be carefully analyzed and tested prior to going to the field to mix cements. Contamination problems can lead to failure of cement plugs to set up or may cause them to flash set.

Also strength retrogression is a problem with oilfield Portland cements at temperatures greater than 230 degrees without proper additives, usually silica flour, to inhibit that. But in the oil and gas industry, cements are used with proper modifications in steam floods and in fire floods enhanced tertiary activities that can get to a temperature well over 1,000 degrees fahrenheit.

Clay seals are rarely used as the primary sealing material alone in the oil and gas industry. Typically in a deep well, certainly one that would penetrate, saturate horizons, cement plugs may be used intermittently with slurry mud sealing material. I think it is primarily done as a matter of economics and not of design with the cement plugs always being set where the highest performance is required.

Typically it is, in recent years it is frowned upon to use either bentonite seals or backfill materials for plugging wells in the water well industry. Some clay seals are used in construction practices for specific reasons.

They are sorptive; no heat of hydration. The disadvantages already discussed include the shrinking/cracking and perhaps not holding pressure and being minimally supportive structurally and potential for settling.

As far as the placement techniques for cement, clay and sand seals, I mentioned sand in here as it has been proposed that it might be used in certain areas as a drainage layer that it would be essentially introduced in the same manner as a powder or pelletized bentonite or other clay material. That is, that it would be poured from the surface either directly into an open borehole or through a workstring or else pumped in as a slurry.

Cement plugs, there are four basic methods for installing cement plugs in a well bore. I will go over those now. They include the dump bailer method, balance plug, what is called the two plug method and a pressure squeeze.

The dump bailer method involves the conveyance of a small volume of cement within a canister on a wireline. It is emplaced in a borehole to a very controlled depth and then released from its canister with either an electrical or mechanical dumping. It is usually emplaced on top of some type of supporting structure, either backfill, a mechanical plug or a cement plug; a pre-existing cement plug.

The disadvantage of this procedure is the volume that is delivered since the canister is roughly a 3 1/2 inch diameter tube a route 30 feet long delivering no more than 5 cubic feet of cement. It would take many, many runs to get a lift of great height on a relatively large diameter borehole. So when greater lifts are needed, they are usually pumped in from the surface through a workstring.

In a saturated environment, caution is taken and it is called the balanced plug method, because once the materials of different density are emplaced through the workstring, the annulus and tube act as the arms of a U-2 manometer such that displacement calculations have to be done to precisely get these two arms balanced such that they are in a static mode. That is when the cement would be displaced with a relatively non-compressible fluid. Now cement could be emplaced in boreholes at Yucca Mountain by displacing them with air. In that case they would probably be a baffle tool or a baffle located inside the workstring and a plug, a rubber dart would be pumped behind the cement. That would be blown pneumatically with air or gas and until the plug engaged in the baffle, at which time the tubing would pressure up and you would get physical evidence of that pressure at a pressure gauge at the surface and know that the cement was at its proper place, then the tubing could be lifted out of the cement and leave the plug intact.

Another technique that might have application here is the squeeze technique. When cement is introduced or interjected into a zone at a pressure that is less than the pressure required to fracture the rock or propagate an existing fracture, it just caused a low pressure squeeze. And it is performed by setting a packer in competent rock above the zone where the cement is to be injected and that forms a temporary seal such that cement or other plugging and sealing material is not pumped up the annulus and this allows us to achieve pre-determined squeeze pressure to force that material into the zone of enhanced permeability or into fractures.

The one caution here is that the packer must be set in an area such that the injected cement cannot circuit around the packer and get above the packer tool. That might result in the packer being stuck in the well.

The testing of plugs in the oil and gas industry, the state of that I would have to say is very primitive at this point. The API or American Petroleum Institute has published specifications for basic mixtures of cements. There are eight basic classifications of cement and tables are generated with those properties. Most of those properties have very practical field importance. For example, not just performance of the seal, but the pumpability of the cement to ensure that you can get it in place with the equipment that is normally used in the oil patch, as well as development of high early strength so that the contractor can get the cement and get it cured and then move ahead to the next operation.

The bigger manufacturers and suppliers of oilfield cements have research laboratories where they can do a lot of testing. They can run hypothetical cement jobs, the bottom hole and pressure conditions. They can do testing of bonding of their cement, candidate cements to host rock and to casing. Generally, there is some testing anytime with the specific materials and the specific mixed waters, anytime a real critical plugging operation is planned, and then also samples of the cement are collected during the field operation to ensure that they will achieve the results. It provides a sample for performance testing at the surface, hopefully representative of the cement as it is at the bottom of the well bore.

As far as in situ testing it is rarely if every done. Most operators or contractors who place a seal will wait a pre-described period of time to allow it to cure, waiting typically a 24 hour period. Then the plug is tested by taking a drill bit, setting down on the plug, applying the weight of all or part of the drill string, say ten or fifteen thousand pounds on that plug, and if it holds, it is generally assumed that plug will meet all other performance criteria. That is the way it is done.

The economics of an in situ test probably is a primary reason rather than the availability of technology in that procedure. I believe the technology does exist to do more specific testing with the performance by applying a hydrostatic load on a seal, but would require evaluation of the seal from beneath through an excavation, for example. It would be a test in one point in time and it would be a test on one particular plug.

As Joe Fernandez pointed out, there are a variety of different types of borings, drilled for different purposes to different depths, at different diameters, completed in different ways with tubing, with casing, some cemented, some not. And so there is a lot of individuality with the particular boreholes such that any sealing plan that would be developed under the umbrella of the specific strategy would be individualized for a given well or a given borehole.

The procedure would however have certain specific general procedures, or certain general procedures that I have outlined here. That would be to check for fill or obstructions; the removal of preconditioning of the borehole; removal of components and junk and fish and a comparison of that borehole to the driller's logs that were prepared here in construction.

As you saw earlier, there are available camera logs for a number of these wells and the quality of those is very good and it is a good idea for us to have camera logs on all of those boreholes as part of the individual assessment and fine tuning of a sealing plan for those individual holes.

In order to determine the quantities of sealing materials that would be utilized, there are available mechanical calipers that will measure the diameter of the discreet borehole with arms available up to a 48 inch diameter. So, holes that are larger than that, or holes which have washouts or cavings to greater than that would be difficult to assess their diameter in order to calculate sealing material volumes. Sonar calipers are available but are not effective in air or gas filled holes. So that would only be a tool that might be used below the water table.

Once this information is gathered in the field, then there would be certain information almost assuredly gathered that would be used to modify or fine tune the individual sealing plan that would be developed prior to site activities.

The conclusions our review of the technologies will be incorporated into Joe Fernandez's next talk on the sealing program. I will leave that to him, but entertain any questions regarding field applications at this time.

DR. CANTLON: Board questions? Staff?

(No audible response.)

Thank you.

Joe.

MR. FERNANDEZ: The last three presentations you have heard result from a number of calculations. You have also heard from Malcolm, his available technologies work.

What I would like to do in this presentation is to wrap up all the ideas that we have discussed and give you our ideas of what our strategy is, currently, to seal exploratory boreholes at Yucca Mountain.

You may recall, I centered my discussion on four questions. One was, what is the significance of the boreholes, and then there were three other questions, how, when and where to seal those boreholes. I would like to present my conclusions along that same line trying to address each one of those questions.

In the area of significance we feel that significant boreholes, those that would potentially represent a preferentially pathway of release, whether they be airborne or waterborne release, basically are within the repository and close to the edge of the repository. I would also like to point out too and acknowledge the fact that we did use a very simplistic model. We tried to maximize the extent in the case of the air calculations, using longitudinal and transverse dispersivities that basically were equal, so if in fact we had some migration in a welded tuff once it hit the non-welded tuff, i.e., the Paintbrush non-welded tuff, there might be a different flow path from that point on. It might actually tend to spread things out a little bit more. We assume that there was a maximum spreading occurring right at the repository horizon. So, I just wanted to acknowledge that fact, too.

The deep boreholes are potentially more important than the shallow boreholes by five orders of magnitude in one of the calculations that we did.

The when to seal, it seems like to seal deep and significant boreholes within the repository boundary concurrent or with or before waste emplacement, I think that was borne out to some degree in the calculations that John had presented. I think we need to, when we complete this work, put it in better perspective such that we understand what the maximum thermal loads would be and, etc.

The how to seal, is really a synopsis of what Malcolm was talking about, and also some other things that we didn't talk about. I would like to very briefly elaborate on those. The first thing is to use low pressure squeezing to develop a compressive stress at the interface zone.

You may recall from Tom Hinkebein's presentation and some of my earlier presentations, that at one time in the program, we wanted to use a material that would develop an expanse of force. The way of doing that was to create a mineral co-ettringite. That mineral had some problems under high thermal loads as far as its stability. Tom also pointed out some other problems associated with that material and also with minerals like a portlandite and gypsum that would have a tendency to expand. This seems like a compromise. We can eliminate a problematic mineral like ettringite, but still achieve some sort of compressive stress development that John was talking about through the techniques that Malcolm had just presented.

The second point, we really didn't talk about. This calculation, what we had done in a nutshell, was to take a look at what types of load might be transferred to a plug, looking at three different locations. We had done our analysis assuming there was a plug either at the PTn TSW-1 contact at the repository horizon, actually slightly above the repository horizon, and then at the TSW-3 CHn-1 or the Calico Hills bottom of Topopah Spring contacts. We then had subjected each one of those plugs to three different types of load. To a static load, that static load being a column of water or sand filled all the way up from its location up to the surface, and then filling it with portions of water, whether it be one-third or two-thirds or the full height of water all the way to the surface. We also looked at the thermal loads that that seal would actually see and we looked at seismic loads. We combined those in different matters. And it seemed like there was a point at which we reached diminishing returns as far as the performance of the plug itself and as far as resisting the forces.

What it turned out to be is that we achieved a significant amount of reduction of the forces along the edge of the plug if the plug was less than ten meters. But, beyond ten meters in length there seemed to be a point of diminishing return where we really didn't achieve any enhanced performance or reduction of stresses along the interface zone. That is why this second bullet says, increase length of the plug up to 10m to resist the static, dynamic, and thermal loads. Lower the temperature of the grout by reducing the heat of hydration and pre-chilling materials. That was a point that John had raised in order to increase the compressor strength along the interface zone.

The how to seal, I would like to expand a little bit on some of our thoughts. This represents the airflow model that you saw earlier. Here is the extended repository boundary. The air is released within this entire plane of the repository and goes out to some distance away from the projected edge of the repository at the surface. Recognizing that this might go up like this (indicating), and it might go over like this (indicating), or back up like that (indicating), or in some other fashion. This is just a schematic representation of what we believe might be the affected area. This is approximately 600 meters to the north and to the south with some restricted area to the east and the west.

The point of showing this slide is really to show this little diagram on the bottom here. What we did is in order to come up with the airborne design requirement, we assume that we would have some sort of a failure along this interface zone as well as the longitudinal crack along the entire length of the seal are what we tolerate with respect to this aperture. This is the plug material, this is the rock out here and this is our aperture. What was the maximum aperture that we could accept in order to meet the requirements that we talked about earlier.

You may recall we had a formula; it was the harmonic mean formula that we looked at in which we had the total thickness over the effective conductivity in materials equal to the summation of T over the conductivities of each one of the layers whether it be the Topopah Spring, Paintbrush Tuff, the Tiva Canyon and then the seal itself in the case of a potentially shallow borehole, or just the thickness of the seal itself.

We had come up with an overall requirement as you may recall. Now, what we are doing is we are saying, let's translate that into some sort of a failure, if you will, along this interface zone, and what can we tolerate with respect to the matrix properties of the seal and the fracture property of the seal or the interface or the aperture zone.

This was the resulting design chart that we had developed.

Each set of curves here represents a model. This was for one model with a certain set of air conductivities, a second model with air conductivities and a third model with air conductivities. This one line represents the repository boundary. The next associated line is the repository boundary extended out just a little bit. We have six curves here and I just wanted to explain that.

This is, on one axis, the log of the aperture in microns. So this would be a thousand micron fracture. Again, the fracture being all the way around the edge of the seal and longitudinally through the seal. This represents the log of the matrix hydraulic conductivity of the seal itself. This is in centimeters per second.

What this shows is, in this particular zone, we have a very low air conductivity material. This says that basically we can tolerate up to a thousand micron fracture around the periphery of the seal and longitudinally splitting the seal all the way to the surface, in order to still meet the one percent criterion of release that we had talked about earlier. It gets to a point in which, however, we need a very tight interface where the conductivity of the material itself is quite high. And as you might expect what you have at this point is a fair amount of air being communicated through the seal itself, so we would have to restrict our requirement on the aperture and that is why this aperture drops down here.

And so basically, if we were to have a very good interface, our material could be on the order of 10^{-1} to one hundred centimeters per second. This is our requirement for the seal above the repository.

Now below the repository, this work was actually done some time ago, and it reflects the same type of thought basically. We want to restrict the amount of water flow that potentially could go through the borehole to one percent of the flow through the remainder of the rock mass. We had looked at a number of cases in which we postulated every repository horizon, we could have contamination dribbling down to some sort of an interface zone migrating over then going into a borehole. But we didn't know to what extent this lateral migration would occur.

We just postulated that, well, let's just presume that all the flow that could migrate down from the repository horizon hitting these contacts prior to hitting the ground-water table, could migrate out one kilometer, two kilometers, three kilometers, four kilometers or five kilometers. And that is what these numbers represent. Let's say they do them in different fashion, such that the stratigraphy is controlling the drainage of water from the edge of the repository either from the east, to the southeast in some fashion to the east and southeast and other combinations as shown here.

What we derived from this particular analysis which really was an areal analysis of flow through either the boreholes and the remainder of rock mass was a broad range of conductivities, depending upon the hydraulic conductivity of the Calico Hills. That was the primary concern. We were looking at the flow through the Calico Hills unit. We had looked at a broad range of hydraulic conductivities of the Calico Hills unit from 10^{-4} to 10^{-8} centimeters per second. And these points correspond to our assumptions on the bulk rock hydraulic conductivity.

So we basically said, given either one of these conditions, our requirements for seal in order to meet the one percent of flow actually passing through the borehole seals in comparison to the 99 percent going through the rock mass for all the situations and all the cases evaluated, cases one through 6, would be anywhere between roughly 10^{-3} centimeters per second to 100 centimeters per second. Actually quite similar to the airborne design requirements.

The other question of where to seal, it was logical I think to begin with, without doing any analysis, to locate seals away from the high temperature zones near the repository. I think the calculations of that were performed, it demonstrated a little bit more that it pays to place your seals, particularly those boreholes that are located within the repositories, to place the seals away from the high temperature zone. It gets into many other concerns, thermal dynamic equilibrium, just thermal mechanical interaction of the plug itself between the plug and a rock that it makes sense to really locate those seals away from the high temperature zones. Place the primary seals, cementitious seals where the hole conditions are good or excellent. We saw a few of those photographs, where in some areas the whole condition is very good.

Those areas that it would be very good would be in the Paintbrush tuff, the non-welded to partially welded zones. The upper part of the Topopah Spring member, the Topopah Spring really grades from a partially welded down to a densely welded material. It is in that upper zone where the hole conditions are good and where we are proposing to place a seal potentially.

In the Calico Hills vitric and zeolitic zones, in those zones the quality of the hole was quite good.

DR. CANTLON: Before you move that, you are commenting on where to place it. What you are really saying is where the seals will be most effective.

MR. FERNANDEZ: That's true.

DR. CANTLON: Maybe where you want to place them are the places that are the biggest problem which is a different thing that what you are addressing.

MR. FERNANDEZ: I also want to point that these are the primary seals also.

DR. CANTLON: Right. But if you've got holes, you've got to seal them whether they are good or bad.

MR. FERNANDEZ: Right. Exactly.

DR. CANTLON: Right.

MR. FERNANDEZ: Based on the information you heard, our concepts were to place granular material in selected fracture areas where perching is possible to dissipate porewater pressures. This is really to address a concern of the potential for perched water, for example. An accumulation of water in a particular borehole, it is better to have a granular material that would actually dissipate that into a fractured area. And also, the possibility of putting granular materials in high temperature areas to avoid the problems of the seal somehow degrading through time.

Place a combination of clay and granular materials in highly fractured tuff below the Paintbrush tuff and the repository. The intention here was to avoid problems of placing massive amounts of cementitious or fluid materials in fractured areas where you would lose the majority of the materials into the fractured rock mass.

As far as the consideration in selecting seal materials, we felt that using a rigid cementitious seal where structural performance is desired as opposed to a bentonitic or other clay-like material; avoid the placement of cementitious seals in high temperature environments; enhance the stability of cementitious seals; to minimize leachable phases such as portlandite. As Tom had mentioned earlier, the issue of portlandite is not really one of the more desirable minerals to have as far as this expansion behavior or like gypsum was to control the reactive alkalized sodium and potassium hydroxide by selecting excessive reactive silica. Or, just by controlling the quality of cement that you use is another way of achieving that.

I want to point out here I didn't put down silica fume, because it wasn't really raised in Tom's discussion, I don't think, but silica fume created some problems in some of the large pores at the Nevada test site. They found out that there was hydrogen gas generated in use of silica fume because of the silicon metal that was incorporated as part of the silica fume. And also in some of the laboratory results that are currently being done by Jim Krumshans at Sandia Laboratories, he noticed that there was a funny hydrogen sulphite smell when he broke the samples open. So, it is another indication that the creation of hydrogen gases is not really a good thing for larger type seals. So we got away from using or will get away from using silica fumes in our mixtures.

Reducing the ettringite formation by selecting a cement with low sulfate content. We actually did develop a material in which the cement that was used was not the type case cement which had the higher sulphate content, so we have already made some progress in that area, and use earthen materials, clay or crushed rock, as initial seal in highly fractured areas for the reasons that I mentioned earlier.

There is a concern about placing a mixture of materials in boreholes. Placing a Portland-based grout above a swelling clay, there may be some problems associated with that. The first one is that the grout can increase the pH and the calcium concentrations of the ground water if in fact water is dribbling down in the borehole. Calcium can cause a decrease in the swelling pressure of the sodium potassium swelling clay. We can have problems with elevated pH which can de-stabilize the swelling clay if in fact swelling is necessary.

Possible solutions would be to avoid placing the grout above the clay, which is an obvious solution, or even perhaps placing some sort of a buffer material between the two. The use of a grout formulation that will not release calcium; the use of grout formulation that will not increase the pH. You know, some of the obvious if you don't want calcium or you don't want pH, we try to reduce the concentrations of both of those. Use a calcic form of clay or process the sodic form of clay into a calcic form. I don't believe the processing at this point in time is very well established. So, I think we have to use perhaps a different design approach rather than try to solve the problem with using these latter two.

The overall conclusions to sealing exploratory boreholes, we feel that existing boreholes and proposed boreholes can be sealed using existing technologies. We however recognize that there are individual boreholes that are very problematic. We recognize that care in a drilling operation can also make our job an awful lot easier.

We feel that a general sealing plan is needed for all of the exploratory boreholes. We further feel that a detail sealing plan should be prepared for each borehole. I think it was very obvious, very graphic going through the video logs to see the differences between the boreholes. Yes, you can make analogies if you look at rock type and you can say this is the hole condition. But there are other things that you just don't see from a video log that you can see from other geophysical tools. So, I think as part of coming up with a detailed plan, it is very necessary to go back into the boreholes, re-run the high quality video log, find out where some of these problematic zones might be and run other geophysical tests that would perhaps give us a better idea of how much grout take a particular zone might have if that is where we intend to place grout.

I think some other aspects of the program may also help us with regards to what type of structural requirements we might need to have for these seals in the sense of where are the actual physical properties, mechanical properties of the rock mass in which we are sealing. So I think from a broader sense, the site-characterization program will help us better understand what some of these properties are, whether it be for a non-welded tuff or for the welded tuff.

Finally, an inspection/assessment of each well should be conducted prior to the implementation of sealing procedures, basically, what I said before.

Here is kind of a schematic, two potential concepts if you will, for sealing. This is actually G-4, we just took it off just for simplification; it could be really any well. We have some sort of casing grouted in at the surface. It goes down and in this particular case it might be 40 feet, I don't know exactly. We have a fairly large-size diameter borehole, 17 1/2 inches. Here we have a casing that goes down; it is spot-grouted over a distance of approximately of 24 feet it looks like. We pass through several different horizons. We pass through the Tiva Canyon, Pah Canyon and Topopah Spring Member down into the Calico Hills non-welded tuff.

Based on what you have heard, it would be one conceivable concept to come in with a cementitious seal in the cementitious and perhaps a coupled seal, a grout seal in the upper portion of the Tiva Canyon. The purpose of putting in some sort of a clay seal or a thick slurry grout would be to restrict the amount of fluid that we might lose out here, than to go back in there and re-ream the hole if it was necessary to remove the seal and place in another type of a rigid seal, just a cementitious seal in the Tiva Canyon and also to meet state requirements as I understand.

In this zone here, if in fact we have matrix flow, it might be best to have a capillary barrier here. So, in that sense we would place in a sand barrier over this distance such that very little water would actually be incorporated into the hole at this point and create a static load on this lower plug right here.

I split this lower plug into two areas. The upper portion basically would have no bentonitic or other type of clay grout or a thick slurry grout that would go into the fractured rock because of its high quality. The lower portion we may require some sort of a combined seal in which we might have either the bentonite and the rigid cementitious seal that was balanced from a geochemical standpoint to be compatible to address some of the issues that I mentioned earlier.

The remainder of the hole down to the Calico Hill seal would be basically filled with a course material. Perhaps it would the course material with fines. There would be no reason to have only a high permeability seal, in fact we can have a very low permeability seal. But basically this would be just a crushed material, until we came down to the Calico Hills unit where we have the placement of another seal in this high quality rock here. This can be basically any distance here. We have restricted the length here, because, we don't feel from a structural standpoint looking at the scenarios that we considered that we need to have a seal that is over the entire length of the borehole.

As was pointed out before, there may be the other problems of concern for rigid seal located at the repository horizon from a thermal mechanical standpoint. The only difference from this design concept and this design concept is the possibility of some sort of perched water occurring over here, where we still would have our sand barrier, but we would perhaps increase the size of this rigid seal such that the geology or the water flow essentially would not see this particular borehole. It essentially would just go over it. That is the only difference that I see between these two concepts.

In conclusion, recommendations to sealing exploratory boreholes, I think these are recommendations that we can use now; we can pass onto those people who are doing the site-characterization program. And I think it is very prudent to do that or at least work with them to see how well they can implement some of these recommendations.

Maintain a detailed construction documentation. I think for the most part the driller's logs and the other logs that I have seen have had a fairly good well, documented documentation. So I think that is in pretty good order.

Select drilling methods, if possible, that will reduce the wall cake build-up to ease our operation of going back in there and scraping off some foreign materials that we may not necessarily want on the wall of the borehole.

Select drilling methods, if possible, that will result in a better well condition. I think some of the prototype drilling that is being done right now will achieve that particular objective.

Minimize the risk of losing drilling tools and junk in the borehole; develop a protocol for tool inspection. Make routine field inspections intermittent with downhole operations. As Malcolm had pointed out, we think we can retrieve these materials, but if we can avoid the problem, let's avoid the problem to begin with.

Utilize materials that are relatively easy to remove through fishing or milling, so if in the event there is an accident that occurs, use materials, tools, whatever, that would make our job a little bit easier.

Finally, limit number of exploratory boreholes. This was a concern that was raised by the NRC. When you go through your site-characterization process, I believe their words actually were to limit the number of exploratory boreholes. And I think this is where the benefit of a statistical program that looks at how many exploratory boreholes are necessary in order to properly characterize the site can be very, very useful

With that, I'll take any questions.

DR. CANTLON: Questions from the Board?

DR. PRICE: Yes. Are there any retrieved/unretrieved fish and junk in boreholes right now?

MR. FERNANDEZ: Oh, yes. There is quite a bit.

DR. PRICE: Is there an inventory of it?

MR. FERNANDEZ: The driller's log keeps a fairly good list of it and I believe it is pretty accurate. I would think that as part of the sealing operation, we would have to go back to verify that, and the logs that we currently have. But yes, there is material currently in some of the holes.

DR. PRICE: Is it the intent to pull all of that stuff out?

MR. FERNANDEZ: Yes.

DR. CANTLON: I have a question. Do you have any ball park figures on the cost of sealing versus the cost of drilling? Is it times 2 or times 4?

MR. FERNANDEZ: I have never done a cost analysis, but I think Malcolm may have been in the situation where he might be in a better position, maybe, maybe not?

MR. JARRELL: I could relate to what it would be in the oil and gas industry in injection well, but it would be difficult to compare. Typically it may cost $100,000 to plug a $1,000,000 injection well. So, a tenth of the cost of drilling.

DR. CANTLON: But looking at this kind of thing which is a very different operation from that.

MR. JARRELL: Yes. And also it would depend very much on the extent to which all man-made materials would have to be removed. I think the cost could amount exponentially if it was decided to remove all cemented casing, for example.

MR. FERNANDEZ: I guess I would like to add an addendum to that. It isn't our intent right now to remove casing which has been cemented in. We would like to remove all casing that has not been cemented in. But, right now we don't see a need--we think we can achieve our objectives by leaving the small portions of the casing that are in there. If for some reason it becomes necessary to remove the casing that is grouted in, I think we have the tools available to do that, but it is not necessarily the cheapest of operations.

DR. CANTLON: Well if you are going to get oxidation or corrosion of the material, you are not going to have a very tight seal after awhile.

MR. FERNANDEZ: Yes. That is why I think an individual sealing plan has to be developed to make sure that it achieves the quality of the seal looking at the quality control aspects of the material that you are placing in there overall meets the performance objectives. So, you can still have a portion of the seal that is in pretty poor condition, but the rest of it is in very good condition, whatever that would mean in order to achieve the design requirement.

MR. CANTLON: Has anyone looked at the PVC breakdown products and the extent to which they might mobilize plutonium and some of the other materials?

MR. FERNANDEZ: Not to my knowledge. I don't know. Has there been?

DR. HINKEBEIN: They have done those kinds of experiments with the Waste Isolation Pilot Plant. They have tried all kinds of strange combinations of organic materials and checked on Kd's, distribution coefficients and with various rocks and they do find that there are some potentially bad actors.

DR. CANTLON: Thank you.

MR. FERNANDEZ: I might also add that something that we are looking at in the overall performance assessment work is to assess how much foreign material is in the underground facility. You know, for example, how much concrete me might use. This is all a sealing issue, it is not a sealing issue associated with exploratory boreholes necessarily, but it is a concern that we have in the overall program to find out how much manmade materials will be in there and how will that affect the overall performance.

We have done some work into that area; the project has done some work. Los Alamos specifically associated with exploratory shaft looked at how much foreign drilling fluids would be in there and different types of materials.

DR. CANTLON: Bill, you had a question.

DR. BARNARD: Yes. On one of your slides you indicated that there were about 400 boreholes that either exist or are planned. Approximately how many would be the minimum number that would be required for site-characterization? Do you have any ballpark estimate?

MR. FERNANDEZ: I guess it really depends on who you talk to.

DR. BARNARD: Well, what sort of ranges of people?

MR. FERNANDEZ: I don't specifically know an answer to your question. I would think--there are many different groups working on the site-characterization program. You have Alan Flint of the USGS who is associated with the UZN holes. And he would argue that all of the boreholes that he is intending to put down, he really needs. And I am not really in a position to evaluate if he needs all of them or not. In fact, he may.

For example, some work that Sandia is doing with Chris Rautman, he is looking at a systematic drilling program. He has done a very statistical analysis of how many boreholes would be required to accurately define the properties of the rock mass. I think he is probably at a bare minimum. You know how many are there, you know probably internal and slightly external I think on the order of 12 systematic drilling holes, something like that. In a certain regard if you go across the project there are many other groups that have an interest in acquiring core.

DR. CANTLON: There is a trade off however, if you have got underground with drifting, you wouldn't have to have as many holes.

MR. FERNANDEZ: That is certainly true.

DR. CANTLON: And since you have to fill the drifts anyway, the whole cost of sealing would go down.

MR. FERNANDEZ: True.

DR. CANTLON: And the complexity of it would go down. A big argument for getting underground earlier.

Other questions? Audience?

(No audible response.)

All right, let's go on then. You are on again, Joe.

MR. FERNANDEZ: The next presentation is the second strategy piece of work that we are doing, this strategy is to look at sealing or backfilling the shafts, ramps and underground openings. It is a combined presentation that I have with Mike Hardy who is a principal for JFT Agapito & Associates. What I would like to do is present the overall approach that we are intending to do and also to have Mike go into some of the more detailed concerns, thermal mechanical concerns associated with sealing and backfilling. Beyond that I would like to have Archie Richardson go ahead and make a presentation.

The organization of my presentation as well as the remainder of the presentations in this segment of our discussions here is I'll go ahead and present the regulatory guidance associated with backfilling, the approach that we are intending to use as far as coming up with a strategy for sealing the underground facility shafts and ramps. I think there is the area of available technologies that I will briefly discuss and the basis for why available technologies is important and where they currently are presented in the SCP.

Archie will present this portion of the work which is sealing, backfilling underground facility. Mike will present the thermomechanical considerations. And then tomorrow, or possibly at the end of today, John Case will present the backfilling and the sealing of the shafts and ramps, work that Ian Hynd has been collaborating with us on.

In the first bullet there I have shown the regulatory guidance. Well the fact of the matter is there is no specific criteria for backfill from the regulations all of it really is interpretative. There is some specific criteria that we have developed from the analysis, from the hydrologic analysis that I have presented earlier for the backfill in the drifts or for the backfill in the shafts and the ramps. There are some general criteria however, that are presented in the regulations, and I thought it was worthwhile just to briefly go through those and to show you the interrelationship between the system that they are talking about and what the criteria is.

For example in the overall criteria of 60.112, they talk about the geologic setting and the engineered barrier system, which would include the backfill in the underground facility and the shafts, boreholes and their seals. Here they basically say that they should be designed such that they comply with the EPA environmental standards and 40 CRF191.

Another point, Section 133 of 10 CFR60, where it talks about selecting the orientation, geometry, layout and depth of the underground facility in the engineered barrier systems, such that they contribute to the containment and isolation. Basically the same as the one up above.

Several others that are mentioned in the regulations include these. The underground facility,it talks about providing control of water and gas intrusion. Again a very general statement. The underground openings, maintain the retrievability option. I don't see necessarily that backfilling will preclude this option, but nevertheless it is something that we have to address in our backfilling strategy. The rock excavation techniques in the underground facility shall be selected so that to limit the potential for creating preferential pathways for release of radionuclides.

And finally, it talks about engineered barriers including again the backfilling in the underground facility to assist the geologic setting to achieve the performance objectives. All of these are very general criteria.

Our approach in developing the strategy will be to try to look at three different areas, look at overall performance that would be required which is addressed in Issue 1.1 in the SCP. Coupling that with the post-closure design which is issue 1.11 of the SCP, and the available technology which is issue 4.4. In the available technologies, Issue 4.4 says that they should be adequately established so as to achieve the overall performance objectives.

There are a number of information needs that are included for 4.4.1 through 4.4.10. The one that we are concerned with and addressing here is the seal placement with reasonably available technology. That is why it is important in the development of this strategy to address what technologies currently exist in order for us to seal the underground facility and are they reasonably available, a basic question.

We are going to integrate these performance designs and available technologies with the issue 1.12 that Jon White had mentioned earlier, the sealing issue, where it talks about designing the characteristic configurations of the seal such as they comply with 10 CFR160 and provide information to resolve the overall performance issues.

The approach that we are going to use is basically again in those three areas. Now, rather than looking vertically down, we are looking horizontally down to develop those performance criteria. Those things that looking at the issue 1.1, what performance criteria do we need to establish for the backfill or for seals in the underground facilities and likewise in the shaft. Are there additional criteria that we have not addressed at this point in time.

We have talked a little bit about the hydrologic criteria for the shafts and the ramps. Something that was not brought out in our discussion was the quality of the backfill from an airflow standpoint and the analysis that we performed showed that the release to the accessible environment, the release, i.e., the airborne releases were in general not affected by the quality of the backfill in the underground facility. It was controlled primarily by the backfill that was in the shafts and the ramps.

We have another set of criteria, thermomechanical that Mike Hardy will be discussing very briefly, not necessarily the criteria at this point, but some of the considerations. Geochemical concerns in some instances will be very close to the waste package. Is there some restriction that we need to have so that we can enhance the performance of the waste package criteria or restrictions in a geochemical standpoint. Vapor transport, something that we have not addressed to this point in time, are there some concerns associated with heating the repository, transport of vapor, is there some way in which we might be able to enhance the repository performance through the use of very selective backfill material.

In the second, the constraints, this really is kind of a designs concern. Issues as far as placement and timing, when do we actually place the backfill. When is the most opportune time? What are the tradeoffs associated with placing those materials, particularly as they relate to the temperatures that we might see if we backfill or don't backfill. Where are the issues associated with the removal of ground support? Are we going to have to remove some of the ground support system and will that present unique challenges for us. Finally, in some instances the confined space for placement, do they represent problems.

The third area is available technologies in the shafts and ramps, underground facilities, define what the limitations would be, determine what the availability of performance-related information. And when I say performance-related information, a concern that I have now is that there aren't many mining operations in this country and elsewhere in which the mining operations are not so much concerned with the performance of the material they are putting in. They are more concerned with the ability to extract the ore, for example. So, if it does the job, they really don't need to spend more money into the mining operation, which decreases from their profits. So, the question that we asked ourselves, do we have any information that we might be able to extract from literature, from case histories, in order to enhance our data base of understanding what are the achievable properties, hydrologic and/or other properties that we would have for the backfill materials or other seal materials?

Again, our approach is to incorporate each one of these analyses, if you will, into developing a strategy which addresses the significance and the how, when and where to seal.

Currently, we are proposing some numerical analyses. What is the role of backfill in restricting lateral flow, primarily from a vapor transport. Is there a real issue there? I think we have to integrate with other people in the program, with the waste package people, with the U.S. Geological Survey people, in order to understand if in fact this may or may not be an issue.

Define what the geochemical constraints are and propose compatible materials. I see perhaps not a numerical analysis but at least a qualitative evaluation of what would be required where some of the restrictions that may be placed on or constraints placed on the sealing system, in the sealing materials so as to enhance a better performance of the repository. An also, to answer the question, is it really necessary to have this enhanced material as backfill or seals in the repositories?

To assess the long-term drift stability of intact rock, slip along joints and potential faults. The correlation within the SCP is not very strong at this point to say what the impact of a slippage along the fault would be. Is it necessary to have a rigid material to restrict the slip along the fault, or to restrict movement around a drift. Is it really necessary? In fact, maybe it is better to have, in the case of backfilling in an underground opening, to have some of the fractures open up and potentially create some of the capillary barrier by a fracture system above the drift. I don't know the answer to these questions, but it is something that we intend to evaluate in our backfilling strategy.

Assess effect of backfill on limiting yield around the drift. Essentially the same thing I just mentioned.

Evaluate the orientation of faults/joints that could be activated with no backfill. This work will be done hopefully over the next year. And the work that is currently being performed is we are looking and have looked at a number of case histories. In order to better understand what technologies are currently available, it is work that I am currently doing with Archie Richardson, and he will describe a little bit more in one of the following presentations here. We have done some of this already. We have visited some of these mines; we have reviewed some of these case histories. Very briefly, to go through it, there is a small mine, Apex mine near St. George, Utah which had used pneumatic stowing. We have visited the mine to see how the operation was done. What were the limitations associated with that backfilling operations? What were their basic objectives? Was there any information that we could acquire from a performance assessment standpoint, a quality of material they were placing in?

As I recall the material they were injecting, it was a very poor rock quality and they were injecting rock from basically 1,000 psi and it was more than sufficient to achieve their objective of extracting ore from the mine.

There is the Billie Mine in Death Valley. That was again another pneumatic stowing operation, basically from the surface down through a pipe into the underground openings. We looked at their operations and the complexity of pneumatic stowing.

The Cannon Mine which is a gold mine up near Wenatchee, Washington, Archie I think has some pictures on that, where they use mechanical compaction in order to extract again more of the ore. So, they mine a certain section out, put backfill in their mine in a section adjacent to that in order to achieve a 100 percent or near 100 percent of extraction of the ore.

The White River Shale Oil Project, which is a project in Utah. Here there was a major problem of sealing off, as I recall the Birds Nest aquifer, a very large producing aquifer. You know, some very unique grouting challenges. Not necessarily the challenges that we would expect to see in the unsaturated zone, but the point here is to say, yes, technologies do exist. They have been applied to handle some very extreme cases.

New Waddell Dam, is a place we have not gone to this point, but it is a new dam currently under development near the Phoenix area in which they have a grouting operation, emplacing a grout curtain at the bottom of a dam in the future.

The NTS faces many unique challenges, particularly in the area of emplacement of large cementitious seals. There is actually quite a bit to be extracted out of that information. We have already capitalized by incorporating the interface study with the work that we currently will be doing.

As far as the case history is reviewed, you know, we have looked at a couple of these, but haven't spent really too much time. There is a Pacheco Pump Plant where there was some unique grouting occurrences that were needed there. The VAT Tunnel which was also in I believe Utah, in which they encountered some very high water producing zones, a thousand gallons per minute in which they had to grout off that particular zone.

These other areas, basically grouting operations for Helms Pumped Storage and the Dam histories and as far as the Sullivan Mine, some pneumatic stowing operations. So it is our intention to complete the review of this available technology, amplify a little bit on it over the next year and find out how short we are on knowing what the potential properties of these materials would be based on case histories.

With that, I'll go ahead and close this portion of the presentation and hand it over to Mike Hardy, if there are no questions.

DR. DEERE: Don Deere. I'll just make a comment on the New Waddell Dam. They have used three methods of cutting off the potential seepage from the new reservoir when it was created and only one of them is grouting. The other is a very unique method of excavating a deep trench, this is in the alluvial area, and placing panel walls, so they essentially have a continuous wall going down over 100 feet deep that crosses the old channel.

Another is an area of conglomerate where there has been weathering and then removal of material along widely spaced joints. And these have been cut off by overlapping 24 or 22 inch boreholes, each one backfilled with concrete and then another one and then they come back and do one in between so that they overlap the other.

The third way is a triple-way grout curtain in the basalt areas which most of the heavy grouting of that type will not be under the dam, but in a saddle which has a very narrow leakage path from the future reservoir to the downstream area. So, that is very costly, very expensive one because there are so many large voids in the granite. But, I think it is very worthwhile to visit.

MR. FERNANDEZ: I also think that out of all the case histories or places that we have visited as far as grouting, that one perhaps offers the most unique challenge, because as I understand it, the grouting is done in an unsaturated formation.

DR. DEERE: Yes.

MR. FERNANDEZ: Most other grouting situations were in highly saturated conditions. So, I think there may be some unique challenges that they are facing that may be very appropriate to incorporate into our program, or at least consider anyway for our program.

DR. DEERE: Yes. But almost every dam has the abutments in a non-saturated condition.

MR. FERNANDEZ: True.

DR. CANTLON: Questions?

(No audible response.)

If not then, Mr. Hardy, we are ready.

MR. HARDY: This is essentially another presentation I am going to give. I am just briefly going to touch upon the regulatory requirements that relate to backfilling the repository. As Joe has already mentioned, they are not direct requirements for the thermomechanical aspects of backfilling the repository.

I am going to just briefly review the thermomechanical environment the drifts will see during the post-closure period. I want to just identify a few mechanisms that we need to consider when we are thinking about the performance, the mechanical performance of the backfill. Then, I will touch on the potential backfill performance requirements or how to develop those requirements. And also I have a heading in here that wasn't in the printed version, but that says operational constraints and they are the same constraints that Joe has mentioned on a previous view graph.

The regulatory requirements as you trace them down through the SCP through the performance allocation process you come to some performance goals, and the performance goals are listed that the backfill can impact. For example, there are a number of requirements, performance goals that limit deformation along faults coming up from the bottom, limits subsidence potential so that the surface doesn't get deformed to form any ponding or pooling of water. Limit changes to permeability of rock mass. That is a goal that comes up a number of times. In some places it has a very restrictive requirement, and of course. limit deformation around the drifts is least locally related to that same requirement.

Just briefly, we have looked at the thermomechanical environment of the drifts fairly closely, at least in a predictive sense for the pre-closure period, but there really has not been much attention to the stability of the drifts, the deformation around the drifts and the postclosure era.

This picture shows basically over a long period of time the temperature history in the SCP-CDR base case, 57 kilowatts per acre. The roof and the floor in this environment reach about 100 degrees centigrade in the first 100 years, essentially will stay about that temperature for another 1,000 years and then drop off with time. This waste package temperature environment is not entirely correct; that is an average temperature down the center line of the drift, it clearly goes a lot higher than that.

If you are looking at the stress changes at the location of drifts, this one over here shows the stress history of the location of the drift for an emplacement drift, not including the stress concentrations caused by the shape of the drift itself, but you can see we start off with fairly large stress levels, vertical stress of 7 or 8 megapascals. The vertical stress doesn't change very much with time, but the horizontal stress changes from the 3 to 4 megapascal range up to the 16 megapascal range. So we are changing from a very large stress field to a relatively high stress field.

The tuff main drifts are in an area that is somewhat protected from heat. It doesn't get heated up so much, but it is still influenced by the development of high horizontal stresses. In this case though, it is in a low temperature area and the vertical stress declines somewhat, comes back up to reasonable pre-existing vertical stresses after a long period time. But the horizontal stresses see a reduced horizontal stress increased then the emplacement drifts, but it is still a significant change from the 3 or 4 up to the 9 or 10 megapascals. These numbers are not absolute and there is great reasons for variations in those things.

I want to just touch on the mechanisms of deformation that occur around the drifts initiated by drifts so the backfill if we placed it in there could reduce those mechanisms or be controlling.

I have listed them in the same order that I touched on in the first view graph. But one of the most obvious reasons to backfill is to fill up the void with some materials, to limit the amount of void volume that could migrate in some way by roof failures. I am not sure if the pictures do represent conditions that we think are going to happen in the repository, but they are the traditional forms of subsidence in a longwall where you have a lot of ore, an underground mining operation where you have a lot of extraction here. You've got a regional collapse and subsidence of the surface. This sort of condition is not credible for a repository environment where we have individual drifts quite widely spaced. We don't expect to have failures of pillars. This is typical of high extraction.

But, on this case here, we've got a piping or more localized type of failure. This thing can happen from a relatively small opening, usually associated with some geological feature and sometimes those conditions can go high, sometimes being expressed on the surface. This is fairly unlikely in this case too. I think the geology locally is not likely to have this. But, the way to overcome these in most mining operations and tunneling operations is just to backfill with a sand, gravel or rock or material with no very specific rigid mechanical properties.

Another mechanism that backfill can help to minimize is development of yield zones around drifts or development of enhanced permeability around the drift itself. The picture just shows the result of analysis around emplacement drift is the center line. And the darkened out area shows regions of yield around the drift after 100 years of heating. This picture shows that after some further time when the rock mass properties have been decreased, not actually to zero, but to half the conditions for this case, the zone of failure increases and potential rock fall out is obviously enhanced. When applying backfill in there it is going to stabilize the blocks falling out. And if you wanted to put in a very still material, you could also limit the growth of any yielding zone around the drift.

Another mechanism that might be considered, the backfill might make some impact on, is if we had emplaced a waste package or waste container in the floor, if we were to get failure in the floor that might cause some macroscopic large scale deformation in the floor, that impact the canister, may be increasing the load on the canister and precipitating some sort of buckling or excessive deformation or stress corrosion cracking, and those sort of things.

If you backfilled in this case to limit this sort of condition, then you would need a fairly stiff backfill to resist this lateral closing of the drift.

Now the fourth mechanism I just want to briefly mention is faulting or slip along joints, and in general we consider that the joints are vertical and horizontal but we know there is also background random joints and in some locations how they are oriented. This line here shows the safety factor of one around emplacement where a 45 degree joint is likely to slip out to here (indicating). These ones have safety factors greater than one; they are just left off the figure. That is for a jointing situation.

Here we are looking at slip along a fault, a low angle fault. We don't know that there are any of these that exist. We haven't mapped any of theses, the faults that exist are expected to be near vertical. But with the increase in horizonal stress, there is likely that a fault, if it existed like that, might move. In that case, which has got a loading of it representing 100 years of heating, didn't show very significant faults. It was only out to this distance. But, in this case, this was a fault located like this that was loaded by the in situ stresses and the potential seismic loads, but no thermal loads. Thermal loads when they are increased at this orientated fault tend to stabilize that fault and increase the horizontal stresses.

Now, it is difficult to relate, this is a view graph that I didn't have in the proceedings, that I wanted to be able to just use this for presentation purposes. To develop the performance criteria, and I think we have to look at these mechanisms of deformation to establish the mechanical properties of the backfill. But, it is part of the strategy for the development of the backfill. Hydrologically considerations have been discussed more directly this morning in terms of redirect flow. And so far, what we have come up with, is the backfill in general could get away with a permeability as low as 10^{-2}. That is about the only leading criteria hydrologically that we have at the moment. Of course there is a number of barriers and things to redirect flow and you'll talk about the technologies that develop those later on, or to emplace those things. But, the hydrological criteria at the moment for the general backfill is still a very permeable backfill.

Mechanical objectives, they've got these four kind of performance goals. We never look at the mechanisms to see if backfill significantly impacts those things and do a trade off to see if you significantly impact one of these things. It requires a backfill that has a really extreme mechanical properties. We have got to look at the benefits first and the compatibility with that sort of backfill with the hydrological characteristics.

Well let me say that the criteria that are clear through the SCP is we want to backfill the voids. We haven't said whether we are going to backfill them with marshmallow or air. Well we have gone further than saying air, but in terms of the mechanical properties, they are not well-defined. So, we have to look at these mechanisms of deformation and see if any of them are going to be significantly impacted by backfill and what requirements that places on the backfill mechanical properties.

The backfill operationally constraints, I have just alluded to the compatibility of hydrological and mechanical requirements if it turns out that there is a requirement for a mechanical property that has got a high modulus, then it is difficult to make a high modulus material that is also very permeable. But it may be that it just has to be a low permeability at 10^{-2} and you could quite happily live with a low permeability.

Placement timing is a thing I want to mention. There are various aspects of the repository that could be backfilled earlier and there are other aspects of it that need to be part of the closure process.

Temperature, just to recognize the temperature will be increasing at least in the emplacement rooms during the preclosure period. So, when we are doing the backfilling processes, we have to either cool down the areas by a blast of cold air and then maintain a ventilation surrogate when we are backfilling and retreating or else there could be some remote handling operations or men would be in cool down suits or restricted to air conditioned vehicles.

Interaction with ground support system, Joe mentioned that we have to establish his removal of ground support systems required. I have referenced sort of considerations about boreholes and removing things from boreholes. If that becomes a requirement, we might have to remove rock bolts and shotcrete or concrete in certain locations before backfilling and sealing.

Of course, selecting the general backfill material, the thermal and geochemistry considerations has to be taken into concern.

I wanted to just touch on backfill placement timing. There may be a need to respond to unfavorable conditions during exploratory drifting. And later on during development of the repository and fill in drifting, there may be areas that you want to backfill early. Routine backfilling, an early backfill can be considered in some places in the repository. Actually, I think the Calico Hills is more a concern that would be considered up here in the exploratory drifting than actually the part down here.

In the routine backfill, there is a number of options to look at, whether or not early backfill has some advantages in some places, but generally, the SCP-CDR the basis that we talk from at the moment considers that you backfill after the retrievability period. So, all drifts will be open for the 50 year retrievable period. All drifts at least to the repository horizon. I am not sure of the status of the Calico Hills drifting, whether people want to maintain those open for monitoring.

But just for consideration or timing of backfill placement in the waste emplacement drifts themselves, clearly there is a trade off to evaluate whether you want to consider backfilling prior to closure of the repository. And here again we've got the temperature history and the stress history and the emplacement drifts. At the early stage you've got relatively conventional conditions in which to place the backfill. The temperatures are reasonable and the stresses haven't developed. It is an easier proposition doing that than having to cool down the repository if you are doing it in this 50 to 60 time frame when things are heated up significantly.

But clearly the drawbacks of doing that is if the retrievability option is required, you need to retrieve, then you have got to mine through some backfill. One of the other concerns may be that if you backfill early you are putting a lot more instrumentation and you will have instrumentation streams coming out through the backfill. That is something that you might be able to avoid if you backfilled light.

But, I just want to emphasize that the strategy at the SCP-CDR is to backfill early, but I think there are tradeoffs when considering the performance of the backfill; the function of the backfill is to consider or to evaluate some early backfilling.

That is the completion of my presentation. I'll be happy to entertain questions.

DR. CANTLON: Board, any questions?

DR. DEERE: Excuse me. In your last comment, you said you felt there were advantages to early backfilling?

MR. HARDY: Yes. I think there are advantages; there are obviously drawbacks. So, it is a trade off that has to be done versus retrievability. To retrieve, now you would be mining out some--one of the advantages is you don't have to maintain stable openings for 100 years or 50 years that requires performance of your shotcrete or your rock bolts or your other materials as yet unspecified to maintain the stability of the drifts for a long period of time.

If you backfilled early, you then avoid that requirement, and if you do need to retrieve, you then have to mine through some very relatively soft and easy mining material. So it is not a significant problem to mine through that material.

DR. DEERE: Thank you.

DR. CANTLON: Other questions from the Board or Staff?

Russ?

MR. MCFARLAND: One question. You make no comment about the influence of diameters, geometry, turn out radius, the design of the repository impact on the potential bore instabilities. Is there a reason for that?

MR. HARDY: In terms of the shape of the drift itself or the radius of turn?

MR. MCFARLAND: The geometry of the repository, the detailed designs, what you would recommend in terms of dimensions, shapes, pillar sizes as a function.

MR. HARDY: There is a consideration. I think actually we might bring up one of his view graphs that is showing a large circular opening, to give you this dimension that is vertical which means you have a larger horizontal dimension than something vertical. And also from the point of view of diverting flow away from the waste package, having a floor that is shaped like this, is not the best shape. Let's say a flat shape or one that diverts up to the side like some of the pictures that Joe showed early. So there is a configuration of a tunnel boring machine in here, but at the time you are constructing, you shave off a flat floor or a flat floor that's got a ditch on one side may be advantageous. It is certainly better than coming in with a bigger hole then putting in a flat floor to work off, and then we've got a sealing problem of diverting flow away from that canister.

That is just on the shape of the opening itself. A circular hole of course is a very stable opening and Archie is going to show a picture of a circular hole juxtaposed to a drill and blast hole to show that it is easier to seal around, a tunnel bored machine hole than an irregular ratty shaped hole.

But in terms of the repository layout, the considerations for the general geometry, the way it was before in terms of the SCP-CDR is you have a mid panel emplacement drift and then cross-cuts off that. So you have relatively short emplacement rooms. So, if you abandon one room because of some unfavorable condition, you are only abandoning a short distance. If you have hot long accesses that go for several thousand feet or a thousand feet or more, as I see in the latest layout, which is a tunnel boring one, if you have a problem halfway down, you may sterilize the rest of that drift. So, those sort of things interact with the sealing because either you go through them and we seal them on the way back or later on, but they also are concerned, you have got to take into consideration and thinking about the flexibility of the particular layout that you select.

DR. CANTLON: Other questions?

(No audible response.)

All right. We are down to the last speaker. Archie?

DR. RICHARDSON: My name is Archie Richardson and I work with Michael Hardy at J.F.T. Agapito & Associates in Grand Junction. I am going to talk about the last subject of the day, last but not least, the available technologies to seal underground openings. As Joe pointed out that this is some work in progress this fiscal year, we hope to finish up.

As an overview of the presentation, I am going to briefly refresh your memory on the sealing components that Joe introduced earlier, talk a little bit about preconditioning of sealing areas, then discuss some of the backfill material preparation and handling methods, briefly touch on backfill placement methods, and then give an example of typical seal component emplacement.

The first of the sealing components is of course the general backfill. As the earlier speakers mentioned we are looking right now at a crushed tuff, but this could have a variety of additives if required.

Other components other than the general backfill are emplaced within the general backfill in specific areas to alter the drainage and direction of water flow, if any. This particular one we are calling a single embankment with a keyway and a grout curtain. This is one of the more complex concepts that we have come up. And as Joe pointed out earlier, we may not need to be this complex; we may be able to get by with a simple contrasting backfill materials. I am going to talk a little bit more about this one because if we can emplace something this complex, we can certainly emplace simpler concepts.

Another one of the sealing components is the repository station plug, which is probably a cementitious bulkhead keyed into the rock at the station and they require some extensive grouting. This would accommodate an extremely unlikely inflow of the situation where you had water in the shaft.

Another sealing component that we looked at earlier would be a water control concept emplacement area where you have a waste emplacement hole and some sort of a graded filter which would divert any possible water that would be in that area off to perhaps a drainage enhancement hole off to the side or in a ditch or whatever to collect the water and help it drain out.

Finally, we are looking at a backfilled sump, which the function of this is to increase the retention time and promote drainage or any water that you get into the drift, for instance from a fault in that area. So these are all localized components that would occur within the general backfills.

I would like to talk a little bit about preconditioning sealing areas. I would like to start out by talking about excavating surface conditions. The upper slide that is a little bit dark, but it is a picture of a machine board mail haulage drift up the Stillwater Mine, which is a platinum palladium mine in Montana. You can see that there is a very nice smooth circular excavation opening. This is in a hard igneous rock, relatively massive igneous rock.

The lower picture is a typical example of a drill and blast excavation and that occurs at the Amethyst mine in Creed, Colorado. This is actually in jointed welded tuff formations in the San Juan Mountains near Creed. You can see there is quite a bit rougher texture to the walls of the opening. So when we are looking at sealing a particular location, there would probably be quite a bit more preconditioning that we would have to do in a drill and blast area versus a TBM situation.

What might we have to do to precondition specific sealing areas? I have shown a little cartoon here of an area of where we might have to do some preconditioning for emplacing a specific seal component. Some of this preconditioning may be done even during the time, very early during the time of repository construction, and others might be done during decommissioning of the repository. We might have to remove support systems and infrastructure like concrete liners and temporary floors, if there are any temporary floors that were installed during construction, we might have to remove those. Also, perhaps even in some cases remove some of the rock bolts.

We might wish to alter the shape of the floor by additional excavation, perhaps put a ditch in the side, drill some drainage enhancement holes in certain locations such as the drainage sumps and do other surface preparation that may even involve some cleaning.

The different materials that we are talking about using for backfill or currently considering, the first which would be a bulk granular material which again is the crushed tuff. The second would be what we are calling a bulk cementitious material if there are any specific areas that require a cementitious seal. Then we have some specialized materials such as grouts and clays which will be handled in much smaller quantities, probably.

Speaking about the bulk granular material, I would like to make the comment the mined material size gradations are dependent on the method of excavation to a certain extent and that there are additional fines that may be generated during handling so that we may have to perform some sizing operations on the surface for the backfill.

The little figure here shows some typical differences one might find in tuff mined using a roadheader type machine. You would probably get more fines that you would with a typical TBM. With a typical drill and blast operation, you would end up with a wider range of gradations from some very fines up to some quite large oversized material. Of course, there are different gradations you can get with any of these methods, but this is just to illustrate the fact that the method you use to excavate the opening has an influence on the kind of muck that you get from it.

To carry this concept just a little bit further, this is kind of a life cycle diagram of the backfill. And, you would be excavating either non-welded tuff or welded tuff with a variety of methods and transporting that to the surface. If you were using drill and blast techniques, you may have to perform some primary crushing of the material before you can remove it to the surface, especially if you are using conveyor belts. Some decision will have to be made as to whether or not a particular material suited to fill porefill, it is not it will go to permanent surface disposal area. If it is suitable, it may be segregated and stored temporarily on the surface, and temporarily may be quite a long time; 50 plus years.

After this there will be reclaim operation and then it will pass to a sizing plant which is probably part of the backfill plant and in this sizing plant, the fines may be removed and rejected back to the permanent disposal pile. There may also be some crushing of a particular size; gradation is required for the backfill.

Now this means that some material will be removed depending on the volumetrics and how much materials are being removed. Some additional makeup aggregate may have to be supplied from another source. Also, there may be some important materials added to the fill depending on the characteristics required. Finally, the fill will be transported underground and emplaced.

Briefly touching some backfill placement methods, this is an overview of methods that have been used to place backfill underground in mining operations. I might point out that placing backfill is nothing new either in surface above ground civil operations or underground civil and mining operations. There is a wide variety of available technologies, and which particular ones that are selected depends on the characteristics of the fill you want and the particular circumstance.

We have in order of applicability to the repository, we first have the mechanical emplacement compaction methods which are basically using trucks and rubber-tired equipment and similar equipment. Then we have what I have been calling the throwing methods, which include the pneumatic, stowing and belt slinger methods. Then we have other methods that I consider less applicable to repositories and these include the gravity emplacement. The reason why gravity emplacement is less applicable is because, it requires boreholes from the surface. We are trying to minimize the number of additional openings that we have to seal preferential pathways to the surface. And, also hydraulic methods which are very, very widely used in the mining industry to emplace fill but require fill emplacement as a slurry and introduce large quantities of water back into the repository.

Speaking of mechanical backfilling, these are some photos taken at Asamera's Cannon Mine up near Wenatchee, Washington where they use a cut and fill method of mining. In this case, they are placing a cemented backfill with about 8 percent Portland cement and they are achieving strengths of about 1,000 psi in this mixture. But, similar methods could also be used for uncemented fill.

They use a rear-dump truck here to transport the fill from an underground pug mill to the general area of emplacement. And you can't see very well in this picture, but I have some cartoons later on. This is a load hole dump unit which is similar to a front-end loader used underground. Attached to the front of this is a pushing plate which can be used to push fill-up close to the back. One of the problems we have underground that we don't have in backfilling operations on surface is the problem of clearance. You can't get on top of the fill because of the roof. So the question of how close the fill has to be to the roof is very important when it comes into talking about how you are going to place it.

Now I took this picture at the Cannon Mine. Here is some cemented backfill that was emplaced using mechanical methods, and you can see very well, but there was about a two inch average gap between the fill and the back. And that is how good they could do it with that modified LHD.

Another aspect of backfill placement is compaction. There is a variety of equipment that is used for compaction in surface operations. Very seldom is fill ever compacted if additional strength and density is required; typically cement is added. Compaction is a labor intensive process underground, so it is not generally used, but it can be done. We have a picture of a hand-held vibratory rammer which would be suitable for granular areas in confined spaces. And of course, it is very labor intensive. You would have also a vibratory roller in talking about vibratory compaction. This is a picture of a surface rockfill embankment vibrating the face of a rockfill embankment. You can see underground in some situations you might have clearance for this type of an operation and in some situations you might not.

For clay-like materials we have a picture of a small tamping compactor. An again, underground if you had a fill with a very low angle and you had enough clearance to run something like this and there was enough extent to justify it, then you could using something like this. Otherwise, if you had a steep layered fill that was up close to the back, it may be more difficult to use something like this. And the question arises as to whether we need to compact the fill underground at all; we will have to do some additional work to decide exactly what characteristics we need for a fill.

Here is a chart of compaction. I don't want to get into any great detail other than to show that there is different kinds of compactors that are appropriate for different types of materials. So you have to make sure that you match the compaction method to the type of material that you are trying to compact. So in terms of our sealing components, some of the impermeable components like the clays, would be done in this range here. Some of the filter materials like the sand, you would be in this range; the general fill would be in an entirely different situation, so that you might have to use different methods of compaction for different situations.

Talking about the throwing methods of placing backfill, we have pneumatic stowing and it is a little dark, but you can see the end of a pipe, pneumatic backfilling pipe. Basically pneumatic stowing works by a rotary airlock feeder is used to feed material into a pipe and then pneumatic pressure is applied to the pipe and it actually blows the fill out the end and you can fill very close to the roof using this method. It works well for minus 3 inch material and you have some trouble with abrasion in some materials, so you have to perform tests on the material you are going to use to see if this is going to be a limiting factor or not. We think that crushed tuff would probably blow fairly well with pneumatic stowing methods.

The previous picture was trying to show construction of this device which was a ventilation stopping in an oil shale mine. This is muck that was stirred with a front-end loader, mechanically stowed, but it is hard to get it close to the back, so they used pneumatic stowing to blow in tight to the back. You can see how this concept is similar to a larger scale backfilling operation, even though it is a limited ventilation stopping.

Now just briefly touching on one of the other methods, or some of the other methods, gravity and hydraulic, this is a schematic of backfilling in an abandoned coal mine where you take the coal mine refuse and crush it to a stockpile and then reclaim from the stockpile, mix it with water, pump it into a slurry through pipelines underground and use it in this case prevent subsidence and fill and underground coal mine opening. This is one of the methods that I mentioned could be considered but would be probably be less appropriate for repository use.

The other throwing method is a belt slinger. A belt moves over pulleys in such a fashion as to take this granular material from the hopper and throw it up onto the pile. This is used in an underground iron mine in Germany, Megan Mine. It is has been attempted in several other mines in the United States. It works well if the granular materials are dry. But if you get some wet materials, it doesn't really sling them, it just makes a mess.

The final thing, I just want to show a few pictures of some specialized seal component placement techniques. I want to talk a little bit about those and then show a few pictures on our construction sequence.

Some of the things that we may have to do to emplace some of these specialized seals includes cutting slots in rock. It sounds very easy but if you have to do it without blasting, it can be quite a chore. Drilling holes and then placement of massive cementitious seals in grouting. I am not going to elaborate much on grouting, but John Case is going to talk a little bit more about grouting tomorrow. It is a talk that we are going to cover in our future work.

Cutting slots, one of the classic methods of cutting slots without blasting is the drill and broach method which we have some experience with that at G Tunnel and it involves drilling a row of holes, broaching these with a jack hammer, broaching the slots out between the holes and then you get a regular slot. Then you drill a second row of holes to widen this slot and put a hydraulic splitter in individual second rows and break the rock into the first row of holes and you end up with a final slot. It is somewhat irregular, but it doesn't require any blasting and there is many variations on this theme.

Something that Sandia has been developing over the last few years which shows a lot of promise is a diamond saw. This particular version of it is one that we worked with in Grand Junction in a recent project. The saw can cut rock. This one is designed to cut a two meter slot and it can be mounted in a variety of fashions. And you can see where you can cut a slot into the wall or into the floor with one of these perhaps mounted on a truck or mounted in some similar fashion.

In the last three slides here I want to show a typical construction sequence. I have picked the single embankment, the graded filter, because it is a fairly complex one and just showed a series of cartoons. First we emplace the general fill up to the general area where we want to build the embankment. And we've kept a fairly shallow slope here so we have a place to work on.

The first thing we do is excavate the keyway slot and I've shown one of the diamond saws mounted on a truck, but there are other methods we could use to excavate this slot. You could excavate an initial slot with the saw and then cut a series of slots and break out the intervening ribs or there are various ways to do this. All of them are fairly time consuming.

Then you might wish to drill and grout this little keyway and really get a good, tight seal. Then you would come in and you would want to emplace and compact our filter material that was finer than the general backfill, but not so fine as your impervious layer. You might want to compact this with a vibratory compactor, a hand-held compactor. Then you emplace the impervious layer. One convenient way to do this would be using pre-compacted bentonite blocks, but it could be a variety of other materials or it could be a layer of clay or the final design is not yet decided on this. I have shown here you could use pneumatic stowing to emplace the front section of the filter and then you would go on with your general fill and continue on down the drift.

So in conclusion, I just want to say that there are a variety of available technologies and we are looking at more of these trying to determine which ones are most applicable to the repository. Much of the specifics of construction depend on the specifics of design and the criteria which we haven't finalized yet, so we will be able to get more specific with these methods in the future.

DR. CANTLON: Okay. Board questions?

One question, is anyone looking at the design for a horizontal packer? It seemed to me that since you are going to have a very long-life need for some of this when you get to the backfill period, a horizontal compactor would be a reasonably easy thing to design.

DR. RICHARDSON: I think it would be relatively easy. I haven't yet seen one in operation.

DR. CANTLON: No, I haven't either. It's pretty obvious.

DR. RICHARDSON: But, it would seem that you could mount some of these existing machines on a rubber-tired piece of equipment without too much difficulty.

DR. CANTLON: Or a track would be better for giving us a more stable base.

DR. RICHARDSON: That's right. Something I would like to point out is, whatever method is selected, it would have to be demonstrated probably prior to license application time.

DR. CANTLON: Sure.

Questions from the audience?

MR. BLANCHARD: I would like to try to answer the question that you posed a few minutes ago about, now that we have extensive drifting within the Topopah Spring as well as in the Calico Hills, do we really need significant surface-based test program over the Yucca Mountain proper within where the repository perimeter is currently conceived?

We have been analyzing that and part of the difficulty in coming up with a conclusion is tied to the fact that all of the comments we have received on our site-characterization plan have never suggested we should not do something. They have always added more. That makes it a little sticky there.

The other thing is, I think we need some degree of confidence that we can predict the stratigraphy. If it turns out we drill some holes and find out that we don't encounter the boundary between the Topopah and the Tiva, or between the Topopah and the Calico Hills and maybe we don't have as good a structural understanding as we thought we might, and more stratigraphy needs to be acquired, which can best be acquired by vertical boreholes rather than drifting in the Topopah

Springs. So, I don't think we have an answer for that yet, but we are looking for it.

DR. CANTLON: Thank you.

We are adjourned.

(Whereupon, the proceedings were adjourned until 8:30 a.m., November 13, 1991.)