UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER BOARD MEETING

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375 East Harmon
Las Vegas, Nevada 89109

ADDRESSING UNCERTAINTY
REPOSITORY SAFETY STRATEGY
SCIENTIFIC PROGRAMS UPDATE

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I N D E X

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P R O C E E D I N G S

8:30 a.m.

COHON: My name is Jared Cohon. I'm the Chairman of the Nuclear Waste Technical Review Board. It's my pleasure to welcome you here to the second day of our public winter meeting. I want to acknowledge again the presence of Senator Jacobson, president pro tem of the Nevada State Legislature.

Are there any other elected officials with us today? Thank you, Senator Jacobson, again, for giving us your time.

This morning's session will focus on the repository safety strategy, a central set of issues for the program. There will be a public comment period at the end of the morning session, which we estimate to be at approximately 11:35. And we'll go to about noon or until you run out of comments. And there will be another public comment period at the end of the day, which we now estimate to be at 6:00. That's when we're guessing we'll end today.

The chair of this morning's session is Paul Craig, a member of our Board, and I turn the podium over to him now.

Paul?

CRAIG: Thank you, Jared. Repository--one, two, three, four. Now it sounds like I'm there. Okay.

This is repository safety strategy, and our first speaker is Jack Bailey, who's on for a half an hour. And the procedure we're going to follow is that a few minutes before it's time for you to stop I'll start to wave, and thereafter comes the hook. So we're going to try to stay on schedule--we will stay on schedule.

Jack Bailey is director of regulatory and licensing for the Management and Operating Contractor. He's responsible for implementing and defining license strategies for M&O, including technical approaches as well as developing a nuclear safety and quality culture. And he got roasted on this subject, he tells me, only recently. So it's an interesting one for me.

Mr. Bailey will provide us an update on the evolution of the repository safety strategy. Welcome, Jack.

BAILEY: Thank you. I will be speaking on the update of the repository safety strategy. At a fall meeting Michael Voegele discussed with you the initial development, if you will, of the safety strategy, the identification of the principal factors, and how we arrived at the safety strategy.

This week we have finally pushed that system--that document through the review cycle, through the publishing cycle, and there are some available in the back of the room. There were yesterday, as well.

Rev. 3 was developed following the LADS work, the LA design selection work that we did last summer. It was developed based on some preliminary analysis, which I will describe in the course of the 30 minutes that I was allowed. And it is an ongoing process, as I'll discuss.

This takes Abe's slide from yesterday. Abe talked about managing the uncertainties, analyzing the quantified uncertainties, assessing all the uncertainties and communicating the uncertainties. What we tried to do in the RSS is capture each of those activities.

We have not tried to capture the specific analyses that perhaps led to that, for example the managing of uncertainties occurred during the LADS development work. We made some decisions and some selections of design approaches of what we should rely upon during that process. They're reported in the repository safety strategy. That's what I'm going to try and talk about today and explain.

The general elements, we summarized the status of the postclosure safety case. We look at--and you'll see a few slides later--how we assemble the important parameters, the important aspects of that safety case. We listed what we call principal factors.

To hearken back to yesterday's discussion, that's how we focus on what is most important in this what we call a safety case, in our evaluation of this system. What are the things that really make a difference, so that we can examine the uncertainties, we can examine our understanding.

Now we describe the strategy for the updated postclosure safety case, and I'll hearken back to Bob Bernero, who said "Understand the body of knowledge." And that's what these five things are trying to do.

The first is extremely important, and that is the performance assessment. That is our tool where we do our evaluation to gain understanding, and that's what gives us a number, if you will, to compare to the standard. It also allows us to do a variety of sensitivity studies and gain understanding of the total system and what's most important in the total system.

As Abe said yesterday, it takes us a few months to do the TSPA and months to put together the analysis of what we know. In addition, we looked at safety margin and defense in depth. And you can look at safety margin in a couple of ways. One is what's the separation from the standard both in time and in dose? Are you close to the standard?

As Warner North said yesterday, if we're arguing 24.99 or 25.01, we're probably talking about the wrong thing. So how close are we to the standard and when does it occur? We have to look past the 10,000-year regulatory period to make sure that nothing falls off the table, that something happens at 10,001st year or the 10,002nd year. So we need to look at that whole picture and gain an understanding there.

There also is a margin piece which wasn't discussed yesterday, and it's not discussed in great depth in the RSS but is inherent in everything that we do. And that is that the goal of the modelers and the goal of what we're trying to accomplish with the study is as we build models we want them to be realistic to conservative. Nothing different than that. Nothing we would call optimistic.

Let's take an answer and say "Let's see how good we can make it." Or "Take it out of a peer available--is it realistic?" We really want it to be somewhere between realistic and conservative, which means that those numbers that you see, the means if you will--if we've done our job right--are realistic to conservative.

And there's a number of these, and I'll talk about a couple of them as we get to the factors, where we know we're taking very conservative opinions. And when we look at the findings that we have from our peer review panel, from our technical reviewers such as you and others, and expert elicitations, our criticisms were "You're doing things too optimistically. It's going to behave more conservatively than that," and we're really working to take all these analyses from a realistic to a conservative nature.

Now when you take everything to a conservative nature you start hiding knowledge because you may bury an understanding inside of a conservatism. And I'll show you one of those in a few minutes. And so we have to keep our mind open to that, to consider it, and the sensitivities are interesting. Your sensitivities can be hidden by being too conservative. But in a margin sense we have to look at making sure we stay in a realistic to a conservative mode so that we can have confidence in that mean value that we see.

Defense in depth was discussed at some length yesterday. Layering is another term for that--how many different ways do you have to accomplish your goal? And we'll talk a little more about that.

We have to do an explicit treatment of potentially disruptive processes. In the reactor business that's the low probability, high consequence event. Do we have something that really creates a problem that would make this a no-go?

We look at natural analogs as a means to make sure that if available is there something out there that gives us a longer term understanding that our processes are going to result like this, either at the subsystem or the system level. And that was discussed briefly yesterday, and we have some talks this afternoon of some of those investigations we're doing.

And a performance confirmation program, which to meaningful has to replicate conditions that we're going to see in the future, so that once again we gain an understanding of how the whole system will respond.

And then finally the RSS provides plans to update the technical basis. We did this last summer and we're to guide our planning. What is it that we need to do to move forward? Where should we focus limited resources and what's most important?

Revisions to the safety strategy--I'll point real quickly. You can see the viability assessment, volume 4, had a table and a section--actually all of volume 4--that says what's important, how much do we know, how much more can we learn, and how do we move forward. That was kind of a first cut at what we were doing in the repository safety strategy. We issued Rev. 2, which identified some our findings in that regard. It was more detailed than in the VA.

The EDA, which we did last summer with preliminary analysis, we did the same thing. We assessed information needs and there very easily could be an error right here that says we made decisions. What did we choose to rely upon and why? Where did we choose to focus our resources?

And every time you assess your information needs you make decisions. You'll notice you have an evolving technical basis because you learn more, and you continue to learn and you continue to revisit. What is the case, what did we depend on, has what we depended on changed? And we'll go and do it again for the SR.

Today we're going to talk about Rev. 3. We updated the safety case from the VA because we got increased site materials knowledge, and I believe that Tom and Bo will talk to some of those pieces. There was a changed regulatory framework. 40 CFR 197 came out in draft, 10 CFR 963 came out. We had to consider those.

We enhanced the repository design. We looked at a modified thermal management approach because of uncertainties. Sticking to the theme of yesterday, heating the entire block up created a lot of uncertainties. Where did the water go? When did it come back? What happens? Keeping an idea of a pillar between so that it would drain, similar to what we're seeing in our drift scale test, seemed to be a better design.

A more robust waste package--we had a waste package that had an outer layer of carbon steel, an inner layer of the corrosion resistant. And we're trying to accomplish a couple of things: one, provide mechanical strength; two, get through the thermal period so that we could keep the kinetics, if you will, the high kinetics of corrosion, off of the package.

And we created a number of problems because the uncertainties associated with the steel C22 interaction. And so we came up with a different design: turn it around, put the corrosion piece on the outside, get the structural strength on the inside so that we can A, lift it, and B, protect ourselves.

And then we were in how do we get past the thermal period, and the drip shield came to mind as a defense in depth mechanism, which I'll talk about briefly; and it's right there, the drip shield for getting us through the thermal period, keeping water away, making a diffusive relief if anything happens to the waste package.

And finally the potential for backfill for mechanical reasons. We conducted preliminary TSPA and barriers importance analysis. What we did is we took the

VA, TSPA and we modified it enough to capture what we believe were the pertinent aspects of this design so that we could move forward.

Now unreal cases--we talked about that a little bit yesterday--and that is doing analyses which are perhaps not valid in space because they can't really happen. But they teach us something. And this is one of those, and these are done with preliminary models again, as I said, and this is only using mean values. This is not a probabilistic solution.

We took and said "Well, what if we take all the waste there is and we lined it up and we put it in water and take it to its solubility limited values, and we provide it to the biosphere or the VA?" What kind of dose will you see for the people? And you can see it's a pretty significant dose--not a real case. But it gets you an idea of what's totally out there.

We then said "Well, let's put it in the mountain, 1000 feet underground, let's grind it up and throw it in the drifts, no clad--nothing--just throw it in the drifts, and let's let the natural system do its thing." And you can see significant reduction because of the performance of the mountain alone. Many orders of magnitude in the early stage and the late--significant.

Then we went and said "Let's put it in a waste package and let's take the nominal behavior of that waste package as we understand it now," and you can see that you went out a very long period of time before the waste package started to fail. And the natural system did its job, the waste package did this, and you push the answer out again.

And then we said "Let's do it one more time, and let's put another piece, the defense in depth of the drip shield," which moves the waste package out of the high kinetics of the thermal pulse which occurs back here, let's use the drip shield in that time frame, let's protect the package with the drip shield, and what do you get? You get no release for 100,000 years in a nominal case.

So when you put all of those together you can see that you have a fairly robust system at a nominal case. This slide does pretty much the same thing, small number of relatively mobile nuclides. The system uses multiple natural engineered barriers. That's what it does for us, and that's a very simple calculation that we did.

In revision 3 we did two kinds of analyses. We did what I just described as the nominal scenario, and that is take everything at average and let's see how it works. We got the answers that you saw. At 100,000 years not much is happening. If we believe the numbers--not look at the vast body of knowledge--if we believe the numbers it's time to go home. We made it, 100,000.

We have to look and say "What can go wrong? What are the uncertainties? What if?" And so we went back and we did another piece, and we did these barriers analysis. We did another, and said "Okay, for purposes of examination let's take one waste package failure."

Let's say it has failed basically at the time of emplacement, and then let's let the nominal behavior from the point carry on, with one exception--which Abe talked to you yesterday--and that is, is we took that waste package and we made the first failure under nominal performance always occur in the drip shield directly above that package.

And the seepage of course was occurring at that same spot. So we created a conditional probability which is fairly unlikely, but it gave us the ability to look and see what happens if these engineered barriers don't work as fully as we thought? What if the 100,000 is not real? Let's start examining the body of knowledge again and do it in that manner. So that's what we did.

Now we went and ran a series of what we call barrier analyses, and I'm going to show you a couple of them. Michael Voegele showed you a series of them the last time we got together. And we did those evaluations and tried to conclude what was important.

Now just so you don't think it was all math, we also did some other pieces; and that as we called in the performance assessment analysts who are very familiar with their model and how their machine runs, and we asked them "What are we doing wrong? What are the uncertainties that we're not considering? What are the limitations of these preliminary analyses? What else should we be considering other than the math?" And we had a good dialogue with them to tell us that.

We also brought in the process modelers. Remember there's two steps to this: process modelers find truth, if you will, as best they can in the nature of the system; and an analyst create an abstraction so that they can calculate. So then we brought in the process modelers and said "What do you think about the system and how well the system is being represented here?"

So we took both of those groups, the analysts who play with it a lot at the back end, and the principal investigators with "Is this working the way that we think it is?" And we elicited them, so to speak, and said "How are we doing? What is your confidence and representation of what we have chosen, what we're concluding? And what's the information that we need to address the current issues and how can we do some simplifications?"

So this was just math. This was a very large group. It started with about 60, and we concluded with a smaller group towards the end, but we investigated and talked through all these issues, not just the math.

Principal factors--when we did that, when we gathered that group together and we asked people "How does this drop of water work?" And if you've never noticed, the goal here is the factors basically follows a drop of water from the cloud to the biosphere. We obviously run into a little trouble with a couple processes, but that seemed like a likely place to put them.

So for a transparency approach we tried to get this drop of water tracking through, and what happens to that drop of water? What happens to hold it up? What happens to form a barrier? So when we did this the first time and we met with everybody, we came up with about 55 of those--two many, overlapping, and we worked very hard. It actually took us two or three meetings to get it down to about this many, to condense and combine, because this is obviously a very complex problem.

Now what's important in this slide is that the key attributes, looking at water contacting the waste package, the waste package lifetime, radionuclide mobilization and release from the EBS, and transport away from the EBS, hasn't really changed from Rev. 1, repository safety strategy.

What we've been trying to accomplish for many years on this job, the strategy and the attributes of that strategy have remained pretty much the same. How we model the system based on our current understand changes. As I showed you in the first with the evolving technical basis, evaluate the case, make decisions, go back, test and keep going through.

So these are the ones that we came up with. As Michael Voegele showed you last time, we did a number of barrier analyses. We asked people, and we concluded that these seven factors contribute the bulk of performance in the performance assessment.

To say that a different way, if you took the climate and you extended the climate out to its most deleterious extreme, of its probably distribution function, if you took it out to its 95th percentile, it doesn't really drive the answer very much. So you could simplify it and take a very high rainfall and it won't make a lot of difference.

We ran a barrier analysis with net infiltration into the mountain, and that's one of those unreal analyses that we talked about yesterday, and that is the waste package is there but let's pretend it rains right in the drift. There is no deflection. It doesn't make a lot of difference to the overall result. Why doesn't it? Well, the drip shield and the waste package are very robust. And so that's part of the strategy. So it's important that we understand the performance of the waste package. Its uncertainties are important.

With the drip shield present, the way--the uncertainties of the waste package are not as important because now you have two materials. You have two functions that are happening. And so these seven items are where the bulk of the performance really happens, and if we understand their uncertainties and we understand their performance we can get a fairly high confidence, because the rest of these don't drive the answer nearly as much.

The example of principal factor on drip shield performance--it's always hard to decide which end to work from on these--what you see here is nominal pace again. This is preliminary analysis, deterministic, not probabilistic. Nominal case, 100,000 years, no release. Take the drip shield away and have the waste package sit in a drift at nominal conditions, and you see--you start seeing almost a micromillirem at the 30,000 year point. It says pretty robust package.

Go back and neutralize the waste package only and depend on the drip shield only, and what you see is that you start getting releases, because the waste is laying naked in the drift, if you w ill, and the drip shields start to fail. And so without the drip shield you don't get nearly as good a result. The two together, you get a very good result in the nominal case.

And finally, if you neutralize both the drip shield and the waste package you basically have removed the engineered barriers. This particular analysis--before you ask the question--does include clad. So your factor would give you about 50--a factor of 50 higher on all three if you neutralize the clad as well.

But it gives you--the picture that you're trying to show is that these two together really provide a defense in depth mechanism and reduce the necessary understanding of the uncertainties on each, because they work with each other.

Now under expected conditions the waste package lasts more than 100,000 years. If you want to believe the numbers, it's time to go home. However, we need to look and say "What about the waste package?" If we rely on a waste package completely, then we have to understand it completely. With the drip shield we have defense in depth. It's not as important to understand those uncertainties as completely.

The drip shield design, by the way, appears to be feasible; a number of corrosion resistant materials, it appears to be testable in a scalable condition, and we probably can do some prototype testing to show and continue the corrosion mechanism type testing. So it looks feasible.

Seepage into the drifts, if you have this waste package failure, if you have this drip shield failure, and now you're getting moisture in, it becomes really important to look at how much seepage is there. What are the solubility limits? How much can you push into the water? And then what dilution do you get as moves through the saturated zone, the unsaturated zone?

We're looking at this with the engineered system failed, and now we're going to be dependent on what's happening on the transport mechanism. And so we're looking into t hose because they're especially important in the event of the engineered barrier failure. So we're not placing all of our eggs into the engineered basket. We're looking at the combination of natural features that also can provide protection.

Again, under expected conditions, lasts 100,000, it isn't particularly dependent on seepage to last that 100,000, as I said earlier. But once again if the engineered system doesn't work as expected, what if, I believe Mr. North put it yesterday, you look at your what-ifs and when you do your what-ifs you start looking, and this drives us to these particular factors.

Now what happens in the revision, revision 3 of the RSS? The performance assessment, we'll put in what we have to have, which is expected performance for the nominal case, igneous activity, human intrusion, TSPA sensitivity and importance analysis of some sort--we'll do lots of analyses; we are not wedded to any particular type of sensitivity or study; we're going to look to gain the knowledge; and we'll go back and look at the principal factors for the SR.

Right now we have done a preliminary analysis with the LADS design, we have looked at what we think is most important; we are focusing there. We need to go back and verify that in fact we are right and that we are focusing on the right aspects, because the evaluation of the updated models will give us more information.

We'll look at the safety margin in the defense in depth. We believe we'll have substantial margin. We will have considered additional design enhancements. We may look at more changes to the thermal management strategy. We're looking at backfill strategy. I believe Dr. Dyer said that--told us to move forward without backfill, keep the ability to do backfill but move forward without backfill.

We'll look at the drip shield design. It may change its size and shape and material or thickness. And the drip shield concept--maybe there's another drip shield that we should be using; maybe a Richard's Barrier instead of the metals; maybe ceramics. We'll consider those types of things; no commitments, but we'll consider, look at how do we make the system more robust.

And we will have looked at the benefits of the seepage threshold and some aspects of the saturated zone retardation. We will have looked at the potentially disruptive processes and events, and it'll do as I said before the unanticipated early failure of the EBS, igneous activity, human intrusion.

We'll be looking at some other features, events and processes that may in fact be screened out but deserve review: water table rise has been discussed many times; seismic activity; waste generated changes from the evolution of the waste, including criticality; or the drift collapse.

Natural analogs, again we're going to take the existing information and see what else will help us as we close o n what we think the argument is we need to sustain, then we'll look at what the additional work is that's needed. And we'll be looking at the performance confirmation plan, looking particularly at the principal factors, because once again that's where the real performance and the real gains lie. How do we show those principal factors behave as we believe.

So the evolution in the event the site is recommended, modification of the RSS would be considered. How do we want to go forward with it. The update would consider the results of the TSPA-SR, and perhaps we'd make more simplifications for ease of the licensing process. So again you go through the design selection, you make decisions, the SR decision--we'll go through it again. We'll look at the RSS, make sure that we've done it right and whether there are some changes we should make in order to move forward to the license application, if that is so said.

Concludes my remarks.

CRAIG: Okay, thank you--

BAILEY: I beat my time, sir. No hook today.

CRAIG: Wonderful, wonderful.

BAILEY: No hook today.

CRAIG: Questions from the Board?

BULLEN: Bullen, Board. Jack, first a compliment on slide number 6. I want to thank you for actually answering questions that we've asked before about the removal of the barriers. I think that's a very good presentation.

I do have a couple of questions about the follow on from that--if you go to slide number 10--and you talk about the neutralization of barriers--

BAILEY: Yes.

BULLEN: --like the neutralization of the waste package only. The implication here is that the drip shields, are they leaking at 3,000 years? Or how do you get a release from a neutralized waste package if the drip shield's still intact, is the question.

BAILEY: The drip shield would have to corrode under the nominal conditions at that point in time. In other words the waste package has been neutralized and the waste is laying bare in the drift.

BULLEN: Okay.

BAILEY: Okay, and if the waste package alone has been neutralized, the drip shield is above it, and what you had to have had is a failure of a drip shield to allow the seepage to come through and contact the waste.

BULLEN: Okay. Thank you.

BAILEY: Did I get that one wrong, Abe?

VAN LUIK: This is Abe van Luik. You didn't get it really wrong, but what happens is if there's a waste package failure and the drip shield's still intact, there's a very slow movement of radioactivity by diffusion.

BAILEY: Diffusion, okay.

VAN LUIK: Into the rock, and once it hits the rock then it gets into the advective flow, and so what you see is about--you know, a few thousand years of travel time through the invert, et cetera, from a prefailed waste package. All of these presume a prefailed waste package.

BULLEN: Bullen, Board. So the waste is just laying on the bottom of the drift.

VAN LUIK: Oh, yeah--

BULLEN: And it has to diffuse for 3,000 years, and then it's an advective flow. So it's not--so the drip shield hasn't failed.

VAN LUIK: No--

BULLEN: You've basically got flow underneath it.

BAILEY: The drip shield fails about 8,000 years in the nominal case--

VAN LUIK: Yes.

BAILEY: Okay, I--

BULLEN: Thank you.

BAILEY: --stand corrected.

CRAIG: Don Runnells, followed by Jerry Cohon.

RUNNELLS: Runnells, Board. Could we go to your slide number 6 please?

BAILEY: Sure.

RUNNELLS: These are the mean values of the parameters. Can you give us--and I know this is a hard question, so just the best guess is okay--how wide would the confidence intervals be on some of those lines? Let's say in addition to the mean values you wanted to put a band of air, let's say about the top line, no barriers--solubility limited release. How broad would the 95 percent confidence interval be on band of air about that mean line? Do you have any idea.

BAILEY: I'll have to turn to Abe for the specifics, I think.

RUNNELLS: I think that's one of the things that troubles people, is we see the lines and we don't know how much confidence we should have in a line or how broad the band should be. I guess I should really say how broad should the band be?

BAILEY: Right, and I think--before--I think Abe will help me--I think one of the things is that we were trying to get an understanding of how the system works here, and that's why I very lengthily said we did preliminary non-deterministic evaluations to get a view of how this would work and--in the average conditions. I don't know that we've actually done the calcs in that particular case, and Abe's more familiar with the TSPA than I am, so we'll let him conjecture.

VAN LUIK: Abe Van Luik. We haven't done those probabilistic calculations yet, but if the VA is any indication, you will be a few orders of magnitude above and below that mean value, to get between the 5th and the 95th percentile.

But as I indicated yesterday, for the very long times, that is the quantifiable uncertainty, and there are other uncertainties. So this, you know, kind of reverts back to yesterday. We need a fuller discussion of uncertainty rather than the calculational band of those things that we know are uncertain.

RUNNELLS: Thank you.

CRAIG: Jerry.

COHON: Cohon, Board. I have a question about principal factors, and this diagram motivates it. The natural barriers are shown to give a several orders of magnitude decrease in dose, but among the principal factors are seepage--well, let me just pose it direct.

Looking at the principal factors, is it fair to conclude from this slide and what you didn't include as principal factors, that the primary actors in the natural system are the ability of the radioactive material to dissolve, the solubility?

BAILEY: That's correct.

COHON: And also the saturated zone retardation?

BAILEY: Yes, and those are basically properties of the material of the mountain, which we know very well.

COHON: And as you said, it can rain directly on the packages and you would still come to a similar conclusion?

BAILEY: Yes.

COHON: Another question on principal factors, and this goes to the linkages among the factors, which is unavoidable and I understand it.

BAILEY: Yes.

COHON: The principal factors can't be perfect because the hip bone is connected to the knee bone? Somewhere.

BAILEY: No on me.

COHON: Yeah, you got there.

BAILEY: I'm a little taller than that.

COHON: Well you're a systems engineer, so you know that stuff. The performance of waste package barriers is a principal factor, but the coupled processes are not principal factors. Yet I would assume that a key driver of performance of waste package barriers is the environment, the near field environment, which of course is linked to the coupled processes.

Now I've made an assumption. Is that correct?

BAILEY: Yes, it is.

COHON: Okay. So when you identify a principal factor though, like performance of waste package barriers, but you don't identify say coupled processes, still you're picking them up because of their linkage to the principal factor?

BAILEY: Yes.

COHON: Okay.

BAILEY: Now the reason I say yes, remember what I said earlier on that slide--if you go to the principal factors slide--

COHON: It's 9.

BAILEY: Next slide.

COHON: No, number 9.

BAILEY: Number 9 please. Remember what I said, and that is if you drive those other factors very high in their uncertainty range, it doesn't make a lot of difference.

So even though the environment on that waste package is very important, if we can show that the bounding, if you will, environment on that waste package does not deleteriously affect or create real problems for us and the waste package barriers, then our effort is to show that we can bound that environment and those coupled processes and drive it, rather than try and understand the purity of everything that happens there.

COHON: Well--

BAILEY: It is connected, but--

COHON: Yeah. Okay--

BAILEY: --and it's easier to show the simplified approach that it is to understand everything about it and show that it's so.

COHON: So--you just said something important, and I want to make sure I understand it. Is it fair to conclude that if a factor is not a principal factor that you've driven it to its extreme value and it still has not shown itself to be important?

BAILEY: Yes. That was one of the bases that we looked at this on, was if we--one of things that we did is we varied these and said if you move it from--you know, have a PDF and if you move it from here to there, from end to end, what does it--individually perhaps--do to the overall response. And we found very little response in the bulk of this. These made a big difference.

COHON: Do you have a concise summary of all the extreme values that you tested for each of these factors?

BAILEY: I doubt it. It isn't in the RSS. We could provide it.

COHON: That would be very interesting.

BAILEY: Abe?

VAN LUIK: If I can--Abe Van Luik--keep in mind that this particular product was created with the stylized calculations to give us insights. But it was really driven by the expert elicitation of the PA people and the scientists in the project.

We are beginning the cycle over again. It is as an iterative thing. In a couple of months we will have the first of these workshops to start, you know, have we learned anything that is going to drive us. And the informal feedback we're getting already is "Oh, yeah, the near field environment may be more important than we thought."

I think we were mesmerized last year and perhaps rightly so, except now the uncertainties are beginning to creep in, that we had selected materials that were immune to anything you could think of in the coupled process area; and now the change that we expect, and we will have to go through the process and see, is that we may have thought more about it and said well it may be more important than we thought this last time. So you're seeing a living product here, and I think your input is very welcome.

BAILEY: Let me make sure I'm not misleading you, and that is, is that we did do a lot of these--as I said before--in large rooms, "What do you think? You're the guy. What do you know?" We captured a lot of it like that without necessarily explicit calculations. I think we can capture what we asked and what the answers were.

And if you recall on the last slide--Lisa--oh, well--we have to go back and look at it again. We have to go back and make sure we made the right decisions and that the choices we made were in fact correct. And we know that. That's one of the things we have to do in order to move forward with site recommendation.

COHON: I in fact have one last question on this slide.

BAILEY: Okay.

COHON: There's no arrow coming out of LA, and I wonder if there will be?

BAILEY: Oh, yes. There was no--if you go back--

COHON: Yes, I--information--

BAILEY: --no arrow coming out of SR before--

COHON: I remember that.

BAILEY: If we put up the other screen you'll find that you have to keep doing this--

COHON: Okay.

BAILEY: --it's a part of the communication process, it's part of the have we gotten it right process, part of the we learned something new--does it affect our results. We talked yesterday about does science stop. No. We have to keep looking and knowing and we have to keep moving. And I just moved one more step from--used to have the VA there; we've now moved on--

COHON: Thank you.

BAILEY: If we all live long enough we'll have 10 or 20 of them.

COHON: Okay. Thank you.

CRAIG: Our sequence now goes to Alberto Sagüés, followed by Priscilla Nelson, followed by Daniele. Alberto Sagüés.

SAGÜÉS: Thank you. Could we look again at the number 10 please?

BAILEY: Number 10 please.

SAGÜÉS: Great. Do I understand correctly that the cladding credit is being taken for those estimates?

BAILEY: Yes, cladding credit was included in the calculations because that's the model that we had available.

SAGÜÉS: Right, and if you wouldn't have cladding, that would have increased those currents dramatically, or not?

BAILEY: At most a factor of about 50.

SAGÜÉS: About a factor of 50.

BAILEY: Yes.

SAGÜÉS: All right, then is it fair to say that without the metallic barriers, that is the drip shield, the waste package, and the cladding, the repository just plain wouldn't work? Is that correct?

BAILEY: I because there's a slide--

SAGÜÉS: --fair--fair way to say--

BAILEY: I wouldn't say it that way. If you'll go back I think two or three more slides to the--keep going--here--this slide answers the question of what is the performance of each of the pieces, given unreal conditions. There in fact is clad, there in fact will be barriers; there will be some credit given to those. But this gives you, without a probabilistic evaluation, mean values, tells you that this is what's out there in unreal situations, situations that don't occur.

SAGÜÉS: Right, but that protects at 8000 years, 100 rem.

BAILEY: I think that's what the number comes to.

SAGÜÉS: Yeah, so I mean that certainly wouldn't be appropriate performance.

BAILEY: That's correct.

SAGÜÉS: Okay, so--

BAILEY: --four--grinding up the waste and throwing it in the bottom of the drift, which is not--

SAGÜÉS: Right.

BAILEY: --the approach.

SAGÜÉS: Right, so what I'm saying is that the present concept relies I would say completely on the adequate performance of the metallic barriers. Without those we would have release rates that would be just totally unacceptable.

BAILEY: Let me make a couple of comments. If you'll jump to the next slide, please Lisa, I think a couple of things: one, the system--and it is a system that's intended to how do we make the whole system perform; the second is--and I mentioned it earlier--we leave a lot on the table.

And that is, is that because we have some of the metals and because we have the ability to analyze the metals and have a great homogeneity in the metals, we don't go after some of the conservatisms that are probably available to us. For example, secondary mineralization. That has the potential of holding up a great deal of the radionuclides inside the matrix as the matrix corrodes, if you will.

So we have a number of areas where we have not pursued additional realistic approaches in the natural system, partially because of heterogeneity, partially because of the difficulty in licensing. Secondary mineralization, for example, in a licensing sense, is a very difficult piece. You've got to look at the inside of a canister with 21 or 54 elements, it's got a whole series of materials; becomes very difficult to prove or gain reasonable assurance that you know exactly what's going to happen.   In fact, is it there? Yes.

And so yeah, if all you're going to do is grind it up and throw it in there, yeah, you have a fairly sizable dose. On the other hand, if you work with a system and you take advantage of each of those systems and look at the fact that you have conservatisms that you've built into the system, then I don't think that you can judge the site on that chart. That's a very simplified chart.

SAGÜÉS: Okay, but you would agree that the metal barriers are a substantial and all important--

BAILEY: I think--

SAGÜÉS: --element--

BAILEY: I think that we have analyzed--

SAGÜÉS: --projected performance--

BAILEY: I'll try again. I think that we have analyzed and found and depended and made decisions that we are depending on the metal barriers to a great extent. We could in fact depend more on some of the natural systems that we are not currently trying to model in a more realistic manner.

SAGÜÉS: Um-hum, okay--

BAILEY: So there's a tradeoff here.

SAGÜÉS: All right. All right, I'm saying this because of the following: the projections of the performance of the natural barriers can be sort of backed up by a number of geologic analogs, and extensive, very long term experience in dealing with geological assistance.

BAILEY: Yes.

SAGÜÉS: So the likelihood that some of the things which are projected will have a dramatic turn of events, unexpected in the next several thousand years is there, but at least it can be assisting in terms of prior experience with analog systems.

BAILEY: Yes.

SAGÜÉS: Now the problem that I have always encountered with this is that when you look at the metals, we are dealing with new materials, materials that have a very short lens of engineering experience. And we are basically betting the performance of the system on the long term performance of these effectively new materials.

And shouldn't there be in these realizations or in these calculations some evaluation of what is the likelihood of this--that these materials will not perform as expected? Shouldn't that be something that should be also quantitatively introduced in some fashion, because right now it isn't introduced in that way, right? We are just looking at for example the slow rate of dissolution expected for these materials, and we are using linear extrapolation.

But there isn't at this moment any input for what will happen if something happens with the corrosion rates say in the year 3000, and they're accelerated by one or two orders of magnitude? Is that correct, there isn't such a provision?

BAILEY: Well the calculations that you see here came from the viability assessment. They are preliminary. We put some quick calculations for the alloy 22. These calculations for example didn't consider stress corrosion cracking, and stress corrosion cracking is one of those failure mechanisms that could happen--forget about the corrosion rate, just the stress corrosion cracking.

And we recognize that as a failure--and we needed to find a way, if you will, to engineer it out or lessen its dependence or put a model in that takes into account that that occurs. And so we are in fact trying to look at the fragility, if you will, the frailness of the engineered barriers.

We are in fact doing the barrier analysis neutralization. We are looking at materials that have different failure mechanisms so that we don't have the drip shield jump by an order of magnitude, as you suggested, which I don't know--

CRAIG: Jack, I need--

BAILEY: --likely.

CRAIG: --we need to break in because we're running out of time--

BAILEY: Okay.

CRAIG: --and we have two other Board--two other questions, which need to be fast.

NELSON: Nelson, Board. This isn't so much a question as a please correct me if I'm wrong. Every time I've seen the principal factors plot, and the identification of the seven selected ones--and in particular in the context of the comment you made regarding climate--I raise an issue which doesn't make--I think really stops a lot of people from understanding the conclusion you want them to draw.

I think most people would think climate was a very important thing and that without an increase in rainfall of some significance you're not going to get the change in seepage that's going to change the processes that happen in the near field environment.

And while the kinds--the order of magnitude that the scale of change that occurs in the seepage into emplacement drift factor probably is the one that's really directly pertinent to the calculations that are involved in the PA. They clearly are very importantly driven by the climate. And so I just caution that statements about climate not being important really deter comprehension and understanding of the model.

CRAIG: So we will take that as a--

COHON: The hip bone is connected to the knee bone.

CRAIG: --comment, and move to Professor Veneziano.

VENEZIANO: Daniele Veneziano. I want to make a remark regarding the assessment of uncertainties, and I hope I'm quoting you correctly when you say that you are using models that range from realistic to conservative, I believe you say, so that you can be confident on the mean value.

Now it seems to me that when you assess uncertainty you should not do so either conservatively or unconservatively. You should do it in an as unbiased way as you possibly can, and then articulate the reasons for conservatism, and introduce the conservatism at the stage of decision making rather that at the stage of assessing uncertainties, probabilities and mean values, or else that has the possibility of muddying the waters in a way, not making sort of that decision about the degree of conservatism in explicit and -- one as I believe it should be.

So unless I have misunderstood, I want to just make that comment regarding what I believe is the imperfect way to assess uncertainties, and then make decisions in an appropriately conservative way.

BAILEY: I would agree with what you said. We in fact ran into that problem in viability assessment where our assumptions on clad masked some of our results and masked some of our sensitivities. And we're trying to stay away from that here in fact by going back and doing barrier analysis and extending sensitivities and taking a look. But if we have conservative results, we have to have some gain in confidence by the fact that we have some that we've modeled conservatively.

CRAIG: Okay, thank you very much.

VENEZIANO: Oh, just a very quick comment. I agree in being conservative when you make sensitivity analysis, but not when you assess uncertainty. Probably that's what you do anyway.

CRAIG: Thank you, Jack.

BAILEY: You bet.

CRAIG: We now turn to Bo Bodvarsson.

BULLEN: Bullen, Board, and I'm sorry for coming back in late date, Jack, but you made a comment to Dr. Sagüés that basically you--this was based on the viability assessment for most of the analyses you did?

BAILEY: There was a viability assessment and we modified certain of the calculations--

BULLEN: Modification--

BAILEY: --accommodate--

BULLEN: --okay, modification included the incorporation of coupled processes?

BAILEY: I don't believe that--no.

BULLEN: Okay, so then I run into a real problem here because you're reducing the uncertainty--or with a new design--but if you don't have the coupled processes included, I guess the question would a cooler design reduce your uncertainties even more?

BAILEY: Well we moved in fact to a cooler design in order to deal with those uncertainties associated with heating the whole block and where does the water go, and those kinds of problems. Now does taking it all the way down to no boiling reduce it beyond a point that we need to be? I think that's something that we have to look at, and I think Abe will comment on it.

BULLEN: So as I look at--before you come in, Abe--this is Bullen, Board, again--so as I look at your principal factors listed and you say coupled processes effects on the unsaturated zone flow, you're talking mountain scale unsaturated zone flow, not drift scale unsaturated zone flow?

BAILEY: I think you have to talk both.

BULLEN: But you don't model--

COHON: --not capable of talking both.

BAILEY: Abe, did you want to jump in?

VAN LUIK: For one second. Abe Van Luik. I think an important point in looking at the RSS is these calculations gave us some insights, but in the discussions and the expert elicitation part of it, informal expert elicitation, all these issues were brought up; and that's why some things were broadened.

And that's why we expect that now that we have this under our belts and we have critiqued it ourselves, the next time around you will probably see a slightly different variation on a theme. But, you know, I think we are too focused on these analyses. They--we discussed their limitations ad nauseam at our meetings.

COHON: But why are you still using VA based calculations? You're starting to write the SR right now.

VAN LUIK: This is Van Luik again. We are not still using them. We used them to create this product and this work was done almost a year ago.

BAILEY: Yeah, last July.

CRAIG: I'm going to jump in here and stop this. This is a wonderful--a wonderful and exceedingly important subject, which I expect will get discussed a lot over coffee break and elsewhere.

I'm particularly pleased to introduce Bo Bodvarsson because Bo has taken on the task with his group of helping to get me educated on how the Vadose Zone works. There's a little idea that I want to write a little dummy's guide to the Vadose Zone, which is exceedingly important, and I simply don't understand it very well. But Bo and his team understand it very well, and together will get me where I want to be. So I'm very, very happy with his little project and your support for it.

In any event, Bo Bodvarsson is the Lawrence Berkeley National Laboratory lead for the Yucca Mountain Project and nuclear waste program leader for the Earth Sciences Division at LBNL. His research specializes on geothermal reservoir engineering and nuclear waste disposal. Today he'll discuss seepage, which is one of the principal factors identified in the previous presentation.

You're scheduled for 25 minutes. I'll give you warning a little before the 25 minute time period has ended.

BODVARSSON: Thank you, Paul. Can everybody hear me okay? Is that better? Okay.

My name is Bo Bodvarsson, Lawrence Berkeley Lab. I'm here to talk about seepage studies a little bit, and the main thing I want to talk about--this number 1--why is seepage a principal factor; number 2, what experiments and tests have been done to evaluate seepage; number 3, what modeling have we done to analyze the data we obtained for seepage; and number 4, where are we heading, what additional data do we plan to take for SR and for LA.

So seepage, as all of you know, determines the amount of water that enters emplacement drifts. So we must do seepage calculation in order to know how much of the water is diverted around the drifts and how much seeps into the drifts. Now under expected conditions with a very robust waste package that lasts 100,000 years, seepage is not really very important if all of the packages would last 100,000 years.

However, there may be some unanticipated early failures and if that's the case the amount of water that enters the drifts becomes very, very important because it dissolves the waste and it carries the waste out of the drifts into the unsaturated zone and down to the saturated zone. So seepage becomes very important. Current information doesn't really preclude significant releases for early failure of waste packages. Next one please.

Now I'm going to start by talking a little bit about the drift seepage peer review just completed a few months ago. The peer review team did a very good job in looking at all aspects of seepage, including the testing program and the modeling program; and there's nothing really we disagree with what they concluded.

They concluded that there are currently large uncertainties in seepage estimates, and that's simply because we just started testing for seepage a couple of years ago; and we all realize that this is the case. More site data, modeling, experimental work are needed and the Department realizes that. There are plans to collect more information as I'll tell you a little bit later.

But what we have seen so far is that the drifts act as a very effective capillary barrier that prevents seepage to occur. Water does not want to go into big openings because it wants to stay where capillary suction keeps in place; so water really wants to go around the drifts. We have seen it both from the data and from models that I'll show you a little bit later.

The TSPA-VA uncertainty analyses concluded that seepage fraction is extremely important for peak dosage, and that for both 100,000, 10,000 and a million years it's a very important factor. Next slide please.

Now one of the issues that the Board brought up was what about tunnel stability, what happens when rock fall occurs and we don't have this perfectly shaped drift anymore? Our current studies are addressing that. The EPA, the engineered barrier systems people developed analysis of likely rock fall, likely changes in the shape of the tunnels.

We used this information directly with our calibrated seepage model and evaluated based on their results what they had concluded most likely was not significant for seepage; that the rock fall will not be so much that it would significantly affect seepage. However, if there are massive changes in the drift which are not anticipated, of course seepage would increase.

The project is planning to couple those mechanical estimates of drift shapes as a function of time of course with our seepage calculations. Next slide please.

Now let's look a little bit at the testing program. Is this focused? Doesn't look real good. Look all right to you guys? Okay.

SPEAKER: Looks like New Jersey.

BODVARSSON: Huh?

SPEAKER: Looks like New Jersey?

BODVARSSON: New Jersey? Yeah. Looks kind of like--I see. The testing program on seepage has occurred in two areas basically. One is the niches, and we have done testing in niche 3 and niche 2 and niche 4 which are located here in the ESF. All of those tests have been in the middle non- lithophysal, which has not been named repository rock. Keep that in mind.

We are also doing tests in alcove 1 where we put water on the surface and we observed the seepage into that alcove. And I'll show you a little bit about that. Next slide please.

What have we collected so far? We have collected seepage data from controlled liquid release tests in three niches in the middle non-lithophysal unit. And I'll show you those tests. We have done air permeability tests on those niches. We've also done air permeability tests in the lower lithophysal tuffs, which is very, very important, because these are the first tests that could indicate potential seepage in the lower lithophysal rocks, which are the main repository rocks. And I'll show you those a little bit later.

We have done the alcove 1 large scale tracer test and we are continuing that, USGS and Alan Flint's team is continuing this work; and then we have also observed construction water monitoring below the cross drift. When the cross drift went over the ESF, lot of construction water was lost. How much did seep, and I'll talk a little bit about that. Next slide please.

First the wall: drift seepage test. What do we do and why do we do it this way? Basically what we do, we put water directly above the niches, very close to the niches, so these are very conservative tests. Only two feet to three feet above the niches we put water in pack intervals, and we try to force it to go into the drifts.

And then we measure and collect the water here and we measure the fraction that goes into the drift versus the fraction that is either in storage or goes around the drift.

That is percentage seepage as a function of percolation flux. This is what TSPA needs for their evaluation of seepage.

We have done a bunch of these test in the middle non-lithophysal. All of these tests are analyzed with models. Next slide please.

The tests in the middle non-lithophysal are analyzed with seepage model and calibrated against all the data. The model, if we observe 15 percent seepage, the model has to agree with it; it has to show 15 percent seepage. The models we generate have a lot of fracture patterns in them. They're measured in the tunnels, the preferred orientation; they are then calibrated to the air permeability tests in the bore holes; and then we apply liquid water, just like the test was done, and we calibrate it to the seepage.

Based on that then, on the calibrated model, we do Monte Carlo simulations to determine what we call the seepage threshold. And it turns out--this is not really the right slide--it turns out the seepage threshold is about 200 millimeters per year for a middle non-lithophysal unit. Next slide please.

Alcove 1 is a very important test for us also. Why is that? It's because it's at the different scale. It's now we don't force water a few feet above the niches and force it to seep. We are working with about 15, 20 meters down to the alcove; we have an infiltration pack here, and we have a collection system in place here in Alcove 1.

There have been two tests done so far. One was completed last year, and the other one is continuing now. What is important about these tests? Number 1 is we apply lots, lots of water, and even if we apply lots, lots of water only 10, 20 percent of the water seeps. Not very much. Much higher than percolation flux you would ever see, including climate changes.

The other thing extremely important too is the issue about matrix diffusion which we rely on in the unsaturated zone for transport. When the radionuclides leave the drift and they flow in fractures from the repository to the water table, there is interaction between the fractures and the matrix blocks. One of these interactions is due to diffusion because there are concentration differences in radionuclides in the fracture in the matrix block.

Diffusion is extremely important for performance. What this test showed us, that with applying this infiltration about 50 percent of the fractures encountered from the surface to the alcove were flowing at this time, and matrix diffusion was very efficient in retarding the movement of the tracers we used. Next slide please.

Now let's go on the ECRB. What are we planning to do in ECRB and what have we done so far? And as all of you know, the east-west cross drift is here, it goes through the repository block, so this is a very, very important piece of real estate that we must test very thoroughly to gain confidence in seepage as well as other results. And of course this is very important because here is the chance for us to measure seepage and other parameters in the main repository rocks, the lower lithophysal. Next slide please.

What are we doing and what are we planning to do? First of all the project has sealed off part of the east-west cross drift, which was done in June 1999, just simply to observe will any seeps develop. This has been ongoing since June 1999. Secondly we started niche studies. Niche 5 has been--studies have been started on niche 5 to evaluate seepage threshold in the lower lithophysal rocks.

We have completed already a set of air

permeability tests, which I will show you, and we are planning to do the seepage in March this year and May this year to feed our seepage calibration model and then TSPA for Rev. 1. This will feed the AMRs and the PMRs for Rev. 1. Next slide please.

NELSON: Bo, can you tell us where nice 5 is?

BODVARSSON: Absolutely. Can you go back two slides? Niche 5 is located about around here. It's just you go into lower lithophysal and just few hundred meters or so, that's where we selected niche 5. We selected it in a very heavy lithophysal area, very broken rock. So the test for seepage should be fairly conservative, because when you look at that rock it is heavily broken and fractured, with big lithophysal cavities. Next slide please.

We are also--the project also decided to do something very important for uncertainty, and that is a systematic evaluation of A, hydrological properties such as air permeabilities and tracer tests, and B, seepage tests. This systematic hydrological characterization will go along the cross drift and there will be bore holes drilled above the ceiling, and we will do air permeability tests and seepage tests in a bunch of bore holes along the cross drift. This should give us a very good handle about the variability of seepage with space, because the niches are only located in a very, very few locations, of course.

Also a very important test is the cross drift tracer test, and that is a test between the ESF and the cross drift where we apply water in the cross drift and we try to observe it in niche 3 in the ESF. So that's a very important test, because again that's a scale of 10 submeters again, not like the niches, a scale of meters.

So I'm going to show you a little bit about these tests, and you can ask any questions you like. Next one please. Here is niche 5, cross drift niche. This is how it's designed; there's a bunch of bore holes coming out here. One part of this--purpose of this is to look at actually excavation effect, look at changes in permeability away from the niche; but the main purpose of this is of course to measure seepage.

It's located in the lower lithophysal zone and pre-excavation interjection tests suggest that this rock has higher permeability in the middle non-lithophysal. This is a very important conclusion, as I'll show you a little bit later. It was excavated -- seepage tests are planned in the year 2000. Next slide please.

These are very new results. This comes from two bore holes in niche 5. This is the first air permeability test in the east-west cross drift from the lower lithophysal rocks. Remember this comes from one location, two bore holes; so it's very limited data. But what it shows is very important.

It shows that the two bore holes have similar permeabilities, average permeabilities is about three darcies here--three times 10 to the minus 12, one darcy is one times 10 to the minus 12--but what is most important is that this is about an order of magnitude higher than all of the niches we measured in the middle non-lithophysal.

Now what does this mean? In general seepage decreases with increasing permeabilities. This may sound counterintuitive, but the reason is simply the higher the permeability of the fractured rocks around the niche, the easier it is for the water to go around the nice. So that's good news. So this is very important information and hopefully the seepage data that we will get in March and May will verify that the seepage characteristics are better than those we have estimated in the middle non-lithophysal.

However, there's one thing we always must keep in mind, and that is the lower lithophysal rocks have something very different from the middle non-lithophysal, and that is the large cavities, the large holes--up to one feet in diameter or so--and how they affect seepage and other characteristics of this rock. We don't know at this time. Next slide please.

This is the crossover drift test where we go from alcove 8, which is shown here, down to niche 3 here in the ESF. We are planning--the Survey is the main participant doing this work. They are planning to put water in here and see how much seeps into the niche down here. Again, very important, because of the scale effects, tens of meters now instead of meters.

So what is most important here is this bullet here, and that is during the construction of the east-wets cross drift, even though lots of water was lost, no seepage was observed in the ESF. Very important. Next slide please.

Also just--go back to the last slide--just to remind you, another very important part of this test again is matrix diffusion, to verify what we have learned in alcove 1 and Tiva Canyon, carry it down to the Topopah Springs unit, and verify that matrix diffusion is again important in that unit. Next slide please.

So I've told you all the data we have; I've told you about the modeling studies that support the data; I told you about what we plan to do, and now I'm going to reiterate it and tell you what we get out of all of these planned tests--and we are almost done.

First of all the lower lithophysal seepage testing in niche 5, this is the goal for site recommendation, are essential to give us some information about seepage in the lower lithophysal rocks, which is the main repository rock unit, of course.

The studies in niche 5 also give us the effects of excavation or hydrological properties. How far from the niche does the permeability increase? And as you recall from our studies in the middle non-lithophysal, permeability increased by almost a couple of orders of magnitude close to the niches due to excavation effects. This is very important.

The systematic testing in the east-west cross drift will give us the variability in seepage, in air permeabilities, in fracture porosities, along the east west cross drift. Very important for uncertainty analysis to understand the heterogeneity of the rocks.

The data on flow and seepage testing between the cross drift and the ESF niche will allow us to quantify seepage on a larger scale, and allow us to calibrate our models not only on a meter scale, but up to 10 meter scales, to gain more confidence, of course, in predicting seepage into emplacement drifts.

The results of flow and seepage testing from alcove 1 we will continue to analyze, and all of these data will go into one single calibrated seepage model that should apply on multiple scales. Next slide please.

This is the last slide. What are planning to do for license application? First of all let's go back to the comments of the peer reviews, some overseeing groups, including yourselves, that has all been taken into account in what we hope to accomplish for license application.

The most important part is this one here, and the seepage peer review as well as some of you have mentioned the need for this, and that is a longer term larger scale seepage test. That does several things for us. Number 1, it will allow us to tell where the water actually goes. When we do this niche test we say 15 percent seeps, but we don't know where the rest of it goes. We have to verify that the rest of the water actually flows around the drift like the models predict it will. So we have to do long term tests to do that.

We also have to evaluate the effects of evaporation close to the drift surface. That affects seepage tests. And this test is aimed to do that. Also what we hope to do, given the systematic variability and seepage study that we are doing in the east-west cross drift, is to do very systematic sensitivity studies to evaluate uncertainty of the seepage estimate, given they heterogeneity of the rocks.

We also--the project has planned a thermal seepage test in the cross drift that is going to be planned later this year, I think, and started to be carried out perhaps next year.

Finally there--we may start to look at percolation determination below the crest where the infiltration models have shown that there's highest infiltration and also close to the Solitario Canyon. And that concludes my talk.

Was I on time, Paul?

CRAIG: Thank you, Bo, you're ahead of schedule. Wonderful. Wonderful. That gives us time for discussion. Priscilla, Richard, Dan.

NELSON: Nelson, Board. Bo, thanks for the new information; appreciate it. I'd like to ask you a question about your comments regarding for example construction water.

BODVARSSON: Yeah.

NELSON: We had heard in the past that construction water has been lost to the formations, and some observations were made about different depths of penetration.

BODVARSSON: Yeah.

NELSON: I guess your comment about it being very important that there was no seepage, I was given to understand that the volume of water that was actually lost per distance, certainly over the ESF, would not have been such that people were actually expecting seepage.

So the question becomes, did--in your models for seepage in the non-lith and the lith units, would you have expected seepage?

BODVARSSON: That's a very good question. Actually the answer is we have not done the calculation with the amount of water that was actually lost during this episode to see if the models predicted it. But we should do that--that's a very good comment. Appreciate that. We should definitely do that.

PARIZEK: Parizek, Board. Bo, on slide 17 you talked about the long term seepage test for flow diversion.

BODVARSSON: Yeah.

PARIZEK: Where would that be done, or how would you--would it be done at sites where you already have instrumentation set up?

BODVARSSON: It definitely could be. There is not a plan in place yet exactly where it will be done. What I think is the most important part of that test is that we would have to do instrumentation and bore hole around it laterally to catch whatever water goes around, doing neutron probes, or doing whatever is going to allow us to quantify it. So that instead of just simply putting three bore holes above we would do a lot more counter instrumentation around the niche. But we haven't decided exactly, but I am sure--or at least in my mind--it should be in the lower lithophysal rocks.

PARIZEK: A follow up question then, the thermal seepage test, that's a new idea, I guess? I mean at least we haven't heard about that. Do you have a little more background as to what that test would include?

BODVARSSON: Well that test has been on the books for probably a year or two years, I would say. It hasn't been totally designed yet--at least that's my understanding. But it will be designed this year. They are trying to get some funding to design it this year. I don't know if funding has been approved for that yet. Do you know, Abe? Mark Peters, do you know?

PETERS: I'm sorry?

BODVARSSON: Why don't you ever listen to me, Mark?

PETERS: (inaudible)

BODVARSSON: No. I'm kidding you. The thermal test, I know we were trying to get it funded, the design of the thermal test in the cross drift?

PETERS: Yes.

BODVARSSON: Did that go through on one of the change requests?

PETERS: Yeah, Mark Peters, M&O. We have funding to start the planning--

BODVARSSON: This year?
PETERS: This year. And the current plan would be to field it next year.
BODVARSSON: See, I'm listening to you.
PARIZEK: One more question, Bo. This has to do with the large lithophysal cavities--
BODVARSSON: Yeah.
PARIZEK: --you're worried about, and you're not sure how they're going to interfere--
BODVARSSON: No.
PARIZEK: --with the flow patterns. But since they're cavities and they're smaller cavities than the--a drift--
BODVARSSON: Yes.
PARIZEK: --why would they not be barriers to water flow, just like you hope that the drift is?
BODVARSSON: Yeah, that's one possibility. But the other possibility is that when you start to introduce those kind of heterogeneities that water also wants to avoid, is the focusing effect.
PARIZEK: Okay, so here comes the analog question: do any of those lithophysal cavities contain young mineral matter--
BODVARSSON: Yes.
PARIZEK: --showing if fluids did get in there--

BODVARSSON: Yes.

PARIZEK: --sometime recently since it's been emerged above the water table?

BODVARSSON: I don't know if you can say recently. This gentleman, Zell Peterman, and Bryan Marshall in the audience there, they--

PARIZEK: The main thing is if you've got--

BODVARSSON: Status of studies--

PARIZEK: --new--new minerals in there, then it would suggest that water damn well did get into little small cavities and therefore it could probably get into large cavities for the same reason.

BODVARSSON: Right, well let me just summarize what I think their studies have shown. They find calcite in some of the lithophysal zones. We don't have sufficient information to say what percentage it is everywhere, but it's in some lithophysal zones--in small, and it's also some of the bigger ones. If they integrate the calcite deposition over the 11 million years or so where the mountain has been in place, the sea beds that goes into these cavities is extremely small.

Is that fair, Zell, Bryan? That's fair, okay.

PARIZEK: Unless it's episodic, it all happens in one day.

BODVARSSON: Right, unless--yeah, that's true. The only thing--well, just as a very good point, what we are trying to do--I think needs to be done--is to develop a three continuum model, because I think the lithophysal needs to be considered as a separate continuum from the matrix and from the fractures to full understand them.

BULLEN: Bullen, Board. Actually I've got a couple of questions. The first one is you mentioned the bulkhead test where you closed off the bulkhead and we understand that there's some observations that are made. Could you comment on those, about the recent observations of opening the bulkhead?

BODVARSSON: I didn't go in there--

BULLEN: Oh, so you're not the--

BODVARSSON: But what test was observed in there, there was a zone like 50 meter wide with salt water that everybody believes is condensate water, that is not seepage. No seepage was observed, no drips were observed anywhere in the tunnel.

BULLEN: Thank you--

BODVARSSON: We are doing chemical analysis on the water to make sure that it's condensate and it's not water that's seeping.

BULLEN: And you'll know that because it'll look like DI water?

BODVARSSON: Yeah.

BULLEN: It'll be very pure.

BODVARSSON: Right.

BULLEN: Okay, this is the hazard of putting extra slides in, so I was looking at your last slide, which is number 23?

BODVARSSON: Kidding me --

BULLEN: --which is the schedule--no, I've got to cheat and look ahead.

BODVARSSON: Right.

BULLEN: You talked about the incorporation of data into the SR--

BODVARSSON: Yeah.

BULLEN: --and you got three nice yellow circles that say this is the data feed--

BODVARSSON: Can you go to the last slide?

BULLEN: Yeah, go to slide 23 please. You've got three nice circles that say, looks like by April-May you're going to have all the data that you're going to have for SR. And I guess maybe the question for you is it looks like the niche 5 test is going to have some pretty good data between now and the end of the calendar year. Is there any possibility that you could incorporate that kind of--those kind of results into SR? Or is it--

BODVARSSON: Yes.

BULLEN: It's going--oh, it will be in there then?

BODVARSSON: Well let me say this, the way we are planning to do is the following: The TSPA uses seepage model for PA, which is based on the seepage calibration model. The data for niche 5 up until the end of July or August will be put in the calibration model, but then will feed the TSPA in due time. And information that comes in from August until the end of the fiscal year, if it provides much different results than what we have in the calibration model, will be directly fed into the TSPA obstructions in January, February.

BULLEN: Okay. Now this is actually a question from the audience.

BODVARSSON: Yeah.

BULLEN: Sorry about that. They want to know what pressure you were using for ventilation during the alcove tests, how much--how many--how much negative or positive pressure was there? Do you have an answer to that one? Anybody know?

BODVARSSON: No. I--sorry--does anybody here know? I'm sorry about that. I don't know.

BULLEN: Okay, and actually the follow on question to this is when you do your permeability tests, and if we heat the rock up to whatever the temperature's going to be in the near field, how big a significant--or how significant is the change--are the changes in the permeability expected to be during the heat up and then the resulting damage that would be produced form the cool down? Do you have--is that the--that's the goal for the thermal tests in the cross drift?

BODVARSSON: Well, you know, I think it's more the goal of the current thermal test, the drift scale test, which is ongoing now. In the drift scale test and in the single heater test, we have been doing systematic air permeability testing all throughout. We did it throughout the entire single heater test and we are doing it periodically for drift scale test.

The results so far show there are not major changes in permeability anywhere close to the drift; maybe a factor of two, up to five in some locations. And most of it recovers very well. You know, factor of two and a factor of five is nothing.

BULLEN: Okay, and you wouldn't expect there to be a big difference in the lithophysal zone and the non-lithophysal zone? Or does that not matter?

BODVARSSON: No, I would expect that if the permeability is an order of magnitude higher, the lower lithophysal, and again, remember this is one location--two bore holes--if that's the fact, the higher the permeability to me the less this impacts anything. Because the drain is potential for two darcies is tremendous, and if you go down to 100 milliliters you still drain all the water anyway.

BULLEN: Okay, I guess this is my ignorance on flow and fractured media, but if I heat up the lithophysal zone would I expect the permeability to go up or down?

BODVARSSON: That's a million dollar question.

BULLEN: Okay.

BODVARSSON: Because--

BULLEN: --this isn't a bad question then.

BODVARSSON: No, that's a very good question. Because when you heat it up, of course the rock expands, goes into the fractures and the permeability goes down. On the other hand when you heat it up you have shear movement also that opens up the fractures and increases permeability. So far the results, we think that the thermal mechanical effects on permeability are not very important.

CRAIG: Thank you. Okay, we now have four Board members with questions, and we're running out of time. So we'll go as far as we can get and then we'll call a break, and I'm not sure we'll get to everybody. But in any event, next is Alberto Sagüés.

SAGÜÉS: Dan, thank you. You answered about two or three of the questions I was going to ask, so. But really, you're looking at the transport properties; they are relatively freshly disturbed rock, right, by the drilling process and so on.

But what is the relevance of those measurements to the condition of the rock after say 5000 years after the drilling? Don't things happen to the surface of the cracks, or that maybe the lateral transport will be slower maybe--I don't know, half as much, or maybe two orders of magnitude less than now--and wouldn't that change the fracture to bulk ratio transport? In other words, how good are these very short term measurements to glean what is going to happen after several millennia?

BODVARSSON: That's another million dollar question. My feeling is that rock characteristics, properties, do not change over geologic time. They do not change much over thousands and thousands of years. However, what of course we are concerned about is the stability of the tunnels and the emplacement drifts, and that the shape of the drift is not going to be as nice as we thought, so that seepage would occur. And that of course is a big concern.

With respect to the permeabilities away from the drifts, I don't think we have a lot of concerns about that, except for the effect of heat --. Did I answer your question in any way?

SAGÜÉS: Well real quickly, I guess what I was saying is the interfaces. Of course there is a crack in the rock, right?

BODVARSSON: Yeah, right.

SAGÜÉS: And could it be that--and you're relying on some of the flow going through the bulk, supposed to go into a crack when you're looking at seepage--

BODVARSSON: No.

SAGÜÉS: --at least on the local scale. No?

BODVARSSON: No. No.

SAGÜÉS: Okay.

BODVARSSON: Our permeability models basically neglect anything going into the matrix. All of it is flowing in fractures around to this. So it's again conservative because it's all due to the fact these are under drifts.

CRAIG: Thank you. Okay, we're unfortunately running out of time. With apologies to other Board members, we are going to have to stop this session, and we now have a 15-minute break, and we're going to resume at 10:10. Thank you.

(Whereupon a brief break was taken.)

CRAIG: We are now beginning the second part of the morning session. And as you see from the Bill Gates special presentation machine up there, PowerPoint, Tom Doering.

Tom Doering has degrees in civil and nuclear engineering and currently manager of Waste Package design for the M&O contractor. Mr. Doering will discuss another of the principal factors, drip shield design.

DOERING: Drip shield design. Again I'm Tom Doering--

CRAIG: And I'll warn you when you have a few minutes left, but if you follow the wonderful precedent from this morning, you'll be finished early and have time for lots of questions.

DOERING: We'll try to make a balance there.

Going in now to the engineering side, we're going to review a little bit of the engineered barrier systems and the waste package, and also we will work into the drip shield, where our main talk is today.

I was sort of brought in--I usually get to do this right after breaks or right after lunch. I usually keep people awake or keep people moving on it; maybe keep people thinking about some questions. So they usually put me in after breaks. Also we have feedback.

What I would like to do today is talk a little bit about the drip shield, the engineered barrier. I want everybody understanding where the drip shield is, how it fits and how it deals with engineered barrier systems, to goals. And what is a drip shield there for?

We heard a lot of good information from Jack Bailey this morning, also from Bo Bodvarsson how the water moves through it. Then I want to take a look at the principal factors. What are the principal factors in choosing a drip shield and how does it behave and how is it designed?

Then the uncertainties, what are we looking for the uncertainties? We're looking at a probabilistic distribution on uncertainties; we're looking at that. And supporting data, what is the data that we're looking at to support those uncertainties, support the design and also support the performance assessment process.

And some of the future activities--what is going on? As we heard today, we are getting ready for the site recommendation revision 0, and as we heard earlier, it's a continuous activity. If the information doesn't make into Rev. 0 it will be put into Rev. 1 and then move on to license application. So the information will be incorporated as the information's available and we can move into it.

Going into the engineered barrier system, how does it look? This is sort of the drift, we do have some steel sets--and right now the understanding is that the steel sets will be there only in the areas that are required. So if the ground is good the steel sets won't be there, but there might be some other--not shotcrete--but removable--removed all the concrete--but what we're doing is maybe putting some steel sets and some anchors up in there.

What we're looking at is this is the representation of the waste packages, as you can see, the 21 and the 44 and the Navy long are in there. The interesting things are is that we do have a palette design that supports the waste package. That keeps it off the drift; also makes it easy for emplacement to the 10 centimeters apart.

Now the topic that we'll be dealing a great deal with today is the drip shield right here, which we have a cutaway so we can see the waste packages. With the EDA II, the license application designed evaluations, we've gone to a line loading which pushes the packages very close together. This also helps us in the sense that we don't have to have a drip shield that stops and starts.

What we do here is provide a drip shield that is also continuous, and right now it is self-supporting and you're seeing it before the backfill goes in. Again the drip shield is intended to go in at the point of closure of the repository. So the drip shields will go in, and there's now--we're looking at, with backfill and without backfill in the design evaluation areas.

So that sort of gets us in the formation of where we are. Some of the materials that we'll talk about a little bit later, but I want to point out the steel sets and the invert material are carbon steel at this point in time, and there will be some crushed material in between t his area so as to bolster that area up, so actually you would not see the cross members down underneath there or the support system underneath there. They would have some crushed material in that.

So that's where we are with the EBS. If you would take that out and sort of refer to that, that's where we will always go back to. So next slide please.

Goals, addressing the uncertainties--one of the questions, what were the uncertainties in evaluation of this. As we heard today from Jack Bailey was the drip shield added a lot of performance to the--transport the radionuclides. So what it is during the EDA II evaluation, we said this is one area that we need to investigate and then put into the system to see how it performs.

So since it added a lot of performance what we're looking for is the sound technical bases for it. Now that's what we're doing since EDA II. We're going back, engineering and the science, all looking at the bases for this--we're defining those things to find the process model of uncertainties. Performance assessment is going off and doing that as we speak and working with them. And then also provide the adequate bases to support performance assessment and the design.

And I'm going to stress a little bit the design because I am the design engineer on this one, so you'll see more from the design side and how we feed performance assessment and how performance assessment comes back to us on that. So it's a bit of iterative activity that we're dealing with.

Why is it a factor? As we heard earlier today, the drip shield does provide a long additional life; if we take the waste package off you still have a lot of performance without the drip shield and also a lot of performance without the waste package. So depending on how we look at it, the drip shield really extends t hat.

As we noted earlier the waste package has a nominal configuration and environment we have today looks like would last close to 100,000 years with the Alloy 22 on the outside and the stainless steel on the inside. Truly, truly the drip shield is a defense in depth. We've looked at that, we've talked about that before. It provides us a defense in depth process.

It also helps us in the chemical. We talked about the nominal conditions; now we talk about the off-normal conditions, what happens if we do have some drips or some other chemical processes that take place in the near field or the far field where it would come down into the drift.

And we heard earlier from Bo is that the drips really went around--the water really wants to run around the opening. It's a matter of what's the probability of the drips coming in and also depositing some chemicals or other material on the drip shield. If we didn't have the drip shield it would straight on the waste package. So there again we're chemically shielding the package.

And third we're looking at mechanical, and you'll see this theme throughout the presentation. We have a general mechanical, mechanical kind of feel to it. Basically it helps the waste package from being damaged through time also, provides a sacrificial shield in some respects. And we'll talk about it a little bit more in detail and how that happens.

So there we have--why is it a principal factor? It really adds to performance; we've seen that through our--at least the simple evaluations that we've done that Jack has shown, and also I can tell you through working with the performance assessment people, this is a very important part of the PA activities.

Uncertainties--now I'm going to talk about the uncertainties and how they all play together. As we talked earlier, the nominal situation--the nominal situation, J-13 water, is relatively benign water, good balance pH; but what happens chemically with the uncertainties? And what we're doing is looking at the uncertainties and the bounding, the sigmas that we're looking at.

And I can't tell you exactly the sigmas we're looking at right now, but we are investigating to see how large those are. So that is what's under investigation right now. What we're looking at the drip shield to do is reduce the uncertainty of water that contacts the waste package. Basically reduces the sensitivity performance to the geochemical environment that the waste package is in.

I kind of look at the drip shield as an interesting event. I also referee soccer at both the professional level and college level. A center referee is the person in charge; he's the person who has to deal with the players and things; the sideline or the assistant referees have to support the referee. The drip shield is the assistant referee in this situation. He is helping that person make the right decisions and protect the players. So the drip shield's there to support and make sure the primary barrier stands for a long time.

Help mitigate water chemistry--we heard earlier before the drip shield will hold the chemistry up and if there is any evaporation it will hold it there and not on the waste package. And it also will distribute the water if we do get a large influx of water. It will distribute the water on the outside on the tails, away from the waste package, into the drift, and it'll go into the natural system that way.

And so with all these things we're investigating, what are the uncertainties, what's the probability of this water coming in? What's the probability of the rock drops that we're dealing with, gaps in some backfill areas that we have to deal with? So those are the uncertainties in distributions that we have to work and understand.

So with that, we'd like to go to the next one--reduction. What we're doing here is to reduce the uncertainty in the models themselves. And we have tests underway right now. There is--I think the last trip we had out here for some of the Board was go out to the Atlas Program or Atlas facility and actually see some of the tests; and those tests are underway right now.

They have found some very interesting results on that, they've put at lot of moisture into it, heated some areas up; and one of the things that we all looked at, we wanted to see if we could actually get some recondensation underneath a drip shield. We simply couldn't make the drip shield drip inside, or rain inside, the drip shield. So that test and that report are being put together right now.

So I mean those are things that are going, so we're very sensitive to make sure that we understand that. We have that pilot test going on with the EBS and understanding what's going on. We also put a lot of moisture in there to take a look at the distribution of moisture through the drip shield, above the drip shield, below the drip shield. Those --that data right now is being sort of synthesized and put into a form that engineering can use and go forward on.

Again the severe conditions and aggressive conditions, what we've always done is that the very nominal conditions seem to make the system last for a long time. Again the waste package by itself can last 100,000 years in the nominal configuration. What we're looking for is the tails, what do the tails look like? And so we're putting a lot more moisture into the system and a lot more evaporative conditioning than we anticipated.

And on the mechanical models we're also looking at the strength of material, the titanium 7 that we're dealing with. We're also look at stress corrosion cracking of the titanium 7. We also have a lot of experimental work with Lawrence Livermore National

Laboratory in their corrosion tests right now for the materials that we're dealing with, in an aqueous system and sort of a bridge system, and also in a gas environment. So we covered all three variations.

So what we've tried to do it put together a testing program to help the uncertainties and bound the uncertainties such that we have a good understanding of how all the different avenues play, the chemical, the mechanical activities, play together.

What I'd like to do now is that we heard a little bit about the uncertainties the more we're doing the testing programs. What I'd like to do now is bring you back into the design. How does design sort of synthesize this information and come up with a credible design that meets the requirements? And also helps performance assessment in that it comes back, performance assessment gives us some insight on how the design should be handled from then on.

The general requirements that we're dealing with right now, again the preliminary one, is that the design life of the drip shield should be round about 10,000 years. And it's the early time frame, so essentially we have the early thermal pulse is over, basically the highly--you know, basically the chemical activities of the near field are essentially finished by then.

That does not detract from the performance of the waste package. The worst thing we could do here is have the waste package actually be accelerated or fail earlier because we have a drip shield. That was one of the reasons we took a look at if we'd had dripping water underneath it, would we get actually secondary dripping water on it? That was the one of the things we wanted to make sure of.

Divert the water around it, around the waste package, into the environment, into the far field, and increase time before water actually contacts the spent nuclear fuel--that's very important. Understanding we haven't taken a lot of consideration of the basket material inside either, there's a lot of performance inside the basket, the waste package also.

One of the things that we're investigating also is the different mechanical failure mechanism that we could have if we put a drip shield in there with and without the backfill. With backfill right now we're taking a look at is that the backfill--with backfill, basically the backfill becomes sort of a buffer or a spring. So it doesn't impart that much load to the--dynamic load to the drip shield. Without backfill we have to take a look at the rock drops and understand how they behave and the probability of occurrence of the rock drops. So those are all things that are going on right now.

With that I'd like to go into some of the material selections and some of the things that we've come up with for the current design that we have. This is beyond the license application design that we looked at before, so this is new information.

Titanium Grade 7 was liked because it has very good performance. As you can see, it has on the order of .03 micrometers per year of general corrosion. Very resistant to stress--to crevice corrosion--that's one of the requirements that we put upon ourselves; and in a stress relieved environment or stress relieved state it does not have--it's not susceptible to stress corrosion cracking.

The Alloy 24 that we have up there for titanium is actually, you'll see later, is a similar material. It's a little bit higher strength, and from a design point of view I need to put a couple of stiffeners here and there to make sure this 5-meter-long device can be actually handled and emplaced and also can sit there and take some rock fall. So that's why you see some of the Grade 24 there.

And also at the bottom here, as I mentioned earlier, the Alloy 22 is to essentially buffer the carbon steel from the titanium; and you'll see that foot and I'll explain that foot. As I mentioned earlier in the EBS picture, the lower support structure inside the drift are all carbon steel. What we're trying to do is sort of buffer the titanium away from the carbon steel there. Next slide please.

Okay, going into the detail of the design exactly, this is 15 millimeters of Alloy--not Alloy 22--but titanium 7. You can see there's internal supports on the upper roof of it. You'll see some supports here, some stiffeners there. Those are to handle essentially the handling loads and the rock fall load and the sand loads, static loads that we're having to deal with with backfill. This on the order of 5-1/2 meters long, so it's a standard unit.

There's no intent to have any special unit for any waste package. It will essentially be put in place above the waste packages after 50 years, right before closure. You see t his little hook there. That is simply a denotion or denoting a handling mechanism so the surface and subsurface people can handle it before it gets placed--emplaced.

We'll go into detail next slide, but we'll go into the skirt area--oh, thank you--right here is sort of a pin that helps us align it. We also have a skirt area

that 7
actually overlaps, as we saw earlier on one of the slides; it's to make sure we don't have any gaps or any kind of material can go in between the drip shield. Now next slide.

And this goes in some detail. I think this is a slide that only the designer can understand without some pointers and some labeling on it. This is to represent one drip shield here, there's one here, and the other drip shield's right here. And this is the interconnect part. All the drip shields are the same so there's no unique characteristics to it; simply places in.

Again the lineup in here, it's really--the designers did a good job. The team we had was--looked at seismic events and different relocation events and what happens if you do have backfill, if you have some motion because of your emplacement; and then if you do have some rock drop, if you get some dynamic load on the drip shield what would happen.

So that pin is there, actually designed to make sure there is no decoupling it, so you essentially have a continuous length and so you don't get any offsets due to that. Now one of the other questions we had is how do we get--how do we make sure that there's no moisture, any kind of water through--a gush of water coming in. Again the tails of the uncertainty bound. How do you prevent that from happening?

We well we put little moisture barrier rings right in here. One's up here and one is right here. Those are welded on, continuous weld--seal weld--onto it. So any moisture, if you have any kind of angle on it, would hit this and then run down. Remember gravity's our friend in this situation, so what happens, it hits those and runs down the drip shield.

Also on this side similarly would come past here and then also run down, so it never gets a chance to come through this gap that we have to have for alignment purposes and things in that nature. We have to have some area where you have to give the engineer some alignment area, some tolerance. So that's the tolerance area, but no moisture and no separation can occur. And again this is for 15 millimeters of Grade 7, so we have that, and so that's the design as it stands right now.

Some of the results that we've done--what have you been doing? We've looked at--from performance assessment to the uncertainty bends that we have. We've worked with--what we've done is take a look at the design to make sure it does meet it. We had a requirement from the performance people and also from metallurgists with a backfill environment. We would like to keep below 20 percent strength of yield--I'm sorry, of yield--by titanium to prevent stress corrosion cracking from even having a possibility of initiating. And that's been accomplished by the 15 millimeters and the stiffeners that you see.

Where we're looking now is looking at different rock sizes and finding the distribution. There was a very good report that was just issued on key block evaluation, and that has actually been updated a little bit now because in the key block evaluation we had the angles, I think 105, now we've moved to 75 degrees with the different key blocks, and it doesn't affect the different key blocks that come out; and actually, to our benefit, it actually decreases the size of the rocks and the distributions that we anticipate.

In the chemical evaluation, since we have the tests going on at Lawrence Livermore National Laboratory we're confident the titanium 7 will behave nicely inside the repository; and localized corrosion rates are very, very low in this environment, even on the tails. SO that's where the design is, and this is the results of it.

With additional work what we're doing is we are looking at the Atlas facility and taking a look at those activities and seeing how the circulation goes; and we're looking at performance model updates. From that information and from the information that we have, geochemical environment, basically if you have moisture that does drip on it, what are the chemicals that come along with it; what are the chemicals that are left there due to its evaporation.

Remember the drip shield will be the second warmest place inside the repository because the waste package is the warmest, and then the drip shields are on the order between 20 centimeters and four--10 centimeters away from the waste packages. So they will have a high temperature for a longer period of time. So we are looking at the geochemistry very carefully.

Rock fall distribution, that's in the work right now. We have a task team that's looking at different rock fall distributions, and at the different strata in rock fall. Basically all the rock doesn't fall the same in different strata, so what we're looking at is the distribution. So it's again a probability distribution, looking at what's the probability of a certain rock and what topography do we anticipate that. So we're taking that, all consideration, and wrapping it into the design requirements.

We're looking at design response to it. We have a dynamic code. We actually do real dynamic evaluations from the design point of view to see its instantaneous hit, what it does to the waste--to the drip shield, and how it protects the waste package in that sense. Do we get contact, don't we get contact.

Essentially when you have dynamic load you get a bend and it comes back up. An interesting part of that is a lot of times when you have a dynamic load is you would think that the highest tensile strengths would be on the bottom. It's actually not the highest; it's actually lower, so actually in compression because it's a plastic defamation, it comes down, it comes back up.

So the lower part of the inside of the drip shield is actually in compression only if you have a punch-through or a very, very high load that would set stress corrosion cracking; you would have a tensile stress there. So we're taking a look at those, making sure we understand that.

And also, again as I noted, we have some tests going on the low C road and we're incorporating that into the design.

With that, I think that slide--13--one more slide? That's it? Okay.

CRAIG: Okay, thank you, Tom. You know, if you ever get around to making a 1:50 scale model, I would like to have it because I need a new mailbox at home.

DOERING: It would last many years.

CRAIG: Okay, questions from the Board, Richard Parizek --

PARIZEK: Yeah, Parizek--

CRAIG: --followed by--

PARIZEK: --Board. Question--

CRAIG: Just a second, let me construct the list here. Parizek, Nelson, Sagüés, Bullen.

PARIZEK: Parizek, Board. Question about retrievability. How--does this complicate retrievability or is this thing easily dismantled if you need to get in there and start pulling out waste packages?

DOERING: Could we go to the very first slide, where they show the picture of the EBS? There we go. This design--our theory right now is that you would not emplace the drip shield until you make a decision on the license to close. So at that point in time you wouldn't put that in.

Now the question is if you have put backfill on, it becomes more interesting to remove it. But if you do have it in and they simply say there's something not behaving well, this is very simple to remove because it would just simply come off and just simply grab the first one, you bring if off and grab the next one--just comes right off as you put it in. So it's a very simple--bring the drip shield over the package and set it down. And reverse it, just pick it up and bring it back out.

PARIZEK: Continuation question, if there are say small rock falls that get in the way of where this thing is going to be placed, at time of closure, would you have to go clean this place out, muck it out?

DOERING: If it would hit right next to the package, lay right up against the package, the answer is--for this design the answer is yes.

PARIZEK: And one other question, what's the worst case failure scenario you imagine for drip shields? What could you do to really make one fail?

DOERING: To make one fail, what we're looking at is--we don't--with the chemical environment that we anticipate, we don't see there's a problem with that. The off-normal event where we'd take and look at that, we don't believe the titanium 7 would actually have a failure due to corrosion activity.

The only time we could really see if you were to get a high stress to a very large rock fall. This is on the order, you know, maybe half the drift would fall in. But at that point in time there is more difficulty than just the drip shield not doing well. Now you're dealing with a major rock fall before you close.

Does it make sense? I mean a drip shield is designed to take a design basis rock.

PARIZEK: The question is the drip shield's in place, you've closed the door and then the drip shield fails. You don't intend to retrieve the package, but in terms of just performance of the whole repository, how that factors into the--

DOERING: Again--

PARIZEK: --mechanisms.

DOERING: Okay, going back to Jack Bailey's presentation, you can see, if we do have a localized failure of a drip shield it probably won't affect the overall performance of the repository. We do have the waste packages directly underneath it, which has the long term performance material on it too, given it different barriers.

So we don't see a few failures of the drip shield as detrimental to the overall performance of the repository.

PARIZEK: And there's no such thing as juvenile failures of drip shields?

DOERING: We'll look into it, but the answer is no.

CRAIG: Alberto, hold off for just a moment if you would. As you all know, the Board likes to take questions and comments from the public, and one's been handed to us and it's a good one. So I insert it.

What is the cost, how many, how will they be placed in Yucca Mountain?

DOERING: The costs, depending on the variations, I think Hugh Benton has the latest cost on the drip shields on that. I think he brought them in this morning, since we just priced them. Let me go into how they're--second part of the question, how are they going to be emplaced?

CRAIG: How many?

DOERING: How many? There will be on the order of around 10,000 segments--on the order of. Again the waste packages are on the order of 5, 5-1/3 meters long, so are these; they're very close to the same length.

CRAIG: And the last is how will they be placed?

DOERING: Emplaced actually be a gantry system similar to the waste package emplacement system, essentially just simply the gantry system. We modified to grapple the four lugs at the top, the hooks, and just take--the gantry takes them in, just sets them in.

And Hugh has the latest costs.

BENTON: Benton, M&O. The--each drip shield segment costs a little bit over $200,000. Total cost for the entire repository, the SR design, is of the order of $3 billion.

CRAIG: $3 billion. Thank you very much. Alberto.

SAGÜÉS: Priscilla first.

CRAIG: Priscilla--oh, I'm sorry, Priscilla and

Alberto.

NELSON: Thanks. Nelson, Board.

DOERING: Let me add something just to that cost. A lot of that cost is the grade 7 titanium. Palladium prices have been going up and down a bit and we're up in the peak right now, so the price within the last month for palladium has gone up.

NELSON: That's right. Nelson, Board. I want to take some sense of satisfaction that the project is doing the work that they're doing on rock falls, probabilistic approach, because I think--well warranted, and I look forward to more information derived from it.

What I'd like to ask you just generally is what are the seismic design requirements? What--what is--what are you designing for in terms of seismic event and to what extent does it control the design? And I guess there's not only the underground accelerations that you'd be working with, but also the possibility of displacement as opposed to just accelerations. Can you tell me about that?

DOERING: I can go into the accelerations. The displacements we haven't worked in that detail yet from the design point of view. The accelerations right now, we're still working with a .66 g acceleration. We're looking all the way up to 1 g--

NELSON: Vertical?

DOERING: Yeah.

NELSON: What horizontal?

DOERING: We have to bring that into a horizontal. That's--our designers have to bring into the frequency and the vibration processes. I didn't bring those slides with us, but there are a whole bunch of different frequency evaluations that we do--what frequency to worry about.

From a waste package and support system it's not only the vertical, the horizontal, but also what we have to do is what frequency does the package and the palette resonate at. And so we're looking at those, and we actually do have that, and I just didn't bring them with me.

NELSON: How much does that--does that control various aspects of the design very strongly?

DOERING: What it couples to, it's the waste package support palette. That's where it's controlled, because what we're doing there is we're forcing the requirement into the palette design to make sure the package doesn't fall out or move out of it, nor the palette move along the drift. So we're--

NELSON: That's for the waste package though. What about the--

DOERING: The drift--or the drip shield has a similar one, where we're taking a look at different vibration modes, and seeing if we need to couple it. Right now we don't see a need to couple it to the support system, but that's one option. Right now this one behaves, from the very limited evaluation we've done--we've only done limited because this is relatively new design--we don't see a problem with its motion at all.

If you put it in any kind of rock fall, anything gets around it, you sort of stabilize it that way; but this one is pretty stable as it is. Remember this is over five meters long and over three meters in diameter--or wide--so it's a pretty big stable thing.

NELSON: Are you planning on doing a displacement consideration for discrete fault displacement?

DOERING: I don't know. I have to take a look at the geotechnical people to see if that's part of the requirement. Again, we're on the design side, so we wouldn't respond to that. So we haven't heard that one yet, so.

SAGÜÉS: This will be just about the largest titanium application ever built, I believe, correct?

DOERING: I think the Russian submarines beat us by a few meters.

SAGÜÉS: I see. Well I was talking about the integrated thing. Each drift would have about kilometer or so worth of titanium, and now that creates a couple of interesting questions. First of all--of course the integrated thermal expansion would be in the order of a meter or two, and I presume that there is some gap in between there so that each renovation expand a few millimeters?

DOERING: Right. That's why you see in that one slide, the very last slide with the coupling, you see there's a gap between the drip shields. And as you note--there we go--as you note, this pin is not a tight fit pin.

SAGÜÉS: Right.

DOERING: It provides some movement, so we have to have some movement through the thermal expansion. When these things go in though, you have to remember the system is already pretty much stabilized thermally, and the repository after 50 years in the drift has stabilized. Now the repository in general is still warming up. But around the drift it's pretty much reached its maximum temperatures.

And so what we're doing is putting in through a very, sort of--not a high rising--there's not a large thermal swing.

SAGÜÉS: You mean you're putting in place already hot?

DOERING: No, we don't warm them up before. I'm saying the repository, the environment itself, it's not a quickly varying thermal environment when we put them in.

SAGÜÉS: Right, but when you close the drifts and then the temperature begins to go up--

DOERING: It'll come up--

SAGÜÉS: --then that's going to--

DOERING: Yes.

SAGÜÉS: --has to come of it for that kind of a-- right.

DOERING: That's why that's--

SAGÜÉS: Now--

DOERING: --that's why the gap is there, that's why the design is the way it is, because we have a skirt that overhangs--

SAGÜÉS: Right.

DOERING: --to make sure that we don't get any separation during seismic event, if we get any kind of buckling. We know we're going to get some motion, but how much--and this will hold it together. And that prevents any material getting in here or any kind of water from getting in there.

SAGÜÉS: So that there--

DOERING: --also thermal.

SAGÜÉS: The friction coupled against each other with a plate on the pins, and now when--have you figured out anything about the stresses that would develop when they accumulate against each other? Like for example could it be --is there any way that they could be like lobbed against each other, friction-wise, and you will end up developing say tensile stresses considerably, around the coupling that--

DOERING: Well--

SAGÜÉS: --induce--because, you know, again the integrated expansion, even in individual shield, should be on the order of millimeters. That's not a trivial amount to accommodate, is it?

DOERING: Not on the lengths we're dealing with, and so that's one of the designer's activities. I didn't bring that calculation with me, but it's something that our designers have looked at and looked at thermal expansion on that. We don't believe we would get any kind of high stresses due to, you know, essentially buckling or essentially, you know, interference on that. That hasn't been a difficulty with this design.

SAGÜÉS: Um-hum, and the possibility of the cold adhering against each other after being for many years together, touching, that's not a consideration?

DOERING: Maybe I didn't understand the question.

SAGÜÉS: The possibility of their cold adhering against each other--

DOERING: Oh.

SAGÜÉS: --after being--

DOERING: Titanium has--

SAGÜÉS: --no?

DOERING: We don't believe so. I mean if you take it out in space where it doesn't have the oxide layer buildup; but titanium loves to build nice oxide layer up.

SAGÜÉS: Sure. Of course when they scratch against each other the layer is destroyed--

DOERING: Right.

SAGÜÉS: --you know.

DOERING: But with the titanium Grade 7 that layer is generated very quickly. That's one of the reasons why welding, abrasing titanium is very difficult because the oxide layer comes back so quick. So that--essentially the oxide layer acts as a sort of a lubricant in that area and prevents the galling like in stainless steel 3 or 4, which doesn't oxide, you know, doesn't have that oxide layer very quickly.

SAGÜÉS: I see. And then the other thing is again, this sort of another--sort of -- ask it, would be that we would have--again kilometer range long chains of titanium metal, has anyone looked at things like the possibility of dielectric currents or some such events? Have you seen pipelines, you know, -- and you end up having currents running from one end to the other--

DOERING: Oh, current--

SAGÜÉS: --possibility?

DOERING: That one we haven't looked at, so to get to the point, we have to take a look if we induce any kind of current in the system.

SAGÜÉS: Thank you.

DOERING: Thank you.

CRAIG: Other questions from the Board? Dan Bullen.

BULLEN: Bullen, Board. Just a couple of quick questions, Tom. If you place these packages--or excuse me--place the drip shields will there be an event where you'd say--Bo told us there were some highly fractured regions that they saw on the lithophysal zones--would there be places where you wouldn't put a waste package? And if you did put a waste package there would you put a drip shield--keep the drip shield continuous, or would you just not put the drip shields either?

DOERING: The decision hasn't been made on that one yet. There's two options at that point. We can either put a cap on the drip shield and put a standoff so the drip shield doesn't--isn't there, so essentially the drip shields now have a new design, essentially has an end; or we could put it continuous if we don't believe that's detrimental. That decision simply hasn't been made yet.

BULLEN: Okay, and then I guess the other question that I have with respect to your rock fall analysis, the biggest gap--or excuse me--the smallest gap that you have between the drip shield and the waste package is now about 10 centimeters?

DOERING: Yes.

BULLEN: Okay, and so if you had a rock fall that essentially didn't deform but displaced the drip shield you wouldn't cause a crevice to corrode--a crevice between the waste package and drip shield by moving--moving the drip shield over with the rock fall? I'm thinking of a rock fall off center that wedges it sideways and basically moves it. Has that analysis been done?

DOERING: That's going on right now, but the palette--which I didn't bring, which I'm sorry I didn't bring--palette design has a system that prevents the drip shield from coming in--

BULLEN: Okay.

DOERING: --to contact the waste package. We call them the bumpers.

BULLEN: Okay, but the crevice would be between the palette and the drip shield--

DOERING: Correct.

BULLEN: --so there's potential degradation mechanism there, but it's not the waste package that has the crevice.

DOERING: Correct. That's the intent.

BULLEN: Okay, thank you.

CRAIG: Okay, do we have any questions from consultants or staff? Don Runnells.

RUNNELLS: Runnells, Board. You mentioned very quickly a footing of some kind to prevent--provide a buffer between this material, and I think you said the carbon steel?

DOERING: Correct.

RUNNELLS: Could you explain that just a little bit more as to what that is and why it's there?

DOERING: Okay, basically what we do, on the bottom of the drip shield there is an angle, basically an angle iron attached to the bottom of a drip shield. That angle iron is made out of Alloy 22, which plays well with titanium--it gets along really well with titanium because there's no galvanic couple setup there.

Also it deals very well with carbon steel. Since the invert has a lot of carbon steel on there, we didn't want the titanium to be any--susceptible to height or hydrogen pickup, which some titaniums are. Titanium Grade 7 doesn't have that characteristics, but we wanted to make sure that that system or that probability of occurrence is simply taken off the table.

So we just put small little angles of Alloy 22 in the bottom sort of as a spacer in between the invert and the titanium Grade 7 drip shield. Does that make sense?

RUNNELLS: It makes sense, yeah. Thanks. And following up on Alberto's question then about currents being developed, have you analyzed the possibility then of the generation of galvanic cells in that three-metal system?

DOERING: We believe that the--again, if a galvanic cell would be set up, there was some dunnage or some rock underneath there, the allow or the carbon steel would go first. So that's the intent, so the carbon steel would be sacrificial to that.

CRAIG: Okay, any other questions? In that case, thank you very, very much, Tom.

DOERING: Thank you.

CRAIG: And we turn to the last presentation of this session, which I'm inclined to think of as the Super Mario or Game Boy part of the session, simplified model available to everyone. Actually I like that kind of thing, so that'll be wonderful.

Mark Nutt is going to tell us about a simplified performance assessment capability. Mark Nutt works for Golder Associates. His doctoral research was in the area of performance assessment, evaluating high level nuclear waste forms that would be generated by the Oregon National Laboratories Electro-Metallurgical Treatment Process.

And we look forward to learning about the simplified model. Again, I'll warn you a few minutes before your time is up if necessary.

NUTT: One thing you forgot is where I got my degree from and who I studied under, who was Dr. Bullen over there.

CRAIG: Dan Bullen.

BULLEN: Don't mess up.

NUTT: Don't want me to embarrass you, huh? I'll try not to. In this morning or day session I feel like I'm kind of the odd man out. You're hearing a lot of new information that was talked about this morning. You're going to hear some new scientific studies that are going to be talked about this afternoon.

Some of the information I'm going to present here is based on an old model, but it's a new way that we're pursuing within the project to try to communicate some of the aspects of the performance assessment. If I could go to the next slide.

So what I'm going to do is start with overview. I'll give a little background of what led us to this effort, objective of the simplified TSPA effort, and keep in mind we are--or I feel we should be looking for a name change. The simplified TSPA is what we started with and it's kind of stuck with us. But I feel we need to come up with a better name.

That said, I'll talk about the software that's being--that we used on the project, on the task, the current status of where we're at, and what we're doing right now. So with the background, you've heard many talks about how complicated it is to present a TSPA type analysis. Especially to technical experts it's difficult to understand --takes a while to come up to speed on what you've done; and to the general technical audience.

This results from the complexity of the system you're trying to evaluate, which Yucca Mountain is a very complicated systems, lots of processes going on, lots of things that have to be modeled. These result in a complex model itself. It's necessary for compliance type calculations.

Everything that's important that could possible affect performance has to be included in the model or else you feel that you've missed something. SO you have to be able to assess the sensitivities of these--every factor to see if it impacts the end result.

It's also difficult due to the representation of uncertainty and the alternative conceptual models involved. You have to be able to carry those into the model. You have to be able to communicate them; you have to be able to explain what you've done.

There's also limitations of the system software that's been used in the past. Dr. Bullen's familiar with using the old RIP software; kind of cumbersome for people to us, and the linkages. We have received some constructive criticism regarding model transparency from this Board, from the USGS, from others.

Another aspect is the organization that we work with helped doing the technical review of the PA products, among other products that are produced for the project. So we have to thoroughly understand the models that go into it, and this task and this effort supports that role of helping do the technical review on the project side of the PA products.

So what was our objective--what do we aim to do? First we wanted to start off developing a tool to help communicate to a general technical audience. And where we're aiming at with the end result of this task is roughly high school graduates to college professors, kind of with a technical background--somebody that wants to understand what's going on at Yucca Mountain, how you expect it to perform.

What do we need to communicate? What is a TSPA? What is the black box magic that everybody refers to?

How does the model work? Because in the end result we want to explain how do we--how do we expect the repository to perform. Part of that explanation is well we've modeled it. How have we modeled it--we used the TSPA. So we have to get across the whole aspect of how the model works, what it is; among other things, to explain to this audience how we expect the repository system to perform.

By doing this effort it also enhances the technical review capability within the project. It helps ensure the transparency of the TSPA models themselves to the underlying documentation. So in a sense, can the model be reproduced? Can model analysis calculations be reproduced by somebody just picking up the documentation and sitting down and trying to do it themselves?

So what we started is a two phased approach. The first phase, it's completed, all status on right now, is a prototype model that was based on the viability assessment; namely to get our feet wet in the process, see what we need to do, get some lessons learned; followed by a simplified SR model that we're undertaking in a parallel effort to the TSPA-SR development. Next slide please.

Going into a little bit about the software that we used. It's kind of set the stage. We've used what's called the GoldSim software. It's the same platform that

TSPA-SR will be built on. It's an evolution of the RIP program that was used for past TSPAs, VA, TSPA-95 and on back; has the same analytic capabilities as RIP, a few enhancements in some areas.

Primarily it has an improved user interface with good presentation capabilities that we on this side--on the simplified PA project took advantage of. Some of the features of the GoldSim code, it has the ability to link to external codes and routines. If there's some aspects of GoldSim that the user doesn't feel do the job adequately that they need to do, they can write their own source code and have GoldSim call it up.

TSPA-SR will do that. They do that in several instances. They feel it needs a little more horsepower in certain aspects of the model, so they call out to these routines or full codes that are written.

Another aspect's the model and results are self-contained, so you have an input deck, you run the code, you get the output, it's all self-contained within a package. You don't generate like reams of output you have to go through. It's all in a software package. Then if the user goes in and makes a change to that package, the results get erased; so it maintains some control within inputs/output.

You have the ability to link to external data sources, for example control database. You can have GoldSim link to it, pull the parameters out, date stamp that that's when it got another software or model control feature. It can also be--the features of GoldSim allow it to be documented internally.

You can document using--there are some what are called notes features, various other features, to document the model--where you got your information from, your source data, your conceptual models. And if you want to do even more you can hyperlink just like a Lotus--or an Explorer browser, and go off to additional documents that will support that model. We have used some of the hyperlink features.

Some of the user interfaces that make it a nice package to use for a communication type aspect is it's a graphical and object oriented program. You can drag and drop pieces, you can pull in icons, you can have pictures, you can do all kinds of stuff with it to make it a presentation capable software. The model itself can be presented.

And that's what we've done. If you get a chance we've got a demonstration of the actual--one of the models in the back that show the graphical capabilities of the software.

You can structure the model on a component basis, so you can put ever model piece parameter, expression, variable related to one component together. Almost in like--if you can imagine Windows Explorer. You can set up folders. We can set up containers; in each one of these you can put everything that has to do with that model.

So unlike the old version that as used for the past PAs, pieces of the model could be all over, and it was difficult to pull them together and understand where things were at. So you had to be an expert in navigating the software, understanding how it worked, to be able to figure out how the model even worked. This one allows you to pull things together.

You can also u se a hierarchy to push the details down, and this is more for aiming at audiences. Some people want to see how the system works on a top level, maybe how release rates and radionuclide masses move from one place to the other. That can be done at a top level. But you can push the engine down, the actual calculations that drive how that happens, down to further levels. You're not hiding them; you're just pushing them down so that you don't clutter up the up-front, where you're really trying to get the message across.

You can add ancillary text, figures and pictures in the model to help really explain what's going on, support the data, support the model; and you can add results elements in any location. So if you want a subsystem release, you want to see how the engineered barrier system is releasing radionuclides over time, you can add it in that component on engineered barrier system releases. After the model runs, doubleclick on it, see what the result looks like.

So it's a very powerful tool for being able to show the model, show the results, show the inputs, document it, and I invite anybody to come back and have a look at what we've got in the back of the room. Next slide please.

For phase 1, which we've just completed, again it was a prototype, it was a simplification of TSPA-VA. It's called a proof of principle, it was to get our feet wet, see what we could simplify, what level we could come down to, how best to package the model and what other things we possibly need to do to get across the communication aspect of it.

And I got the bullet--simplified does not mean simple. It's still a very complicated model. It's a complex process. We ended up having a pretty big model. We've included all the component models in the VA, from climate, infiltration, all the way out to biosphere. All the same components that you saw in VA are in our simple model.

Some of the VA models were simplified where we could, and what I mean by where we could, some couldn't be simplified without affecting the results. If we went--and the examples are EBA transport and seepage. If we were to try to change those much, we would have missed our constraint --which I forgot to mention.

We had a constraint that we put upon ourselves that we wanted to reproduce the VA results; we wanted to stay faithful to the VA since we were trying to get a model to help communicate the VA. We tried to stay--we aimed--that was our aim. So it forced us that we couldn't simplify some of the models. EBS transport, seepage were a couple of examples. We had to stay with what we did.

Some of them were sufficiently simple, as they were included in the VA that really didn't require us to do anything else. The climate model, for example, was just--if you recall the step changes to a different climate. We just kept that one. The biosphere was just those conversation factors that took concentrations, multiplied them by a number, and gave you a dose per radionuclide. We stayed with that value.

Others were significantly simplified. How we represented the EBS, how we represented--used the unsaturated zone and saturated zone flow and transport. For example, for the unsaturated zone transport the TSPA-

VA calls out to a three-dimensional particle tracking routine that takes masses output from RIP, tracked it, put it back in, and went on its way within RIP.

We didn't do that. We used the features within the GoldSim to build our own unsaturated zone transport algorithm to model it--much simpler, same conceptual model, just a different approach. Next slide please.

What we ended up with was a self-contained model with results that are consistent with VA. So as you can see, these are the VA results, these are what we came up with. These are the 100 realization runs on each case for the three periods, 10,000, 100,000, million years; same with this one. So we're very close, so we felt we passed the test on maintaining consistency with the VA.

And it is a functioning model. That model sitting back of the room functions. A single realization requires about one minute of simulation, of run time. And that's not --I'm not doing this to brag, that we're fast, we can do it quicker, we can do it better. I'm doing this because for the next phase we needed something to run fast, we needed--we didn't want--and I'll get into that later--we needed something that moved quick. Next slide please.

What we did with the communication aspect--and after this page I'm going to s how you a few examples--and those examples on the next few pages are actually screen grabs that I pulled out of GoldSim. We had an introductory page to set the stage.

We gave an overview of geologic disposal and the Yucca Mountain Project, a primer on performance assessment, a primer on risk in the context of geologic disposal, and brief summary of design. And the aim was to come up to a higher audience level.

These are all hyperlinks to semi-interactive presentations. In this example some of them call up your Internet browser and run you through essentially a text presentation. Some of them call up PowerPoint viewer where we've written some presentations in PowerPoint and they dance around and allow the user to read some text and what not.

We've also added results toward the top of the model in a concise fashion and presented them on a component by component basis, so they're all up front. If you want to go look at the climate you can see a result on how the climate's moving. If you want to see releases from the waste package you can go in there and see the releases.

We also developed the subcomponent model structure, the overall model, on the hierarchy to push the detail down, as I talked about earlier. We pulled the importance up at the top, mass transport and the general model structure, and we put the detailed calculations that drive the model underneath. They're still there; they're just lower; but that allows the user to explore, browse the model at any level they want. Next slide please.

These are just example screen grabs. This would have been the introduction page, and it can be on the machine back there. There is the overview, the risk discussion, the PA summary, repository design and the all important how do you navigate the software.

Some of them are, like I said, links to a PowerPoint viewer that brings up a presentation. Some of them will put up your Internet Explorer page and load up a HTML file. Next slide please.

This shows an example of how we did the results together. If you can imagine, this would be like in your Windows Explorer, this would be a folder. You doubleclick on that, you'll pull up another folder--it's difficult to see up there--you doubleclick on this one about seepage, you jump down to here, you see an element expression--let you pull up a result--and you pull up a result; all self-contained within the model, but it's just different layers to let--to pull it where you want. Next please.

This is how the model was put together, and you can see how GoldSim kind of works. It has a typical

Windows Explorer type thing, different browser view over here, graphic view over here; and you can see--you can doubleclick on this one, it'll pull you to that one, it'll pull you down to the actual seepage model.

So we go from the repository level to the drift seepage down to the model that puts together the seepage. These are actually--further--you could further click on these and go down and find more of the engine behind it. Next please.

What else did we do for communication? We did heavy documentation on the model. We included summary notes with each graphic pane. We had hyperlinks to the detailed explanatory text of how that model worked. In some areas where we didn't do a whole lot of simplification, they weren't all that detailed. They just kind of gave a little summary about it.

Other areas they were pretty heavily detailed since we did some pretty major changes, but in all instances we had hyperlinks to the VA documentation. So if you were in the software using this, you were looking at one of these discussions, you could doubleclick and you'd be right to the VA document if you had a connection to the Internet, and go out and see the basis behind the model we put together.

We also had hyperlinks to what I call semi-interactive discussions on the various subcomponents. These were again done with PowerPoint viewer. They would discuss each component, seepage, waste package degradation, waste form degradation.

What we included--these, at a higher audience level we aimed at, was what is this component, what is this piece? How does this piece affect repository performance, so why do you have it in the model itself? How we modeled it on the project side; you know, what are you doing for modeling seepage, what are you doing for modeling waste package degradation? What are your results.

We did a summary in more detailed level. Again we had hyperlinks to the TSPA-VA and supporting documentation in those to take the reader to really where the basis is, the real basis for the models we put together. We went on the emphasis of how that component works rather than more why. And we used the ability to link to the project's existing documentation to allow the reader to really understand why.

This page gives an example of this, still another grab. These here are the summary texts on the graphics pane that attempt to explain what these two do. These are actually expressions within GoldSim. They're mathematical operators. You doubleclick on one of those, it'd pull up a dialogue box that said "How am I going to set this parameter?" These for example are essentially "if-then" statements; if something, then this. And these texts kind of tell what it is.

These are the two hyperlinks to supporting documentation. One is the component model discussion of PowerPoint viewer. One is the actual implementation into the simplified model, and you can also add some notes that show more detail on where the data source came from. So you can do some heavy documentation within GoldSim to allow the reader to see what's going on.

What I said was that Phase I was a get our feet wet--what do we do, how do we structure. So we went through the effort, we looked at it, we've shown it to people like we're showing it here, eliciting feedback on where to go with Phase II, and we've learned an awful lot.

So we're now embarking on our Phase II model development and what are we doing with Phase II? First thing --one thing we're doing is refining the model based solely on TSPA-SR based solely on the analysis of model reports that are being generated by the project. What we're doing this for is to support traceability, transparency of the AMRs. Can we reproduce the TSPA-SR calculations independently?

And that will--by doing so, we'll be able to provide feedback to the authors, to say well we can't quite do it this way, we don't understand what you did. And that will, we feel, help in the transparency issue of the ultimate AMR.

We may simplify multiple levels. We may bring it up another level, and an idea we've had is build the principal factor simplified model that maybe only works off of seven or eight--the seven principal factors. These are all just thoughts. We're still working with what we finally want to end up doing. We need to refine the documentation of how the simplified model works.

We're also having a parallel effort to enhance the communication capabilities. We want to enhance the subcomponent discussions based on the current understanding, to be consistent with the PMRs. What the goal is, to bring the PMR discussions up to another audience level, to get at more people. Next please.

We're also investigating the what-if capability of the user. The demonstration in the back has a pane that has "What-If" on it. That pane's a future enhancement. The what-if button on that model back there doesn't work today. The intent is, or the hope is, to get it to work in the future, and what we want to do is allow the user to set uncertain parameters--if we don't figure out how many we want--and execute the models.

The parameters will be set within a predefined range, say the uncertainty bounds that are allowed in TSPA-SR. The user can pick three or four parameters they want, of their choice, and run the model. The remainder of the model will be locked. We also have to investigate a way to lock down the GoldSim so the user can't go in and change parameters on their own, build their own model, do whatever, if we decide to release this out to the masses, or the public.

We also are aiming to develop an animated simulation of repository performance. We're looking at how the system works and illustrate the importance of various components, what each component does--a little animation simulation that we're aiming to run from biosphere or climate all the way through how each one works, how they impact performance; kind of give the flavor for how--you know, the movie to support the text of how each component works.

We're also investigating doing a dynamic linking to the model so if the user changes something up here they can kind of see in an animation fashion what the end result of changing that is. If you change infiltration you may change the infiltration portion of the animation to show a little different picture.

But this is, as I said, a work in progress.

We're just really initiating it right now, and we elicit feedback from any on how best to proceed or best to communicate these types of aspects. And with that, I'll close.

CRAIG: Thank you very much. I've got Richard and Jerry and Priscilla. But I'm going to throw in one just because I've got to take advantage of chairman's prerogative.

To what extent can I go--use your model to go back and ask for first principals or fairly fundamental understanding? For example, if I'm interested in corrosion growing by a diffusion limited mechanism and I want to look at the square root of time evolution, can I go in and get at that kind--

NUTT: No.

CRAIG: --understanding?

NUTT: No. It's--we're taking the results of TSPA and bringing it--essentially a higher level abstraction. So for waste package degradation what we did in the Phase I and probably what we'll end up doing with the second phase, is the abstraction that'll go into the--the VA was a waste package degradation, number of waste package failures as a function of time. It's uncertain, so the number that fail over certain time frame changes. We just took that data and used it. We didn't--we abstracted their abstraction, per se, and it brought up one more level. So first principals.

PARIZEK: Parizek, Board. A similar question, you would not replace existing models--

NUTT: No.

PARIZEK: --being used. This is really to help edify what's going on in those models and the findings.

NUTT: Exactly.

PARIZEK: So you still would use yours in conjunction with theirs, the programs in other words?

NUTT: Yeah. The TSPA-SR will still be done, the same group that did the VA, the same efforts. Ours is just a companion to try help communicate. That's the real intent. The added benefit is it helps us as technical reviewers to understand what's going on. So there's no replacement, no.

COHON: Cohon, Board. So did you learn all this from Dan Bullen?

NUTT: I taught myself.

COHON: Good answer.

NUTT: --Dan's support.

COHON: Good answer.

NUTT: He just pushed me in this direction.

COHON: You said that the audience would be one with some technical background.

NUTT: Yeah.

COHON: Have you had interaction though with non-technical members of the public?

NUTT: Have we had any reaction--no.

COHON: Any interaction with--

NUTT: No, we haven't.

COHON: Have you thought about how to make this sort of a simplified, simplified model?

NUTT: Thought about it. I guess--sorry? Well that's part what we're aiming at to get at with the animation, to bring it up to that level. But also maybe with what I talked about earlier, the simplified, simplified model that gets at the seven principal factors that are controlling it.

And I realize that this kind of has to explain what the principal factors are and why you got there; but, you know, hopefully we can do it so a higher level audience can understand it; but, you know, that opens up tremendous amount of effort, and it probably should be done.

COHON: I understand that, but the potential here seems to be terrific. Did you hear our session yesterday about uncertainty?

NUTT: Um-hum.

COHON: Have you thought about how to communicate and quantify uncertainty to the users of the next model?

NUTT: Thought about it. I don't know if we came to a conclusion. I was very interested in what the discussions were yesterday and took down quite a bit of notes. We have to do it. We have to come up with a way.

COHON: I'm just probing to see if we can get some advice here. I mean do you have some thoughts about it or is it too soon yet?

NUTT: It's too soon.

COHON: Okay.

NUTT: Sorry.

COHON: That's fine. Thank you.

CRAIG: Priscilla.

NELSON: Nelson, Board. We all have good ideas, I'm sure, how to extend any work that we hear about. And my contribution is the possibility that in a time frame work that's very important to people trying to understand the project, to not only look out towards the 10,000 years and beyond, but perhaps to have the capability of looking what's going on during construction. In a time frame work that I'm sure you could do and I'm sure that that's--many people will want to link into that.

NUTT: Look at what's going on in terms of--

NELSON: I think--yes, and in terms of schedule and cost, way of integrating that aspect. And it's not really

PA--

NUTT: Yeah.

NELSON: --but it goes along with that in a short time scale. I think we've always had a question about perhaps technically and policy-wise people are interested in the 10,000-year regulatory time. But there's also a wish to really understand the time that's more comprehensible.

And this tool could pretty readily do that, both from the standpoint of the what-ifs and leading on to the longer term response, based on what happens short term during the thermal pulse. So I just really encourage you to think about that shorter term as well as the long term PA prediction.

NUTT: Okay.

BULLEN: Bullen, Board. Dr. Nutt, I have a couple of quick questions as a professor who gives students things like this and says go tinker and find out what's wrong. You mention that you could do sensitivity analyses and set the number of iterations, and it took 100 seconds or whatever for one iteration to do.

Have you got some way to control for example the reasonable bounds of what you're doing? For example, if you did one iteration and it was sampling on the tails, and it ended up with a result that kind of skewed the results, versus somebody who sat down and said okay, I'm going to run 100,000 iterations. What kind of range of results do you get if you just do a few iterations versus 100,000 iterations or 100 iterations?

I guess what I'm trying to cover here is that you don't want to give a misrepresentation of the capabilities if it just happens to sample at the end of the tails and gives you a number that looks like it's 200 millirems of release versus if you did 100 realizations. That wouldn't be the real number that you'd get. Is that a problem or you don't foresee it to be one?

NUTT: Just in the number of sample sizes?

BULLEN: Yeah, sample sizes. I mean if I only did one realization and came up with a number versus I did 100 or 1,000, people not understanding how Monte Carlo operates--

NUTT: Sure.

BULLEN: --might look and say okay, I did one calculation and gosh, it's going to fail.

NUTT: I mean what we're talking about, I realize what you're saying, but part of the problem with these complicated things is when you start throwing the switch in Monte Carlo it gets very difficult to explain what's going on. But it is something we are going to address in this next phase of the package.

But part of the deal with the interactive--one thing I've been doing at the demonstrations is with the model, just letting it sample single realizations. So I'm hitting the button and letting it go, and it's going out and sampling. So I can get a realization out in that tail, but, you know, for the 100 versus 1,000 versus a million realizations, yeah, you're right, you're just going to go more into the tails. Hopefully eventually you can find the stable mean and--

BULLEN: Actually you just led into something that I wanted to ask about, was the stable mean. Because if you just did one iteration, you know, you could end up in the tails. But if you had a minimum that said okay, I've got to do 500 iterations on this type of calculation--not that you've locked out what they're doing--but you want to make sure that what you do focuses them toward reality or what--what the capabilities of the code might be as opposed to just being the extremes.

Now obviously when you unlock it the people are going to do exactly that. They're going to sample all the extremes and come up with the worst case. And so you want to have sort of a caveat that says if you do this, this is the worst case scenario as opposed to uncertainty analysis, and that's what people would do if you give them the capability to use this.

NUTT: Yeah, what we're planning on doing, where I said we're going to give them the ability to interactively select a few parameters, we want to give them a conditional probability. Okay, you pick these three parameters, here's your probability of getting that. You might end up with a high dose, but here's why. You picked something that's 10 to the minus 7. So --

BULLEN: I think--

NUTT: --want to give that information and present the result they come up with in terms of a likelihood of grabbing that number.

BULLEN: Okay. Thank you.

RUNNELLS: I think, Dr. Nutt, that Professor-- Runnells, Board--I think Dr. Nutt--Professor Bullen will agree that you passed your oral exam.

NUTT: Okay.

RUNNELLS: You didn't mess up. You addressed an issue that has been of great interest to me ever since I joined the Board a couple of years ago, and that is communication with the public. And I want to compliment and compliment the DOE on making this effort to communicate with the public. It has all kinds of pitfalls; we all know that.

When you try to simplify a very complicated system you may deceive people. But that in this case may be good. They may--folks who try to use this may ask such wild questions, come up with such wild answers, that it'll give you good information on what to address. So I have a very difficult time seeing a negative aspect of this.

I would urge you to try to, even at greater danger, simplify further. But I would absolutely support the continuation of this effort. The one thing that I would suggest is on one of your early slides the target audience was high school-something and above.

NUTT: High school graduates.

RUNNELLS: Yeah, let's make it the public, okay? I think there are lots of high school graduates who will not be able to handle this and there are lots of non-high school graduates who will absolutely be able to handle it. So let's direct it to the public--that's what its real purpose is.

But anyway, I think it's a great effort and more power to you.

NUTT: Thank you.

COHON: My question is a follow up directly to Don's. Can we have slide 9? Okay, I think the average member of the public would understand almost nothing in that slide. And--which is not your fault. I mean this is exactly the kind of result that the program has produced, and keeps producing, and for good reason. I mean there's a lot of information to be contained and captured in one diagram like this.

But I think we don't do--this is the big We, not you--but we don't do the public a service by presenting results in this form. And I also think that we sell the public short by believing there's no way to translate this into something that is accessible to the public.

Yet it's essential. This is it. This is the result. And I don't know if it's your job or not, but we need someone to figure out how to make this understandable to the public. You don't have to respond.

NUTT: --do with that. I won't disagree. Took me a while to figure out what these things are.

CRAIG: Yeah, boy, is that a tough question. Other questions from the Board? Staff?

In that case we have some extra time, and Jerry--wait, wait, wait, I haven't relinquished my time to you yet. You need the extra time.

SPEAKER: --if you can relinquish--

CRAIG: Well, I was going to have open session on the panel, but if you'd like to go to the public, that's fine with me.

SPEAKER: Let's give the public--

CRAIG: Go to the public.

COHON: My thanks to the speakers and to our wonderful and stern chairman, Paul Craig, for his generosity in yielding the time, the remaining 10 minutes in the session.

We have five speakers who have signed up, and I want to give them as much time as we can, until about noon or so. But that will mean I'll still have to monitor your time.

In the order that you signed up, we'll start with Jerry Szymanski. (Pause) Maybe we won't. Is Jerry in the room? We'll see if he rejoins us. Mr. McGowan, Tom McGowan.

I have this feeling that they figured we'd be right on time at 11:35, and that they'll be back in.

Is Sally Devlin here?

DEVLIN: --sir.

COHON: Ms. Devlin. Welcome back.

DEVLIN: Mr. Cohon, Dr. Cohon, thank you so much, and welcome again to Nevada. Thank everybody for coming, as always, and participating. And of course I have to have some fun, and where is Dr. Nutt? Where'd he go? There he is.

Mark, you did super. I hope you join Toastmasters. You did wonderfully. Again on this public relations thing--and I made a note, and that was I got Abe on six acronyms in a sentence, and the one I note on yours is RIP. RIP to me means rest in peace. So you need a glossary. And it must be in English. As I say, it really is kind of fun.

When there was one little thing on waste package failure, and radionuclides release rates--where are you? Mark, come up here so I can look at you. But I don't understand when you talk radionuclides release rates. What are they? I'm the public punching in my doubleclicks. What are they? What do you save the explanation for?

This TSPA-VA relation is supposed to be for the public. How are you helping the public understand what all the stuff is? I understand the Monte Carlo and the iterate and all the rest; I did my bit yesterday. But this is very important because just as Dr. Bullen, everybody, said, they--the public doesn't understand it. RIP is rest in peace, and you put that stuff in there it will rest the peace.

Now the other question I have to ask is where is this going, what does it cost to go, and so forth? Remember we have nothing in Pahrump. We have two computers, period, for the public. If you're lucky to get on it. We have nothing. Now how can the public get this information?

COHON: Did you understand the question about release?

NUTT: Yes.

COHON: Okay.

NUTT: I'll try.

DEVLIN: You got my RIP?

NUTT: Okay. Mark Nutt, Golder Associates. RIP stands originally for the repository integration program that was developed a while ago, so it's an acronym for a program. It just ironically has the same acronym as what you mentioned.

For radionuclide release, what I meant was by--in the eventual failure or degradation of waste packages, water gets into them, waste dissolves, how much gets out. That was our aspect, was try to come up with a way to communicate to yourself how much gets out, what's the importance of it getting out and how does it relate to the downstream dose.

DEVLIN: But again, what is my topic? Transportation.

NUTT: Sure.

DEVLIN: I don't want it to get out before it gets in. You got the picture--thank you.

COHON: Did you understand the answer though, Ms. Devlin, about release? Okay.

DEVLIN: Oh--sure I did. But you're hearing what I'm saying, and it is not--

COHON: Okay.

DEVLIN: The other thing I'd like to question, on the drip shield design you want 10,000 segments, cost $200,000 apiece, that's $3 billion. Now those are good numbers. What do they mean? Absolutely nothing. Where are they fabricated, how much do they cost to be fabricated, where do they--where are they built? How are they transported? Does this $3 billion--is the gentleman here?

SPEAKER: He's coming.

DEVLIN: Okay, let's get some real costs in here, because you know I'm going to bring this up in the next public comment. Who built them?

DOERING: Tom Doering with the M&O. The fabricator hasn't been decided yet. The cost includes total labor of fabrication. Shipment is not included in that cost because again the fabricator has not been awarded yet. And the point of closure right now is right around 2060, so we don't think we're going to award the contract for a while.

DEVLIN: 2060, good number; very, very, very nice number. Thank you very much. But you understand I'm the public. You say $3 billion, to me what is $3 billion? I say on the canisters, $50-60 billion. On transportation a trillion.

I mean, you know, there are no roads in Nevada, there are no railroads in Nevada. We're talking no purchase, no this, no that. You're talking a trillion dollars. The public's got to be made aware of this, and it's very scary.

And I thank you very much for that, because these are questions the public is going to ask you, Mark, and they're going to ask you, you know; so long time. And my feeling is I love Bo. I've been with you people for so many years, and I hope y'all keep your $100 million a year jobs and model and model and model at the door.

But the--thank you, thank you, Abe. But I can't understand one other thing, and that is--and I'll just end with this--how can you talk post-closure--you hear the marvelous word closure--when you don't know the basis for the natural analogs and the this and the that? Maybe my terminology for analog is different than your analog. To me an analog is Cigar Lake up in Canada, and that's depleted uranium in case and clay that's 100 trillion-billion years old.

What we've got here is a leaky faucet full of fractures, fissures and faults. And so I don't know--I want definition on this analog thing. But the worst thing is again getting back on the metals and the things you're using, carbon steel, Alloy 22, titanium 7, and that is there is not one thing in that entire 14 pounds of VA or EIS on this that mentions my bugs. And I am insulted because MCI has to be mentioned.

There must be something about microbes being tested. Livermore has proved microbes are in the rock, they're going to eat the rock. You better have some protection because the rock's going to fall down, it's going to disintegrate. And then you're going to have the bugs for the rocks, you're going to have the bugs eating the Alloy, that love nickel, you're going to have the rad-eating bugs; you're going to have bugs up your bugs. And I think there should be far more discussion on this.

Thank you.

COHON: Thank you, Ms. Devlin. Tom McGowan. You have someone who volunteered, I understand. Dr. Wong? You can stand anywhere you want.

SPEAKER: Just so you talk into a microphone.

MCGOWAN: I indicate the answer to Sally's questions are readily available. My understanding is they were worked out --those figures were worked out by constipated mathematician, he worked it out with a pencil. No, it wasn't Dr. Banbot (phonetic).

BULLEN: Check please.

MCGOWAN: Check please, right. Thank you. Security. My name is Tom McGowan. That's excellent, thank you.

You're hired. Las Vegas, Nevada.

In -- public comment I'll address the previously referenced alternative to underground storage. And I'll ask the chairman to enlist assistive services. Dr. Jeffrey Wong I understand has manual dexterity to manage the overheard viewgraphs. The instruction is on the bottom. It's not in code. It's rather understandable.

As Dr. Wong prepares to assist, I wish to say that notwithstanding variable sections to the contrary, I hold the chairman, the Board, the DOE, OCRWM, YMPO, all meeting attendant persons in the highest personal and professional respect, admiration and esteem, as uniquely qualified and dedicated proponents of their respective agencies and entities in service to the genuine best public interest.

And I appreciate your forbearance as receptive of the following presentation and proposal by an unlettered member of the local public. I should qualify that with one negative--leave something tending negative, which you might expect of me from time to time. And that is that I'm currently convinced that this is your best to date, and that's what more or less concerns me a little bit. I think you're capable of far greater things, and that's what I will begin to address here and now.

In -- and in premise the issue of high level nuclear waste was long since previously departed from the realm of responsiveness to manageable control by traditional means in terms of policy and process, and has entered a greater dimensional realm wherein it is solely responsive to address manageable control by a neo-policy and process paradigm comprised of voluntary reform-based attainment to a higher idealized standard of human spiritual quality effectiveness in terms of ethics, morality, reason, integrity, and above all, conscience; from which realm they will never again return. So we can forget about the past. We have a new millennium ahead of us, a new way of enhanced thinking, let's call it.

First viewgraph please, Dr. Wong. And thank you, sir. Let's first have upper tier. That neo-paradigm has a geometry which is neither pre-middle nor rectangular, but is spherical. And thereas omniparticipant, omni-interactive, omni-intercommunicative, interenhancive and interreinforcive. There ascertained to context as an optimum viable integer whose hold is greater than the sum of its parts and whose output efficiency is greater than a unity, hence what you obtain is a virtual human laser, notwithstanding the particulars in dimensional scale. It'll work as well at any size and scope.

Quality and integrity are interchangeable and intercoincident, dual aspects of one and the same integer whose ensured effectiveness is expressly contingent upon the total quality, integrity of the integer; inclusive of each and all of its component elements--hopefully like you. And there's a major difference between total quality and total quality management, since while TQM extends from the -- apex in descending order to middle management, as you see indicated. But not beyond the subtending broad based rostrum of rank and file.

Total quality is permeated and ubiquitous throughout the entire infrastructure, which slowly thereas and thereby obtains as comprehensively integralized, ergo enhanced, as attained to optimum integral viability or OIV. Within -- both flexibility and resiliency impervious to any law externally imposed stimuli.

In that enhanced state -- equation E equal MC squared can be juxtaposed and expressed as QVE equals QVMC squared, wherein and where by the quality and volume of the human energy yield is equal to the quality and volume of the coherently integralized human mass times the speed of light squared. And thereas generative of a historic non-precedent volume of utmost attainable quality, productivity at a fraction of the cost incurred by persistence in the deemed traditional policy and process paradigms and concombinant geometric configurations.

It occurs to be the universe works something like that. I don't know who designed it in particular, -- who we always refer to as a supreme being, or supreme infinite knowledge. But it wasn't one of us--that's obvious. We wouldn't have been done with it yet.

That enhanced state is expendable--expandable on the national and international scale to comprise a crash program of universally dedicated context, spare purpose, and then 10. Prerequisite essential and categorically imperative to the assured effect address and remediation of high level nuclear waste, completely and permanently at a substantial profit in terms of both tangible and intangible omniparticipation based reciprocal benefits.

May we have the second viewgraph please, Dr. Wong? Thank you. Want me to give you three minutes? What do you do here exactly? Thank you. The lower--the -- depicts the geometric acceleration and expansion of the integer over time, obtained through context as exponential arc tending toward infinity. I believe in the upper one is the--excuse me--the linear progression of the total quality enhanced integer configured as concentric flaring horns evolving, expanding and accelerating in continuum while available range of energy -- options with no constraints or impedence impacted upon the direction or rate of acceleration. I think I got--had that backwards for you, but it comes out the same way regardless.

The neo-paradigm abhors underground storage and is comprised of a composite of surface based high level nuclear waste storage and robust canisters at decentralized generator sites, pending one way transport to not more than 500 miles distant regional federal sites, pursuant to 4-9s (phonetic) drastic reduction, transmutation and separation of the most egregious and long-lived radionuclides via limited range of optimum accelerated driven transportation technology systems, san (phonetic) inclusive of an ultimate save--molten salt reactor in a self-amortizing expanding national and international program ensuing over a minimum term of 50 years and extending to 100 years or more.

Highly toxic residual byproduct will be in vitrified and -- pending substantial stabilization, while shorter-lived radionuclides will stabilize within 200 to 300 years under closely monitored security and canister integrity maintenance and preservation. Entire process will be subject to strict military discipline, responsible oversight, stewardship management and control, initial funding of approximately $250 million for limited test and survey and refinement operations; will expand to full scale operations under the electrical power generated, profits plow back, to approximately $250 to $500 billion nationally and worldwide.

That profits all applicable sources including tangential business development, employment opportunities, amplifier affects will accrue to approximately 4 trillion over the enduring term, approximately 50 to 100 years; and equating to a long term cost ranging from nominal to nil to de minimis--which means it's free. All you've got to do is apply yourself.

The transformation of egregiously impactive liability into a valuable asset will surmount all -- barriers and will invoke a waiting list of ready, willing and able qualified applicants pursuant to participation on an ensured reciprocal benefits, recipients basis.

Additional benefits of neo-policy and process paradigm include both nuclear and conventional arms reduction, nuclear non-proliferation, global solidarity preclusive of organized terrorism, increased international trade and mutual cooperation and understanding, and commensurate peace progress and coexistence in perpetuity.

And some reminders, problems are opportunities, not use of a problem, the problem is solved. The principal guidelines is the spirit of genuine community based on the realization that none of us is smarter than all of us combined, and as Bucky Fuller said, unity is plural. I'm quite sure it is.

In conclusion e pluribus unum, (inaudible) self-mutual ennoblement shall be our legacy instead of failure and infamy. I'll adjust the third viewgraph in delineation of nuclear waste dedicated secular priesthood in the next public comment segment, and I wish to thank the chairman and Dr. Wong and members of the Board.

I have one question. There was a speaker today called Jean Cline on fluid inclusions. I don't see a presentation of hers on the table. Is there one available of her report?

SPEAKER: Apparently not.

MCGOWAN: Apparently not? But she's on the agenda.

SPEAKER: She'll be speaking.

MCGOWAN: Oh, but she doesn't have a copy for the public? Oh, I see. Well when can we get one of those?

COHON: Dr. Cline, will you be making something available in writing, or could you?

CLINE: I had not anticipated that, but I could perhaps put--

COHON: Okay.

CLINE: --something together.

SPEAKER: Your work is very important.

CLINE: Thank you.

COHON: Good.

SPEAKER: We'll get a copy.

COHON: Well, talk to Dr. Cline, okay. You will hear her today.

MCGOWAN: Okay.

COHON: There won't be anything in writing today.

MCGOWAN: --have that on the record that you did not bring a copy of--

COHON: I think it is. Thank you Mr. McGowan. We're going to have to hook you up with Dr. Nutt so we can get the simplified version. Check with us.

Brian Marshall from the U. S. Geological Survey.

MARSHALL: Brian Marshall, USGS. I just wanted to inform the full Board that there are ongoing studies being performed by the USGS that relate to seepage, that were inadvertently left out from Bo Bodvarsson's presentation this morning. We have data on secondary minerals which indicate that factors other than the capillary barrier may control seepage.

As you may recall from Bo's presentation this morning, he emphasized the capillary barrier and seepage threshold in his presentation. We have a record of past seepage at Yucca Mountain extending millions of years into the past. Seepage of water has been recorded in deposits of secondary calcite and opal within open cavities and fractures.

To the extent these deposits are an analog for seepage, they do not support the importance of a seepage threshold for three reasons, and I will list these three reasons in order from least significant to most significant.

So beginning with number 3, the surroundings of the cavities are heterogeneous and include many fractures and complex shapes. Number 2, capillary barrier theory states that there should be a correlation of seepage with cavity size. However, there is no correlation between the amount of calcite and the size of the cavity in which it occurs.

And finally, the most important or most easily understood reason is that adjacent cavities with similar characteristics often display very different amounts of calcite, suggesting that seepage is not controlled primarily by the capillary barrier.

COHON: Before you leave the mike, I thought I heard Bo say that the depositions you're talking about, if they were deposited continuously, would suggest a very--I don't want to use the wrong words--slow seepage rate or very -- yeah, you know what I mean--

MARSHALL: Yes.

COHON: So do you disagree with that?

MARSHALL: No, I do not, but I was not--I didn't want to emphasize the amounts of water that can be interpreted based on the seepage records. I merely wanted to emphasize some of the characteristics of the deposits which bear on the presence or absence of a seepage threshold.

COHON: So this goes more to the way in which seepage happened, influences on seepage rather than the amount.

MARSHALL: Right. Stated another way, other factors which I don't believe are fully incorporated into the UZ site scale model include things such as flow focusing, film flow, et cetera. I can't think of the other one at the moment.

COHON: Okay. Well, thank you very much for that contribution. We appreciate it.

Atef Elzeflawy from Agua Viva.

ELZEFLAWY: Oh, -- just fine.

COHON: That's good. I did something right this morning.

ELZEFLAWY: If you had any problem with my name, just call me Bob. I learned that 30 years ago when I came to the United States, became a citizen. One of the things I like the most about reading is to read autobiographies of people, and also autobiography of some--some workers and so on.

So I have a good idea about some of your background, the Board members, and some of the other people who work here. But I need to give you about probably 10 seconds of my background. Born in Egypt, finished my first Ph.D degree there, University of Alexandria, and I came here in 1970, went to Gainesville and got another Ph.D in soil science and hydrogeology; and went to University of Illinois as assistant professor, met Chester Cease (phonetic), who was the chairman, who got me in trouble in this program.

He said "Well, you know a lot of things about soil, and let's go to Hanford." He was a member of ACRS. I don't know if some of you know the ACRS of the NRC at the time or not. These are the board like you guys are elected by good people, the best in the country, to look at the safety of the nuclear power plants.

Chester Cease got me involved in that. We went to Hanford and we discovered that their nuclear waste, quote, unquote, tanks are leaking. And that's how I got involved into this program. And then in 1980 I moved from University of Illinois, came here to work for the Desert Research Institute and the Department of Energy gave me a nice free boot--I have them since then, still on my feet--the only free gift I got from any agency in the United States or any person.

And last Christmas my brother came, that I haven't seen him for about 25 years, came and visit me and he stopped by for two weeks and when he left he gave me keys. And I got a brand new Volvo for free. And so thank you for the free time that I have here. I don't get any money anymore. I've got to take off my hat in respect to your program and so on.

But I like to say couple things, because I've got to go. I was planning to have some thoughts this afternoon and maybe written a piece of paper or so. But in 1980--I think in '81 before the Act was passed I was visiting Washington, D.C. and visiting Congress of the United States. And they were debating in some committees the Nuclear Waste Policy Act.

The Nuclear Waste Policy Act was so nice to hear because it reminds me with my daughters I pledge allegiance to the flag, da-da-da-da-da, justice for all. You live in the United States and you know sometimes that justice is not for all. Sometimes justice is for some, and that's the sad part of what I see today. Here it is 20 years later or almost 19 later about the Act.

The Act back then was fair enough to say okay, we will have--if the first repository will be in the west, the second will be in the east. Well back then was fair. About a year later I got involved and I got to be somebody who commended my name to work on the unsaturated zone with NRC as a consultant.

And I remember a fellow assigned here again, Bill Dudley; we were talking about drilling for the unsaturated zone. And the USGS was going to be drilling and after about 15 minutes of aggravating the speaker he said "We're going to be drilling with drilling mud." I said "You don't drill with drilling mud to assess the unsaturated zone." Unsaturated zone doesn't have a whole lot of water, so you don't want to add a lot of water to assess what's in the water--what's in the rock before.

So--and then I left there, worked for the NRC for about three years, and I think in 10 CRF 60 was a fair document. At the time the EPA rules were fair document. The Department of Energy program in general was going into a fair situation until we got Senator Johnson, who gave us this Nevada Bill.

The whole thing behind it was that the federal government did not have the money to afford to take care of three repositories, one in Texas, one in Hanford and one in Yucca Mountain. So the Congress with the wisdom declared, okay, Yucca Mountain only. Well Nevada didn't like that, and I know that Yucca Mountain might not really be good site in terms of at least the hydrogeology, since I know a little bit about hydrogeology and unsaturated zone and so on.

And then the Congress after that enacted or added the Nuclear Waste Transportation Research--I mean Technical Review Board. I said ho, this is good because we're going to get some fair minded people to give the DOE some direction, because their train is heading for MPL. Anybody know what's MPL here in this Board? It's called Mars Polar Lander, that we heard about a couple weeks ago.

The train at the time, technically speaking, because of 10 CFR 60; I knew that deciding guidelines and da-da-da-da-da. It's not going to be--in fairness the site is not going to be--or is not going to be passing through in terms of the guidelines as a good site from the geology point of view.

Now I got to know the Board members, I attended their meeting, I still read everything you guys publish. I still read everything the DOE published, sometimes in details and sometimes not in detail. But here's the situation: after all those years now the Department of Energy is saying that the engineers will make a waste package last for 1,000 years. Back in the NRC we were laughing at them in 1983 and '85 that they were talking about waste package that's going to last for 300 years.

So I think somehow, somewhere this Board needs to stand up and say something with regard to this Yucca Mountain thing. If you have a problem with that waste, maybe you need to send it to Egypt where they have three pyramids lasted for 5,000 years, where I came from. You can see it there.

But I don't think coming here--and I can argue technical things until I kill you, like Martin Luther did back with the Catholic Church, and I'll talk to you in geochemistry and hydrology and engineering and all that, but that's not my point here.

My point here is that I like to see the Board stop from taking that train to become MPL and assess the situation technically, fair minded, using all your good brains. It's hard to talk to people when you want to really talk to their brains.

And so I think from what I see during the last 10 years, almost 10 years, of the nuclear board, that at least I'm glad to see that what I said in 1982, one millimeter a year in the unsaturated zone, that wasn't one millimeter. The DOE said one millimeter, one millimeter. And now we know that it's about 15 or 16. The USGS didn't listen to the simple analysis, and they spend $20, $30 million a year, and here it 15, 16 years later they came back a full circle, and say Oh, that's about 20 or 15 millimeter. And I saw it in the Board meeting sometime about two years ago or so.

Somehow, somewhere I got the privilege to see Ward Valley. By a phone call I got from the Secretary of

Interior and Barbara Boxer and Diane Feinstein of California. They asked me what do you think about this program, to prove that Ward Valley would be a low level site?

I said after I looked at all this two-inch document I'll tell you this, you can do all that and 10 years of research from now, and after you collect all these data, it's not going to be very conclusive either to a scientist or either to the public that this site is quote, unquote, safe. So the Secretary of Energy and the two senators sank the site.

Somehow, somewhere you've got to address--I've seen remember Pat Domenico passing through and all the others, and couple other professors that went through the Board. It's an honor to be a member of this Board. I know what that honor is. I already had one in 1976 from the Transportation Research Board. But what I'm saying is again, to summarize this--this is probably the first time and the last time I will speak to you guys--but you need to stop and look at the program and see what the DOE is doing for the program.

All these technical details might not happen. One problem with the toss-back, they used to call it, the assessment on the performance assessment, all those computer things, all that is going to give you some data and all of a sudden you are not going to see the faults. And then after you get the waste in and you put it, 50 years later, oh, we were stupid back then. We didn't really see that.

So simplicity is--one of you guys said something about simplicity is the name of the game. And just yesterday, to give you an example to finish up with, the Department of Energy and Yucca Mountain, putting--not Yucca Mountain but in Nevada Test Site--spend about $150 million on a model, computer model, mud flow and flow paths and all that, to come up with one single flow path with regard to the water and where the tritium is going.

You know what? My--not mine, mine and some other guys 17 years ago met, was exact -- and you put them one next to the other, what did we do for $120 million aside from what did we learn from spending $120 million? You know we learned nothing except we gave people jobs for five years, to spend $120 million.

What I like to see, maybe a recommendation from the Board that hey, now we--the country is rich, and we gave the State of Nevada Yucca Mountain only because of the money. How about going back and opening that law and say well, let us see what Hanford is going to look like, let us see what Basalt is going to look like in Texas. So somehow, somewhere your reports to the Congress are so beautiful and so nice to read, but they don't highlight the problems right up front. Watch out. You're heading for MPL.

So thank you for your time and I appreciate your effort. I'll still stay with you in the back seat, but somehow, somewhere the Board needs to go into maybe technical session or maybe a closed session--whatever it is--to address that point. So thanks.

COHON: Thank you, Dr. Elzeflawy. Dr. Szymanski will be speaking in this evening's public comment period, so that concludes the public comment period for today--for this morning, I should say, and concludes our session for this morning.

We'll now break for lunch and reconvene at 1:00. Thank you very much.

(Whereupon a lunch recess was taken.)

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

COHON: Please take your seats. Thank you. Our afternoon session is devoted to an update on the project's scientific programs. Chairing this session will be Board member Don Runnells. Don?

RUNNELLS: Welcome to the afternoon session. This is the one we've all been waiting for. I personally can hardly contain my excitement. We're going to hear about the update of the science, and we're going to hear about analogs, things that the Board has great interest in and we've often asked about. And we're looking forward to this afternoon's presentations.

Let's get started, not waste any more time with my chatter. Our first speaker is Mark Peters. Mark has a Ph.D in geological sciences from the University of

Chicago. Sorry I reverted back to Colorado--Ph.D in physical sciences from University of Chicago--and he's responsible for the technical integration science construction and design organizations. He's going to give us an update, an overview of the scientific programs that are ongoing. Mark?

PETERS: Thank you very much. It's great to be back talking to the Board. I think you've gotten used to--I usually come in armed with quite a stack of paper. This is no different. There is a lot of material. Attempt is to try to cover the entire testing program and give you an overview of where we're at with most of our testing.

You've heard a lot about some stuff that we're doing in the ESF and cross drift related seepage. There is actually some duplication, so a couple my slides Bo showed this morning, so that will help with the time. So I'll probably go over those relatively quickly and spend more time on the things that you haven't seen as of yet in this meeting.

In terms of overview I'm going to talk about ESF. I've tied, for the purposes of the overview, all of the testing programs and the different factors of the repository safety strategy. You heard an overview on the RSS this morning, principal factors and non-principal factors. The overview slide simply has those factors and then the testing program that feeds those factors underneath it.

So in terms of the unsaturated zone, including seepage, talk a little bit briefly about Alcove 1, some work that we're doing in the PTN and fault zone, a small fault zone within Alcove 4, briefly talk about the ESF niches that Bo mentioned this morning. Again those niches are the middle non-lithophysal unit in the ESF, which makes up only the upper part of the potential repository horizon.

Get into the cross drift, give you a detailed update on where we're at with the construction and drilling, and the testing in there. It'll compliment somewhat what Bo had already talked about this morning.

A little bit more on what we're observing in the bulkhead studies in the cross drift, some on the fracture mineral studies, and the Chlorine 36 studies in the ECRB and the cross drift; a little update on Chlorine 36 validation fluid inclusions, and then what we're doing in the area of overall stratigraphy.

Switching gears to coupled processes, an update on the drift scale test, temperature, evolution, what the moisture's doing, and looking at some of the comparisons to predictions. Over to the saturated zone, very briefly discuss how we're integrating Nye County results into the

DOE program; refer mainly to the poster sessions sitting over on the side wall, which everybody's had an opportunity to hopefully look at.

And then a couple bullets on the flow and transport model improvements we've made for the SR versus what we had in VA. And then talk some about primarily the pilot scale testing at the Atlas Facility in north Las Vegas, and then some discussion, a broad overview of where we're at with waste package materials testing. Not a lot of detail there. If we want to talk more about the detail, I'll take some and Dave Stahl I know is in the audience to help with some of the really gory details if we get into that. Next slide please.

Just to refresh your memory, you've seen a lot of this this morning. We're going to start with the ESF studies. Here's a map view of the ESF, the U-shaped tunnel with the potential repository block and the cross drift running across. We'll talk about Alcove 1 here in the Tiva Canyon, Alcove 4 in the lower part of the non-welded, Paintbrush non-welded PTn; again Alcove 5 where we're doing our drift scale test, and then ESF niches. Next slide please.

More detail of the layout of the cross drift. I am going to spend quite a bit of time on the cross drift. This is just a variation on a theme of what the map that was shown in Bo's presentation this morning. In the cross drift what was referred to as the cross drift tracer test, I believe in that presentation, is actually the crossover alcove. That's the drift to drift test; from Alcove 8 the crossover alcove to niche 3 and the ESF underneath. So that's where we're getting at the scaling effects. That's about 18 to 20 meters below--of separation.

Niche 5 where we're doing--process of constructing and doing some drilling for our seepage tests. That's in the lower lithophysal, the lower lithophysal in the cross drift, pick up right around approximately in here. The lower lith is exposed from this part of the cross drift basically all the way close to the fault; pretty close to the fault.

And then again we have bulkheads installed. One bulkhead is about 1750 meters from the start of the cross drift. The other one is just before the Solitario Canyon fault here about 2500 meters from the opening. And those have been closed since June, and we'll talk a little bit about what we observed there. And we just had an entry last week and I know there's been some discussion about what we saw there, and Bo alluded to that this morning. Next slide please.

Starting with Alcove 1, this is just again an update. Bo did talk about that quite a bit this morning and how he's using that in his model. Phase 1, you've seen this before, but reminder--we're introducing water at the surface and then monitoring how much water actually seeps into the opening.

In Phase 1, which was really finished up about a year go, we applied 60,000 gallons of water. It took two months, approximately two months to get water to seep into the alcove after about half of that water was applied, and then since that time we ended up seeing about 10 percent of the water enter the opening. Next slide please.

Phase 2 was started about a year ago now, little under a year ago, and the statistics are contained in the bullets. We put a lot more water in phase 2. We are varying the application rates at the surface, and we saw seepage in the alcove much faster. Not surprising given that the fractures were probably still relatively saturated from the phase 1 tests that we saw break through earlier.

We're seeing about the same amount of water enter the opening, but we're also varying the concentration of the lithium bromide tracer. This is just an illustration of alcove 1, again the infiltration plot is about 30 meters from the surface to the crown of alcove 1, and the infiltration plot at the surface is larger than the plan of the alcove itself.

This gets at varying the concentration of the bromide. We are varying the concentration of lithium bromide in the water, and this is a series of predictions as well as observations. The red squares are actually bromide concentration as a function of time. The three curves are--the green curve is if we would stop injecting the tracer at the surface.

As of a couple weeks ago we actually have continued, and we're planning on currently thinking about stopping the tracer, end of this month; and then we'll continue to monitor the test through the year to gather enough information for--to be used in the UZ flow and transport model for SR. This is just showing this simple 1D prediction actually does a pretty good job of predicting the breakthrough of bromide and the change of concentration with time.

Alcove 4, if you remember Alcove 4 sits at the base in the ESF, at the bottom of the Paintbrush non-welded. And in Alcove 4 we have a test in the back of the alcove. We've drilled a slot, an opening in the lower part of the block, and we're actually interested in testing what is a small normal fault in the PTn at that location.

So what we're doing is we're injecting water in some of these high holes and then looking for breakthrough of the water along the fault and into the opening. Preliminary data, but what we've seen is not actually dripping into the opening but a damp spot.

So early on when we started infiltrating along the fault there was a lot of wetting of the matrix. But we have seen breakthrough in the sense that it's damp now at the fault, but again we haven't put enough water in to get any dripping.

We are able to get some information on flow velocity along the fault, and all that's being--this is very preliminary at this point so we don't want to say a whole lot more than that. But it will be incorporated into our understanding of how the fault's acting in the PTn in our models.

In the ESF niches, again these are the seepage niches that Bo spent a lot of time on this morning. In the middle non-lithophysal unit niches the work that Berkeley's done on seepage is really in niches 2, 3 and 4. Niche 2 has been complete for quite a while now.

Niche 3, although there's been a lot of comments--and this was again alluded to this morning, from the peer review panels as well as yourself and other oversight bodies --about the importance of doing seepage tests at what would be considered ambient humidities.

So at niche 3 there was a lot of attempt to do the seepage tests under relatively high humidity conditions to evaluate how the wetting history influences the seepage, to really get at what we expect during--after a cool down, during the majority of performance period.

And then also there's been a lot of comments on having--we should understand better the details of the fracture distribution, so we have in niche 4, I've got an example of a detailed fracture map that's been done by the Berkeley PIs.

Niche 3, the testing is basically complete for niche 3 itself. Again niche 3 is going to be used in the crossover alcove test as well. And niche 4 testing, air-K is ongoing and is actually complete and they're in the process of getting ready to either--start the seepage phase.

Plot of the relative humidity and temperature inside niche 3 with the test durations at the top, just to show that we did make an attempt here to actually conduct these tests under relatively relative humidity conditions; and just shows the different tests that we did in terms of liquid release in the niche.

Example of a fracture map that we've done for the ceiling of niche 4. These upper boreholes are where the liquid release tests were conducted, so this would be the opening, this is the entrance to the niche, here's the niche itself; so we've done extensive fracture mapping of the ceiling to correlate with the air-K and what we see in terms of liquid release.

Switching gears now to the cross drift, still focusing on the UZ flow and primarily seepage. The crossover alcove, the cross drift tracer test--however you want to call it--sits right there as the cross drift goes over the top of the ESF. This is more of a field update on where we're at.

We originally were going to excavate the alcove with drill and blast techniques, but we actually found as we were going into the upper lithophysal there--and it was actually going pretty slow--so we made a decision to terminate that and we're now using an Alpine miner to excavate that opening.

So we moved away from that and actually moved to niche 5, and now we're back, so the Alpine's actually underground today working. It's been excavating on alcove 8 now since last week.

We finished drilling the boreholes that are going to go up from niche 3, and now like I mentioned, we're excavating with the Alpine and the testing will start in the spring time frame in alcove 8, the alcove 8 niche 3 test.

Niche 5, about halfway down the cross drift, about 1600 meters down the cross drift, again looking at seepage processes, but this time in the lower lithophysal which we have not tested in the underground. We completed drilling--it was mentioned this morning we're not only looking at seepage but the effects of excavation on air permeability, et cetera.

We drilled some boreholes, pre-excavation, to do some air permeability. Those have been drilled and we've actually done the testing. There was a part of that shown this morning. That's duplicated here. I'll probably skim over that relatively quickly.

We're in niche 5; we've excavated the first phase of niche 5, and that will become clear when I show the diagram; and now we're drilling the Phase I boreholes, and the testing again is--we're pushing real hard to have as much information as we can for the site recommendation.

Schematic of alcove 8, niche 3 test, again about 20 meters of distance here. Upper lithophysal unit here, and then we transition into the middle non-lithophysal as you get down closer to niche 3. But you again have these up boreholes and the down boreholes and the infiltration part will be in the back end of alcove 8.

So we're excavating right now and we're probably right about here in terms of excavation progress; and we should done with that sometime in March on the current schedule.

Schematic of niche 5--when I talked about Phase I excavation, if you remember the niches from the ESF, they were much shorter. The actually niche--test niche, if you want to make a parallel to the ESF--is back here. We excavated an access drift. That's complete; we finished that just before--or just after Christmas holiday.

And so what we're drilling right now is these pre-niche excavation boreholes, so we'll drill those holes, do air permeability testing, and then come in and excavate this Phase II niche, and then do the actual liquid release out of some of these same boreholes.

Terms of moisture monitoring work, I've also tied in some of the air-K work that was discussed this morning, and the bulkhead studies. Just to summarize what was discussed this morning, we have done some air permeability--Lawrence Berkeley has done some air permeability in the lower lith and some of those boreholes--excuse me, I'm jumping ahead on myself.

Let's talk about water potential first. We've discussed in the past when the USGS has installed a series of instruments in the cross drift and they were showing relatively wet water potentials and uniform, and one of the questions that we had to ask ourselves is was that--how much of that was due to the instruments that were being used.

So we went in and installed in some of the holes in the cross drift behind the bulkhead thermocouple psychrometers to compare to what we were getting from the USGS heat dissipation probes. And we're actually finding that they're giving us a very similar answer, which is a positive thing; so there's not some bias in terms of instrumentation.

Second big point, and this was discussed t his morning, is the preliminary air-K in the lower lith suggests that we may be an order of magnitude or a little more more permeable than the middle non-, based on limited testing and two boreholes in the lower lith and a lot of testing in the middle non-. But we're continuing to do the air-K not only in the niche but the systematic air-K that was discussed this morning to better nail that down.

This gets back to the water potential issues, or data. You've seen this before. It's not terribly up to date but it gets the point. This is water potential in bars, so as we go in this direction we're getting drier, so this is wetter. The data to notice at first glance is this data across the bottom. It's a time series as a function of station within the cross drift. You can see that we had relatively high "wet" and uniform water potentials.

Then with time we started getting a spiky pattern. A lot of that's due to the drying, due to the ongoing ventilation in the tunnel.

Sub-plot, again a function of time, water potential on the y-axis, dry in this direction. Of the two different instruments that we're using to measure water potential in the tunnel. The USGS heat dissipation probes were installed wet, so there's a very wet number and it takes a while to equilibrate.

The psychrometers were installed dry and they also have to equilibrate, but you can see that they're converging on a very similar answer in terms of water potential. This is just an example of the kind of data that we're getting, but that's a very important measurement in terms of water potentials used for the UZ flow model.

This was shown this morning, so I won't dwell on it, but this gets at the preliminary air permeability measurements in the lower lith, shown on the top with the geometric mean, as compared to what we're seeing in the middle non-lith based on measurements in the ESF niches.

Bulkheads, again we have two bulkheads in the cross drift, one about halfway down and one just before the fault zone. We instrument so it isolates basically half of the lower lith is exposed in the cross drift, the lower non-lith, and then the fault. And then you run into the TBM trailing gear, for those who have been down there.

We've instrumented--we had a lot of instruments in place before we installed the bulkheads, and we're basically measuring the rewetting and continue to monitor water potential behind the bulkheads. We are entering their periodically. We had an entry in September and we just went in, what, a couple Thursdays ago.

We're seeing continuing of the rewetting, no terrible surprise. The bulkheads are actually sealing up pretty well. And then we obviously don't ventilate during those times. And we're also seeing no apparent evidence of seepage. Saw some interesting things in the last entrance, but it appears to be condensation phenomena and not dripping from the rock; and that was again discussed briefly this morning.

Just an example of what we're seeing on some of the probes from a rewetting perspective. This is a next of heat dissipation probes at different depths, anywhere from 30 centimeters to 200 centimeters into the rock--two meters into the rock. And it shows--again this is water potential, so we're wetting in this direction, and this is as a function of time.

The bulkheads were closed right there, so at shallow depths we're seeing an end to the drying phenomena and a rewetting; whereas intermediate depths, we're getting a leveling off. Deeper in the rock we're still seeing a slight drying, but we expect all this to start turning to rewetting here very shortly.

Just in bullets, makes some of the points that I've already made. We are going in and doing some neutron logging, active neutron logging when we go in for the entries. And it indicates the bulkheads have stopped the dryout and that we're wetting at shallow depths.

We're seeing that the air temperature is actually higher than the rock temperature, and that may influence some of the additional dryout; and we are seeing some variability in rock temperatures. And that spiky pattern that was shown in the water potential diagram as a function of construction station may very well have something to do with evaporation along fractures. Some of the units have longer through growing fractures. And then we're not getting apparent evaporation in the matrix adjacent to those fractures.

Estimates of water potential between the two bulkheads are in the minus half to minus one and a half bar range, and if you go beyond the inner bulkhead towards the fault zone they're in a very similar range.

Over to looking at the fracture minerals, you know, we've done--USGS has done a lot of work looking at fracture minerals to get a long term percolation flux, concentrating on the ESF. There's a program now in the cross drift to do similar work.

One of the exciting things that's happened is, if you remember, they were doing bulk techniques. They were taking small samples, they could, and analyzing using standard techniques, concentration techniques and then using standard mass spectrometry.

They've--cooperative effort with Stanford, they're now using an ion probe which can sample a much smaller volume, and trying to get traverses across grains. And they've done some preliminary work there, and across to opal grains that are on the outer--coating the outer part of the fracture. And they're showing some very interesting data in terms of those traverses, but they're getting very good resolution at the scale of tens of microns.

The encouraging thing is that the data are consistent with what we're getting--we were getting conventionally. The deposition rates, they're consistent with more of a continuous deposition model that Zell Peterman and his co-workers have had for, what, two, three years now. And also it's consistent with deposition rates on the order of millimeters per million years; so very slow deposition, but appears to be consistent with continuous deposition.

Another way we're addressing percolation flux, and flux in the repository horizon, is continuing our Chlorine 36, Chloride studies in the cross drift. This is distinct from the Chlorine 36 validation, which I'll get to in a minute. This is the work going on at Los Alamos, June Fabryka Martin--you're familiar with her.

There was a presentation that I believe Paul gave at the Beatty meeting on this in detail. Terms of the--in the way of an update, we have done--we did see some bomb-pulse levels in some of the locations within the cross drift associated with faults.

And we've done some replicate samples now, and in fact taken separate samples from the same general area; and again--and we've replicated those bomb-pulse levels. But we've gone in and done a significant amount of additional systematic sampling.

Remember the systematic sampling in the ESF; all of our samples that showed bomb-pulse levels were featured-based, meaning we went and saw a feature like a fracture set or a fault and went for it. The systematic samples in the cross drift still are falling within the range of background. That's in the way of an update.

Still on Chlorine 36 but not a Chlorine 36 validation, we've also had a program--remember we've seen several locations in the ESF primarily associated with faults, where we saw bomb-pulse levels.

So the DOE has a program where we've gone into two of those locations in the ESF, Sundance Fault and the Drillhole Wash structure, and we've drilled some boreholes. And USGS, Lawrence Livermore, working with Los Alamos, are trying to validate the occurrence of that bomb-pulse Chlorine 36.

We're also doing U series analyses, tritium analyses and I believe also technetium 99 analyses to try to get an integrated set to tell us what we're really seeing in terms of bomb-pulse and what it means for flow.

Preliminarily the data we've seen, disequilibrium in the U series from the Sundance Fault, which indicates that long term water/rock interaction, this is similar to some of the other U series work that's been done in the ESF. We've looked at 11 samples from the Sundance Fault for tritium and found no tritium anomalies.

But can't say a whole lot about how it all fits together probably for a couple weeks anyway, until we get the Chlorine 36 analyses from some of those same samples. So still a work in progress. We should be able to say more as time passes here in the next three to four months.

Fluid inclusions, I will not spend hardly any time on this because you're going to hear a lot of fluid inclusions in a couple presentations from Jean and Bob. We are involved--the DOE is involved in a cooperative study with UNLV and the State to evaluate the paleohydrology of Yucca Mountain and what the fluid inclusions are telling us.

For the DOE part, the USGS has some new fluid inclusion equipment installed, and we've got 50 samples that they're going to look at in great detail. Nothing in the way of hard conclusions as of yet, but the interactions are healthy and there's a lot of good interaction going on in that study.

Stratigraphy--you know, our mapping of the underground and our mapping at the surface has really come to a close, but we're in the process now of really thinking about how we can document all that information and validate it and use it technically in a QA arena.

So we're working extensively on what we--what the USGS terms stratigraphic workbooks, and that's where we're basically documenting, and again validating and integrating, all with the stratigraphic data from the surface based boreholes. And it's being used primarily as the documentation for the geologic framework model for the integrated site model.

It's confirming our contact picks, it's giving us some idea of the resolution and the acceptable window for al the contacts. It's doing a data verification function for the contact picks, and basically you have a workbook for each borehole. And it's providing us an integrated, again, QA documented database for use in the SR when it comes to lithostratigraphy.

Okay, moving away from ambient UZ flow, seepage, now over to coupled processes, the drift scale test--you're all familiar with the drift scale test. It's conduced in alcove 5, and that's where we're evaluating the coupled processes at the field scale. The test is in the middle non-lithophysal unit.

It was discussed briefly this morning that there are plans to conduct a smaller test, but nonetheless a thermal test, in the lower lithophysal; and that's again in planning stage for--and current plan will be fielded next year, next fiscal year.

In the way of an update on the temperature, we're still running at the same power, 80 percent on the canister heaters, 100 percent on the wing heaters that we've been running with since the start of the test. We've been running--it was two years early December, so pushing 26 months here. The plan is to continue to heat the rock for the four years as planned.

We're targeting 200 C at the drift wall, and we're getting there, right around 190 Celsius. And as we approach that we will turn back the heat to sort of ramp up to that goal and maintain that for the remainder of the four years.

This is just--you've seen plots like this before. This is two holes drilled within the heated drift, horizontal holes drilled above the plane of the wing heaters. This is the center line of the heated drift, this is a time series for two of those boreholes. And remember that the wing heaters are segmented. The outer wing heaters are higher power than the inner wing heaters, so that's why you get this hump profile.

We did see some flattening, some conductive type effects at local boiling, 96 C, and the rocks continued to heat. You can see in the vicinity of the wing heaters we're well up--we're approaching 240 C in some cases. This is just a time series; this is as of day 700, so this is back in the fall, in that time frame.

Terms of measurements versus simulations, this is just measurements for one of the--for a series of boreholes after 21 months of heating. So this isn't a time series; this is at 21 months of heating, one array of boreholes. Remember the arrays of boreholes in the heated drift, some are horizontal, there are some down holes and there's also some up holes.

And then on the right is a dual permeability simulation prediction for what we thought we'd see at that same time, and broadly speaking we're doing well with temperature, terms of predicting temperature.

Now what about moisture? This is similar to plots that you've seen. There was a detailed presentation at the Beatty meeting on the drift scale test. This is electrical resistivity tomography results, and that's where we're looking at moisture distribution as a function of time.

This is a tomograph for back in the September frame, and what you're comparing here in colors is the saturation at the time it was measured in September versus what we measured in the baseline. So you're looking at a difference.

So red areas tend to be areas where we're seeing drying, whereas the more blue areas tend to be areas that have either maintained their saturation or actually wetting.

So we're getting, as could be expected, drying around the heated drift, but we are seeing what appear to be wetting underneath the drift as well as up in this corner here.

Following along those lines, looking at--as you well know we've done predictions, extensive predictions.

This is just another--this is a blowup of one of the previous tomographs for resistivity for a plane intersecting the heated drift right about midway down the heated drift.

Color scheme is the same again, drying around the wing heaters and around the drift where the canister heaters are influencing, and then wetting up in this corner. And this is just a prediction, again a dual permeability simulation, showing that we would expect to see drying--no surprise--and expect to see some wetting on each side of the heated drift because of the influence of the fractures in the middle non-lithophysal unit.

Geochemistry, we're primarily out of the holes drilled from the observation drift. We're analyzing--we're collecting a lot of water. We're also analyzing gas chemistry as a function of time. These are two of the boreholes from the access observation drift.

This is work that's been done by Lawrence Berkeley, both the field work in terms of collecting the gas, analyzing the gas composition, and also the predictive modeling. Eric Sonenthal at Berkeley's been doing that a lot, in conjunction with Yvonne Tsang's hydrologic modeling.

This is again two boreholes. The data--the actual data--this is a time series, and CO2 concentration in parts per million. The data is actually shown in the-- what appear in this particular thing to be kind of like brown diamonds. The measurements are right here for each of the boreholes, and then we're also showing the predictions. This is a dual permeability prediction, so we'll have predictions for CO2 concentration in the fractures and also in the matrix.

You can see we've done a relatively good job of predicting the CO2 concentrations, and I also know for a fact we came back in and we've taken additional gas analyses, and we're seeing a rise in the CO2 concentrations consistent with what we're seeing in the model. So we were seeing a leveling off here, but now we've seen another rise in the CO2 concentrations.

On to the saturated zone. I heard a presentation from Nye County yesterday, and I won't dwell on that again. There is poster session on the DOE--the data that we're using at DOE to--from the Nye County work, to incorporate into our saturated zone work.

This is just a list of the kinds of things that we're using in our models, and will be used and documented for the SR: lithologic data, some of the water level data, pump testing. There are some very interesting preliminary results on sorption analyses from the alluvium, for some of the real bad players from our perspective, Neptunium, Iodine and Technetium, and that's actually over on that poster.

But we're seeing Kds, non-zero Kds, relatively high Kds, which can provide a lot of--it's a good thing, could be good for performance in terms of flow through the alluvium and sorption of some of the key radionuclides. We're looking at hydrochemistry for calibrating the flow field, and there's quite an extensive discussion of that. And then Eh/pH.

Terms of the process model capability, we've done a lot of improving of our capability within the saturated zone and transport model based on we had in VA and how we're evolving towards SR. Some of the--a couple examples of how we've improved that capability, we can now handle any source size, and we're also not having problems with grid size impacting the source size or introducing any kind of numerical dispersion.

Al Attabar is actually--I believe he might--he's here still, and if there's any detailed questions he can walk you through that. He's the modeler. But at any rate.

Okay, quick--that was a quick one through the natural system. Now let's go to the engineered barrier.

We've talked before about the Atlas testing, where we're doing pilot scale testing for engineered barrier options, and we're evaluating different various engineered barrier configurations, capillary barriers, Richard's Barriers, standard backfield, drip shields which are more timely considering where we've evolved here in the past couple months, and looking at combinations of those barriers; and not only at ambient conditions, but we're also conducting some elevated temperature tests right now at Atlas.

They're of course providing data for model evaluation for the EBS models. I'm going to focus on the pilot scale testing. We do have--we are doing a significant amount of properties testing at the Atlas facility, but I won't talk too much about that today.

In the way of an update, you've heard a lot about canister 1. That was a Richard's Barrier test that we conducted at ambient temperature. That's still going. We're just about to complete that test. Canister 2 was a single layer backfill test, at ambient temperature again. Canister 3, which is probably of interest today, was a drip shield test where we had a crushed tuff invert. That was done at elevated temperatures. That's just been completed recently.

And we're just in the process of starting up our fourth canister, and that's a drip shield with a similar invert, but this time there's a backfill over top of the drip shield, again at elevated temperature.

So to walk through an update on what we saw on the capillary barrier tests, the Richard's Barrier tests, again this is a scale about a meter and a half in diameter. We have a clear plastic tube that's kind of like the mock waste canister, a coarse with a fine backfill over top, and then we're dripping a line infiltration system along the crown of the test canister.

Then we have load cells, so we're going for complete water balance; and we have wicks at the side that are wicking the water so that we can again constrain the water balance in the system. The focus of these tests to date has really been on where's the water going, trying to understand how the water's flowing through the EBS system.

This was presented at the last meeting. Just to remind you again, canister 1, we're looking at effectiveness of that capillary barrier to divert water. We've seen that a large amount, greater than 97 percent, of the water has been diverted by that barrier.

We've seen water break through at the wicks placed here, and also some breakthrough at the bottom of the canister. And we're seeing some wetting within the course. We think that's primarily due to the presence of fines in the coarse material. So there's some wicking going on in the fine material.

We're also doing flow visualization tests at Sandia, laboratory tests to complement the pilot scale tests in Las Vegas. We've constructed some mimic cells at a similar scale, and again to evaluate our conceptual models and also to complement what we're doing in terms of the pilot scale tests.

In this particular example, this is again a Richard's Barrier course with a fine material here, and infiltrating from the top of the cell. We put no wicks on the right side, but we have wicks on the left side. And the next slide is going to show a time sequence.

The blue is showing the infiltration of the water into the system, and this basically shows the water balance--but let's concentrate on the time sequence, four days through 82 days. Again this is the fine material overlying the coarse material with kind of the mock waste canister there more in the center.

Can see the water is pretty effectively diverted by the barrier, but we're seeing some wetting within the coarse, same coarse material. We think again that's the influence of the fines, probably wicking water into the coarse material.

You can see the influence of the wicks. You're getting--basically the wicks are taking the water on the left side, but we're getting damming up on the right side because there's no wicks; and so we're wetting significantly within the coarse material on the right side of the test canister. Next slide please.

Couple points about the testing that we're doing there on the capillary barrier. It's different than a typical capillary barrier. Again we were infiltrating on a line along the crown of the test canister, so it's single infiltration point along the line versus uniform infiltration, which is more standard for a capillary barrier type barrier.

We also have a fine boundary versus a long boundary--we're calling here a wick boundary condition. The canister's finite, a drip would be finite. And that requires that we use not simple analytical solutions like you get in the Ross equation for capillary barriers, but we're doing simulations using TOUGH 2 to predict this test and then analyze the results.

Just to bring home the point, the typical capillary barrier has an extended coarse/fine interface and also uniform infiltration along the top, whereas an EBS barrier has a single point infiltration with an impermeable boundary at the sides. That just drives home the point that we really have to model these things differently than you do a standard capillary barrier.

So again, the Richard's Barrier test is very close to being complete. Canister 2, we looked at a plain backfill. That was the material used for the plain backfill was very--was the same material that was used for the coarse layer in canister 1.

In the way of observations, we were really focusing in canister 2 on how well we could deal with the water balance. We were also looking at the performance of a plain backfill, similar layout, clear acrylic tube, single layer backfill, ambient temperatures. We observed water at the top of the package very quickly, three days, and saw water at the drainage wicks in seven days. So breakthrough very quickly.

We were able to do a pretty good water balance, but for the backfill that we used, those properties, it basically does nothing in the way of providing any hydrologic protection to the simulated waste package.

We did go in in canister 2 and do some post-test characterization. This is that acrylic tube. Here's the outer surface of the test canister. We went in and shoveled out the backfill very carefully. We were using dye in the backfill, so we were able to sort of qualitatively map where the fluid had gone during the test.

There's some lines drawn to lead your eye--I guess you have to take my word for it--but we were able to see the dye, and we say that basically the water moved down by gravity and spread around the waste package, and it remained relatively dry on the edges of the canister.

So in the way of some conclusions from the first two tests, we can do some simple pretest modeling and it gives reasonable results for the performance of the Richard's Barrier. The capillary barrier does divert the water toward the drift wall.

The standard backfill, at least for the properties that we had, has basically no diversion capability. And of course, you know, we're different than a standard barrier and the performance is dependent upon the boundary condition to a large extent, and also how you infiltrate on top of the barrier itself.

Moving to the drip shield concepts, which are of course more appropriate to where we're going with our design concepts right now, this is a layout of test canister 3, similar scale, one and a half meters in diameter. We had a simulated waste canister; this time we're heating. And then we have a drip shield. It's a stainless steel drip shield, but a drip shield, but a drip shield about similar dimensions over top of the waste canister.

We heated with a single element heater in the waste canister, and we also had guard heaters on the outside of the canister. We tried to--we maintain the surface of the waste canister at 80 C and the surface of the entire test canister at 60 C, 60 degrees C.

First we went in and just heated up, just within there, just with the waste package, then we emplaced the drip shield and heated for longer; and then we started dripping at very high rates, again from the crown. I should also mention there was a crushed tuff invert, but no backfill. Next.

This is just pictures of the same thing that I just described, the waste canister with the single element heater, and then the drip shield with the crushed invert, crushed tuff invert.

Preliminary results, first we didn't see any dripping from the inner surface of the drip shield. That's the big take home. There was different thoughts on that, but we didn't see any significant condensation. It was contacted by moisture, but that was primarily by leaking through the drip shield joint. But drips did not form and drip onto the waste canister. So the surface didn't come in contact with moisture, we didn't see a lot of salt deposits on the outer surface of the drip shield in the invert.

We had--Livermore had installed coupons in various parts of the test. Carbon steel coupons on the outer surface were visibly corroded. These are all visual observations to this point. There's a lot more information on that I believe right now, but I'm not prepared to speak to that in detail.

We had titanium coupons on the outer surface and those appeared to have an oxide film. And then the coupons between the drip shield and the waste package showed no obvious change, no obvious develop of film or corrosion.

Before I move to waste package, canister 4, I don't have anything in the presentation. That's in the process of just being completed and up and going. The backfill part is I believe going to start today or tomorrow, or it might have already started. There we again got similar configuration of canister 3, but we're going to put a backfill over top of the drip shield.

So I think if you have an opportunity to go over and see that you'll be able to hear more and next meeting we'll have some results on that test. And then further testing of variations on that theme with drip shield concepts, probably changing the temperature regime that we're at, et cetera, is sort of the longer range plan.

Waste package materials testing, again, objective as you all well know is to confirm--look at corrosion rates and corrosion mechanisms for our candidate materials, for the waste package and the drip shield. We're doing both long term and short term testing and looking at a range of water chemistries, J-13, concentrated J-13, et cetera.

We're looking at all the different corrosion type mechanisms and all the important things that might drive corrosion in our system, cyclic polarization, hydrogen pickup, the influence of microbes, development of passive films on some of the candidate materials like Alloy 22 and titanium, using atomic force microscope. Because some of these things take so long to form we're using some very detailed microstructural examination with the microscope to try to get at the mechanisms and the rates of some of these films being formed.

Stress corrosion cracking I know is of a lot of interest. We continue to look at that, and hydrogen induced cracking in the titanium alloys. And looking at welded sampled to get at induction annealing and laser peening of samples. And then of course looking at long term thermal stability of Alloy 22 for development of intermetallic phases and how that affects the stability of Alloy 22 over time.

And that was really fast, but I made it through.

RUNNELLS: Thank you, Mark. You did indeed make it through, and you made it through right on time--maybe a little to spare. It'll give us a chance for questions, beginning with Paul.

CRAIG: Okay, I just would like a little background. There were a lot of actors involved in your presentation. You've got a lot of people here. Wonder if you could quickly go through and tell us who is actually doing the various pieces of work--

PETERS: You bet.

CRAIG: --you're describing.

PETERS: You bet. You bet. I'll just go through from the start, okay? Alcove 1, USGS, Alan Flint, PI. Is that the kind of detail that you're looking for?

CRAIG: Yeah, the organization--

PETERS: USGS. Alcove 4, Lawrence Berkeley. Joe Wang is a good contact on that. ESF niches, Lawrence Berkeley, Rob Trautz is the PI for that. Help me out--cross drift, Alcove 8, that's a combined effort between USGS and Lawrence Berkeley. Again Al Flint, Joe Wang are good--good guys on that.

Niche 5, Rob Trautz, Lawrence Berkeley. Systematic hydrologic characterization, that I didn't talk about but Bo alluded to, that's Berkeley again, looking at air-K, Yvonne Tsang's going to be heavily involved in that. Bulkhead studies, USGS, and Berkeley, same players.

Those guys are busy. Flint and Wang are very busy.

Alcove 5, everybody. All the laboratories, the U. S. Geological Survey, they're all involved. What have I left out? Saturated zone, integration of Nye County results, USGS is heavily involved. Rick Spangler, stratigraphy. Al Attabar is a good contact overall for that. He's the PMR lead for the saturated zone.

Los Alamos is heavily involved in the sorption analysis and the detailed modeling. Where am I at--EBS testing, Sandia. Livermore is heavily involved in the modeling component. Sandia does a lot of the day to day conducting of the tests. Waste package, as you know, is Livermore. Dave Stahl is a good contact, Joe Farmer.

That get it all?

CRAIG: Good. Thank you.

RUNNELLS: Does that answer your question, Paul?

CRAIG: Yeah.

RUNNELLS: Okay. Question for Priscilla Nelson.

NELSON: You can ask me one, but I'll ask you one first. Nelson, Board. Just a couple that will probably be short.

First in the bulkheaded section, one of the reasons I always thought to do this was to see if there was air exchange. What kind of mass permeability and flux of air could we expect? Do you get any handle on any air exchange into, out of the bulkhead--

PETERS: From the ventilated--

NELSON: Through the rock mass, one would assume, rather than--assuming the bulkhead itself is not leaking.

PETERS: They seem to be sealing--I think I know what you're getting at--they're sealing up the opening pretty well, so we're still seeing some evidence of drying. That may not necessarily be from the opening and leaking around the bulkhead in some way. That may be actually flow in the rock itself; you get all the time.

NELSON: I wonder if there is a way to get a handle on that because that would be interesting information for the passive condition--

PETERS: Right. I think we're probably collecting data that will allow us to get a handle on that, but I'm not sure how much we're thinking about it from that perspective. You know, the evidence that we're seeing of drying and continued drying in some areas and along fractures, I think there's probably something there. It's a good point.

NELSON: Yeah, and particularly because you are getting focused drying along--

PETERS: Um-hum.

NELSON: --indicated were fractures--

PETERS: Yeah.

NELSON: --which might indicate that there is some air flux--

PETERS: Right.

NELSON: --through the fractures. It should be interesting from the modeling perspective.

PETERS: Yes.

NELSON: Okay, let me ask you about this. We saw it referred to a couple of times this morning, but the idea of rock mass stability and how that affects seepage.

PETERS: Um-hum.

NELSON: And there was discussion about perhaps running a thermal test--

PETERS: Um-hum.

NELSON: --in the cross drift. Is there any plan to really evaluate how the thermal pulse may affect rock mass stability? I'm just trying to get a handle on whether there is an impact of a hot repository on stability.

PETERS: Of the opening. That's one of the things that we're--in terms of mechanical, one of the things that we think we really want to go after is the M/H coupling, mechanical/hydrologic coupling in the rock. Let me talk--I know that's off the line of your question; let me talk about that first.

We'd like--you know, we think that it's second, third, fourth order effect. Bo I think alluded to that this morning in terms of the M/H coupling. We want to go after that in the lower lith. In terms of looking at the stability of the opening we would like to look at--we're looking at ways to try to design and test to get at seepage under thermal conditions, and I mean--what else would we do except for just monitor the opening and see how it performs under thermal? I mean we're doing that in the drift scale test now. I guess--

NELSON: I guess there could be some focus measurements across discontinuities to see if there is any--

PETERS: And--

NELSON: --in the general condition. The reason I bring that up is because it appears to be one of the things that's involved in design--

PETERS: Yeah.

NELSON: --of the--what do they call it--the canisters--

PETERS: Right.

NELSON: --the drip shields. And with the probabilistic approach going on to really characterize the rock mass now, to try to understand how frequent fallouts might occur, the thermal impact would be important--

PETERS: Right.

NELSON: --in trying to evaluate cold versus hot repository benefits.

PETERS: We'll absolutely do that in MPBX type arrangement. We've done stuff like that in the drift scale test, but I could see where you could put the extensometers or something, or strain gauges across individual fractures--

NELSON: --opening, yes.

PETERS: Yeah, to look for that. We did that in a large block actually, and so that's certainly something we should consider as we go into this lower lith test, yeah.

NELSON: But in that compressed environment--

PETERS: Yeah.

NELSON: --interesting to see what happened. Do I have time for one more?

RUNNELLS: Yes.

NELSON: Okay, is there--I would expect that in the long term for the backfill scenario, whichever one you're talking about, that given natural water you may well build up some cementation.

PETERS: Right.

NELSON: In the backfill. And you might even be able to detect it in some of the experiments now, you know, with very careful measurements, a small stream, seismic measurements might pick up that gain and stiffness--which seems to me might have something to do with how backfill performs.

PETERS: Right.

NELSON: Long term. Are you looking for that or are you in any way going to be able to evaluate any of that from your tests that you're running on the backfill?

PETERS: We're evaluating it absolutely. We're focusing on column experiments. We have--also at the Atlas facility what I did discuss today was we're starting a series of column experiments where we're putting invert and backfill type materials and doing flow through experiments to look for the chemistry effects.

Pilot scale aren't the greatest thing to look at for those things. We'll characterize the backfill, try to characterize it; but we're using the column as a better constrained way of getting at the chemical effects.

NELSON: But it would include the evaporation--

PETERS: Yeah.

NELSON: --access as well as you would--

PETERS: Right.

NELSON: Think about it, because--

PETERS: Okay.

NELSON: --there's probably some information to grab there.

PETERS: It's just harder to control in that pilot scale test. It's easier to deal with in the column type environment.

NELSON: Thanks.

RUNNELLS: Question from Leon Reiter of the staff.

REITER: Leon Reiter, staff. I've two questions, and I don't know if you're the person to answer them, but I'll ask them. First question, now that you seem to be confirming Alan Flint's estimates of water potential, what does that mean for the repository and its performance and performance assessment?

The second question, and this--tried to ask it before. I'm not quite sure I've gotten the right answer. It seems to me the project is leaning away from backfill because of the concerns about the thermal affects, that they might cause too much heat.

Maybe you can explain to me how in other countries like Sweden and Finland, where they use a lot more backfill, their are thermal constraints are much more severe, they're concerned about the bentonite not being above 100 degrees, how do they manage to do it? Is it because they have different fuel packages, they space them apart, they cool them; and why can't we do these kinds of things?

SPEAKER: Say thank you.

PETERS: Yeah, thanks, appreciate--you know, I'm going to do the logical. The second one I'm not going to try to answer myself. So I'll defer that to the audience.

The first one, we're going--Bo--I'll probably ask Bo to comment further; but yeah, the water potentials that we're observing in the cross drift, as we confirm that we're converging on an answer that appears to be these are really what they are as observed from the cross drift, those will have to be incorporated into the modeling effort.

Now we've been using--you know, I'll speak for Bo since I'm up here, but he's going to have to either confirm or deny--we've used data--the available data really up until this was really based primarily on the surface base measurements. That's really where the water potential--a lot of the water potential information was coming from. The differences there will have to be dealt with in the modeling process through sensitivity and possibly alternative calculations.

Bo, are you in here or did you leave? He left.

REITER: Do you have any idea what the impact might be--

PETERS: I wouldn't want to speculate on that, Leon. That's Bo's answer, on the impact. The second one, I've been completely not personally involved in the details and the decisions on backfill, so I'd really rather not even try to answer that.

Is somebody in the audience willing to do so?

SPEAKER: That's in the saturated zone.

PETERS: Okay, well--go ahead, Dave.

STAHL: David Stahl, M&O. I just want to take a crack at answering Leon's second question having to do with the other repositories. These are of course as you know saturated zone, much lower thermal output per waste package. For example we're looking at 21 PWR assemblies in a package. Most of their designs look at either 4 or 9, so it is a much lower heat output.

They're also looking, as you know, at keeping the backfill below the boiling point because that's when the bentonite begins to degrade. So they need that combination of high conductivity and low thermal output to keep the temperature down. So that's how they approach it.

Does that answer your question, Leon?

REITER: Where is the thermal conductivity--the thermal conductivity is higher?

STAHL: It's higher for the bentonite, yes, because you don't have air in there. That's what keeps the conductivity lower in the case of the crushed--any crushed material.

LEITER: So if we had a strategy for low temperature repository could we adopt some of the methods that other people are using, or is it possible?

STAHL: Oh, of course you could, but it would be a much more expensive repository. You'd need much more area, and we want to take advantage of the unsaturated nature of the site rather than going to a saturated repository. That's a whole different discussion.

RUNNELLS: Question from Dan Bullen.

BULLEN: Bullen, Board. Mark, you did a great job of giving us an overview of all the data that are coming in. The same question I asked Bo this morning with respect to the availability of data in the AMRs and PMRs, and how it feeds into the decision process for I guess the characterization report, consideration draft this November, and TSPA-SR that will be coming out; and then I've got a quick followup after that one, but I'll let you do that one first.

PETERS: For the Rev. 0, for the consideration draft, the data, what I'll call freeze, or the data that can make it into that, was really collected as of the end of last summer. So what I'm talking about here in terms of anything that's been collected beyond that is all up until the summer time frame going to go into Rev. 1.

BULLEN: Okay, and that will be the Rev. 1 for--

PETERS: For the final--

BULLEN: --TSPA-SR.

PETERS: For the final SR.

BULLEN: Right.

PETERS: Right.

BULLEN: Okay, so then following question to that--

PETERS: Let me--let me just--

BULLEN: Oh, okay.

PETERS: --one clarification. A lot of it will be based on impact analysis. We've made certain assumptions in the Rev. 0 and we'll see additional data, and there may be impact analysis done. We may not change, significantly change the models. It may just simply--

BULLEN: You're a great straight person, because that was the question. What's the critical pieces or what are the critical pieces of data that you expect to see--

PETERS: For--

BULLEN: --or be needed--or be required for the SR? Is there anything in here that we should really be paying attention to, that should jump off the page at us?

PETERS: I think the seepage stuff in the cross drift, and we're putting a lot of effort in the field to get that--get as much as we can by July time frame. That's one that you should be looking at, because we're spending a lot of time and effort to make that happen, working in some extra time.

Some stuff associated with the stress corrosion cracking I think is important. I think the drip shield, as we continue some of the tests on the drip shield in Atlas, I think that's important to watch.

BULLEN: Thank you.

RUNNELLS: Question from Dave Diodato of the staff.

DIODATO: Diodato, staff. I--with regard to the seepage issues Bo presented this morning, those--all those experiments done at ambient temperature, and I'm just wondering in a higher heat situation where you might tend to reduce the viscosity of water, the mechanism for limiting the seep that was evoked was capillary tension phenomena.

So it seems to me reducing viscosity of water, one, might reduce the capillary tension and result, you know, in increased potential seepage--is one thing to think about. SO the idea of these thermal experiments, I think if you're going to go with a high heat design you might--might be something to think about.

The other thing is the geologic model that you have, your stratigraphic workbook--

PETERS: Yeah.

DIODATO: --slide, I had the impression that you're coming to closure on a geologic model. Is that--would be a static final geologic model, or would there be possibility as the drifts are drilled for example to add to your database and keep this as a living model and add to knowledge as we go and that reduce the epistemic uncertainty that we learned about yesterday?

PETERS: As we would--right now, I mean we've done the mapping at the surface. That's complete. We've mapped the ESF and cross drift. We're not drilling any additional deep surface boreholes right now, in the plan. So the data is what it is now. I mean absolutely if we were to go off and do some other things, that would be updated. But we are converging on sort of a final product there as we go to SR. The flexibility--you know, it of course could be updated.

DIODATO: Thank you.

PETERS: We have no plans to any additional data.

RUNNELLS: Question from Alberto Sagüés.

SAGÜÉS: Sure. The test canister number 3 tests--

PETERS: Um-hum.

SAGÜÉS: What kind of liquid was it that they're dripping?

PETERS: It was straight--it was water, straight--I believe it was J-13.

SAGÜÉS: Oh, was it like a J-13?

PETERS: Yeah, I believe--yeah.

SAGÜÉS: What was the temperature of the simulated by the surface?

PETERS: The whole test canister itself was maintained at 60 degrees C, and the surface of the mock canister was 80 degrees C.

SAGÜÉS: Okay, I was interested when you mentioned the titanium coupons having oxide film. Was that like an invisible -- they found or was it like a clearly visible--

PETERS: I--

SAGÜÉS: --something like that?

PETERS: I don't know the answer to that. Dave, are you familiar with those observations at all, on the oxide films?

STAHL: Yes, on the--David Stahl, M&O--we did take some photographs of those samples and we weighed them, but we haven't done any detailed analysis on those samples yet. Some of that was due to staining. There were some dyes that were used that we're not 100 percent certain what the cause of that discoloration is at this point in time. Certainly we expected the carbon steel exposed to moist conditions to rust, and it did. And the other materials were by and large unattacked, but with some staining in some cases.

SAGÜÉS: --is brand new yet. It's just couple--some kind of deposit--deposit--other than the corrosion product.

STAHL: I'm sorry?

SAGÜÉS: A deposit other than a corrosion product, I would expect.

STAHL: Nothing out of the ordinary.

SAGÜÉS: Thank you.

RUNNELLS: Other questions from the Board? Yes, Dick Parizek.

PARIZEK: Parizek, Board. Question about the Kd work. Is there additional samples being planned to be collected from the current drilling, to do more Kd work? And I guess as I understood the first samples were from very coarse textured material; there also seems to be plenty of clay, minerals also present. So will there be additional Kd work and will it also include some search through the clay fraction of the boreholes?

PETERS: Yeah, you're right. The initial samples were--the fines--our protocol as we've done with all of our bad sorption work, is you analyze the coarse fraction. So you're right; so there's--it could be that the fines could be--provide additional benefit.

Right now in the plan--there is an additional plan, but we are considering, seriously considering looking at doing some additional work there. But right now if you call Jim Conk (phonetic) on the phone he doesn't--he's not doing anything right now. But we're--DOE's considering that with a lot of other things, to bring back into the plan.

PARIZEK: And one other question about whether you have any pneumatic data from behind the bulkheads. Is there any attempt to measure pneumatic responses in the--

PETERS: In the opening or down hole?

PARIZEK: Well any opening let's say over toward the fault side of the--

PETERS: Right now we don't have anything, but in talking to Alan Flint, we're talking about doing some additional measurements behind the bulkhead based on some of the observations we've had with condensation in certain areas, and that includes Rh and maybe some pressure measurements to try to understand the flow within the opening a little better, because there's some interesting dynamics going on there..

RUNNELLS: Mark, I have a question about the CO2--the concentration of CO2 in the gas. What's the source of that? Is that a breakdown of some kind of carbonate cement or something? What's the explanation?

PETERS: I think it's primarily just heating of the pore water, the gas in the pore water.

RUNNELLS: The gas in the pore water. There's getting to be some pretty high numbers--

PETERS: Um-hum.

RUNNELLS: --in there.

PETERS: --percent levels, yes, very high. And they've looked--Mark Conrad at Lawrence Berkeley is doing a lot of that work. He's I believe found--done some carbon 14 as well and it's mostly dead carbon, for your information. So dissolution of calcite--calcite sources come to mind too, but it appears to be mostly the pore water.

RUNNELLS: The model was not fitting very well until you said you have new data--

PETERS: Yeah.

RUNNELLS: --kicking back up.

PETERS: Yeah, you're right. Don't really know why they flattened out like that. They think--we had a power outage of about three or four days just before that, and so we were speculating on the phone yesterday that maybe it was because we turned off the engine. So we--that's pure speculation.

RUNNELLS: Okay.

PETERS: --because sure enough, they started coming right back up.

RUNNELLS: Okay, I have one other I think quick question; then I'll ask for other questions from the Board and staff. You've mentioned in looking at your figure 21--and we don't have to go back to it, that's the one of the water potential measurements, comparing the psychrometers with the heat dissipation units--that they were converging rather well, I think you said.

PETERS: Yes.

RUNNELLS: To my eye they're actually crossing. The dry installation continues down and the heat dissipation probes--

PETERS: Yeah--

RUNNELLS: --like they're continuing up. There are different depths in the rock. But regardless of that, what difference would it make--if I were to look at the numbers--

PETERS: Right.

RUNNELLS: --I see a difference in bars of about .5 bar. PETERS: Um-hum.

RUNNELLS: What difference does that make? I mean what's the implication of whether or not they are converging? With a .5 bar there--

PETERS: That's what's--I think that's basically--I mean not being an expert in that instrumentation--but that's basically really within the error of the measurements. You know, the error on these measurements is probably substantial, quarter bar, half to a bar minimum.

RUNNELLS: It feeds into the seepage more or less--

PETERS: Yes. One of the--the other thing is, is these are actually--yeah, they're different depths. That's important to notice--you pointed that out. But the two meter depth, we shouldn't be seeing a lot of effects of ventilation there at two meters depth. And they're behind the bulkhead. SO--

RUNNELLS: It may turn around and start--

PETERS: It may turn around. I's a little bit of an apples/apples--it's not totally apples/apples because they're different depths, and it's also very preliminary. But we were concerned. What we're really concerned about, I would be more worried if this was sitting way up at 3-1/2, because that's what some of the surface measurements were telling us.

RUNNELLS: Right, right, right--

PETERS: So at least we're not seeing an instrument artifact.

RUNNELLS: But you're--without worrying about the details of the model you read would be that .5 bars is not going to have any great affect, let's say--

PETERS: That--that would be my--

RUNNELLS: --threshold seepage number.

PETERS: Yes, that would be my take.

RUNNELLS: Okay. Thank you. Question from Priscilla Nelson.

NELSON: This may be a little bit off the way or out of the mountain, but the title of your talk was Scientific Programs. And you present the--this is Nelson, Board, sorry --you present the stratigraphy, the site materials work as being fairly well completed and canned, or for this major iteration.

But I'm wondering about the alluvium, and material that's not directly in the mountain or in the ESF or in the cross drift; and wondering what the scientific program is to really characterize the alluvium, the variability of the alluvium; and even the interface between rock and alluvium out in the downstream part of the flow path.

PETERS: So you're talking down--

NELSON: Out--

PETERS: --in--in--where--where SZ hits alluvium, down gradient.

NELSON: Yeah.

PETERS: Not--not on top of Yucca Mountain.

NELSON: Yes, and so this is--may fall into hydrology, but it also falls into material characterization in terms of how variable--

PETERS: Right.

NELSON: --should that alluvium be. Is there anyone address this? Is it a component of the project other than just from the standpoint of hydrologic testing at specific depths and boreholes, really trying to get a conception of what the variability of the alluvium is?

This question is derived from several conversations with Richard Parizek as well, so I'd think he'd second the general question about what the project's doing regarding characterization of alluvium.

PETERS: We're--that information's coming from how we're integrating the Nye County results. I mean Nye County's down there drilling and looking at those kinds of things. The U. S. Geological Survey, Rick Spangler in particular, is looking at those issues. I had a bullet about the stratigraphy. There's discussion of it over there as well.

We're integrating the Nye County results as best we can, to look at the stratigraphic--the hydrostratigraphic aspects of alluvium as they're drilling their holes. That's the program. Al's standing up and wants to say more, but that would be my take.

ATTABAR: Attabar, the M&O. The project is also planning some testing complex, called the alluvium testing complex, down in the Armagossa Valley to correct -- the alluvium and collect data on--hydraulic data--on the flow in the alluvium, and also validate in some of that transport process in the alluvium and get in transport data in that portion of the flow paths in the alluvium.

So the characterization of that portion of the flow paths is an important aspect of the SZ. And as Mark mentioned, the Nye County exploration has been integrated into the SZ fluid transport model, and in addition to that the project is planning this alluvial tracer or testing complex which include hydraulic testing and also conservative and reactive tracer testing.

NELSON: Okay. I think my question direction, really the standpoint of so much careful characterization of what the rocks in the mountain are, and what the rock mass characteristics are expected to be. And understanding how variable this alluvium is from a few boreholes is difficult outside of a geologic context for environment and deposition.

And they're difficult to sample, and to really say a lot about grade size distribution, lateral continuity, many, many other characteristics of an alluvium that are really going to strongly influence the long distance travel information as opposed to the short C-well type complex information.

ATTABAR: I think it's a very important issue. The Nye County early warning drilling program is planning a total of 22 wells that are perpendicular and parallel to the flow path. And I think you're prepointing to a good question, and that is the scale of testing as opposed to the scale of the actual flow path.

We are hoping that the multidisciplinary approach to the characterization will help reduce the uncertainty in that field. From the 22 wells we are collecting a wealth of information regarding lithology. And we have also aero-magnetic information, and also the hydrochemistry. And the testing at the complex will be in a few phases, and my personal opinion, it's going to take a long time.

We are going to get some of the early information into the SR and some more broad information into the LA, but I think a lot need to remain to be done for information purpose, especially in terms of--you know, the heterogeneity and then the scales problem that you are talking about.

NELSON: Thank you, I'd really like to reinforce this whole stratigraphic sense of exactly what's there and its heterogeneity is really important, and I think your multidisciplinary approach is one which is good; and I encourage it to expand to all variety of information.

PETERS: I guess I would also add, you know, it gets back to maybe the uncertainty discussions. We're going to have uncertainties with this as we go forward, and we're going to have to--it's how you handle it in the performance assessment where it comes together.

So when we go to SR and LA we're not going to know as much about the alluvium downgrade as we do about the lower lith underneath the mountain. It's just reality.

RUNNELLS: We have time for perhaps one question more. Dick Parizek--

PARIZEK: Parizek, Board. Brian Marshall in the public comment period, I think what he said was that the 200 millimeter value for, you know, drips is not supported by the hydrological data. What's the program going to do about that? Did I miscast what he said?

PETERS: No, no, no. We're going--

PARIZEK: So then this question, how to deal with this? PETERS: Ahh--

PARIZEK: --flagging a concern--

PETERS: I mean--

PARIZEK: --program.

PETERS: --the information's being gathered based on calcite distribution--

PARIZEK: Right.

PETERS: --et cetera, lithophysal cavities, if I'm familiar with it. We're going--I mean Brian works on the project along with me and the others, so we're going to have to understand the implications there. But we're seeing certain things in the field tests, and if he's seeing something different in fracture minerals, that has to be dealt with.

RUNNELLS: I think he was suggesting perhaps even a different mechanism--

PETERS: Yeah.

RUNNELLS: --than has previously been looked at.

PETERS: And that absolutely has to be addressed.

RUNNELLS: It's in the film precipitation, that sort of thing. In a couple of minutes, Mark, could you just describe to us what you have seen, what the researchers have seen behind the bulkheads in the section that is being closed off?

PETERS: Yeah, the first entry, we didn't see anything terribly exciting, in September. But when we went in--I wasn't there, I was actually getting ready for this talk, I would have liked to have been there--several of the PIs, Berkeley and USGS were there, some of my folks.

We went in and we saw some areas of organic where there was mold, quote mold, growing in the cross drift. We had seen that in alcove 7. Remember alcove 7's bulkheaded off. We'd seen that.

But then the interesting thing was, as we--about a 50 meter section of tunnel--this was alluded to a little bit this morning--just before the second bulkhead, so from about 2450 to 2500 there was a lot of condensation on the bent line, on the cables. Don't think that it--no apparent evidence it had dripped from the rock, but it condensed from the air. Now we've got to understand why, what's going on here.

We think right now that there's a temperature gradient in the tunnel because the TBM is still parked at the back end. And it's powered because we don't want it to rust in place. So there's probably a temperature gradient. This is preliminary. Alan Flint can speak more to it.

There's a temperature gradient in the tunnel and we may just be condensing along the temperature gradient. We've got real high humidity--it doesn't take much--and you're condensed. So that's what we saw.

We're still grappling with what exactly it means, and how we're going to go forward with the test--with that testing program, because, you know, you've got to--if you're condensing, is that drips or how are we going to tell if it's really dripping. Those are the kinds of questions that we're asking. Premature to really say what our solutions are, but we're working it.

RUNNELLS: Okay, appreciate that description. Thank you. With that we'll close, and thank you very much for your presentation and time.

PETERS: Thank you.

RUNNELLS: Our next speaker is Ardyth Simmons. Dr. Simmons has a Ph.D in geology from State University of New York at Buffalo, and since 1995 she's been a program manager in the earth sciences division at Lawrence Berkeley. Prior to that she was geochemistry team leader of the DOE Yucca Mountain Project.

And Ardyth is going to talk to us about natural analogs. Welcome, Ardyth.

SIMMONS: Thank you. It's a pleasure to be here.

For some of you who have been around the program for a few years you'll recall that the Board had a meeting in I think it was April of 1991 that dealt with natural analogs. The project has changed quite a bit since then, but some of the analogs remain good analogs.

So my presentation is going to talk about the studies this year as well as the role within the program of natural analogs, and the current work that we're going to be doing this year and the next years to address uncertainties.

But first I'd like to give a definition, our working definition of natural analogs. And we are referring to both natural and anthropogenic, or human-produced systems, in which processes similar to those that are expected to occur in a nuclear waste repository are thought to have occurred long time periods--that's one key--and large spatial scales that are usually not accessible to laboratory experiments.

This is the benefit of natural analogs. There is a caveat however, in that they must be carefully selected to exclude analogs for which initial conditions are poorly known or where key groups of data such as source term are poorly constrained. So it's important to select analogs carefully.

Now within the Yucca Mountain Project the TSPA-VA in '98 did address natural analogs as a means of building confidence in certain process models. But there were no specific recommendations as to particular analog sites to study. So that was one of the things that we wanted to look into.

I also want to call to your attention that natural analogs are the fourth element of the post-closure safety case that was talked about earlier in this meeting, and you'll find them addressed in chapter 2 of the booklet, if you picked it up. The NRC also anticipates that our program will use natural analogs as a means of building confidence in modeling processes.

Now I'm not going to go over this table in detail. This is from the actual repository safety strategy. But what I want to do is highlight the shaded areas, which are areas within the safety strategy that can be addressed effectively through key natural analog studies. So you'll see that not each one of the factors, but many of them, can be addressed through analogs.

Now in addition to confidence building in modeling, which is one of the primary uses of natural analogs, there are other uses as well. They include confidence in design, verifying that codes represent processes correctly through the use of data from analog systems, testing databases by use of thermochemical and kinetic data, particularly in these areas; and also for public information and education. And all of these are important.

In FY99 these are the particular items that we worked on, and I will not be--I won't have the time to talk about all of them to you. I'm just going to highlight a couple of examples. But the first thing that the project did was to synthesize relevant analog studies from the literature to provide a foundation for future work.

Another aspect was fracture flow analog at Box Canyon, Idaho, and this was a modeling study in a location that was just outside the border of Idaho National Engineering and Environmental Laboratory. The purpose of this was to provide confidence building and testing of the UZ flow and transport model.

An additional component of this study was modeling dispersion in a tritium plume at Hanford, and this was directed towards the saturated zone flow and transport model. One component that I'll be talking a little bit more about today is the work at Peña Blanca, Mexico that was directed to the UZ flow and transport model, and can also apply to spent fuel dissolution.

We also did some information gathering about a site in Krasnoyarsk, Russia as a potentially thermally coupled process analog. And all of this work listed here went into two products: an analysis model report for the unsaturated zone and then a synthesis report which will be a chapter in the site description to come out in 2000.

Some points about the synthesis report, again that it was to bring together information from past studies, document how the project was using natural analogs and also to make recommendations for future work in this area.

Now I want to mention anthropogenic studies just briefly. I mentioned the work at Box Canyon and at Hanford. And anthropogenic studies are a little bit different from those at the natural sites because the time periods are not in the order of thousands of years, but are usually in the order of decades.

But it's important to utilize experience from DOE sites and other sites where there has been flow on preferential pathways to try to understand this occurrence and use this to building confidence in our own models. So I've listed again the sites that we started to look at in '99, and there will be some additional work in this area in this year and the next.

Now when one is searching for an analog there is nothing that's perfect, but we look for certain characteristics, particularly in a transport, a radionuclide transport analog. We look for a known source term, similar suite of radionuclides, well-characterized data, and so on.

And so with these criteria in mind, we have been focusing on a certain deposit at Peña Blanca in Mexico, and DOE can't take credit for identifying this site. It was called to our attention by the NRC and by workers at the University of Texas. But many of the characteristics of this deposit in Mexico are very similar in tectonics, in climate roughly speaking, in geology, and so forth, to Yucca Mountain. It's probably the most closely matched analog site we have.

This is sort of a cartoon of the deposit. This is the uranium--it's a uranium deposit, and it's located in welded tuffs that are very similar in mineralogy and chemistry to the middle nonlithophysal unit in the Topopah Spring. It's a breccia pipe type deposit, and these areas are sections off at different levels. They're adits into the mine. It's an abandoned mine.

So the previous work had indicated that although the deposit is in oxidized unsaturated tuffs, the vertical migration of the uranium and daughter products appeared to have been minimal. So in the last year we did a scoping study to look at collecting additional data to try to develop a three-dimensional picture, and eventually a three-dimensional model on the transport of uranium. And we want to look at the natural barrier conditions at that site that provide isolation.

So, so far the work that the DOE people and laboratories did this year suggests that the geochemical system there restricts actinide mobility. This confirms the previous work. And it was uranium series work that was performed by Los Alamos, and a series of nuclides including uranium thorium age data that was supported by protactinium U235 activity ratios that showed that the primary transport of uranium to fractures occurred roughly at 300,000 years ago.

There has been limited migration since then, but it has been quite limited. We have a few opal and caliche data that suggest that there was enhanced fluxed on a very local scale, about 50,000 to 90,000 years ago; and that there was minor redistribution of radium and 234 uranium about 5,000 years ago. So we're looking at three different ages, and timings, of rock/water interaction and potential migration. But the emphasis is the the majority of the uranium has been in place for the last 300,000 years.

So in the synthesis report--now moving off of Peña Blanca a little bit, but still making a conclusion with regard to it, is that the sequence of uranium paragenesis or alternation minerals at Peña Blanca is a very good analog to alteration of uranium oxide spent fuel. And this has been observed in past mineralogy studies that have been compared to laboratory work done by Dave Ronkowitz.

Another point from that synthesis report is that colloid filtration has been effective at several analog sites, not just Peña Blanca, but numerous other sites that were investigated. And in most of the sites advective transport along fractures has been a more significant mechanism than matrix diffusion.

Also analogs suggest that sorption along fractures enhances radionuclide retardation significantly. So these are a few qualitative points that we've learned through analogs.

Now the report also made some recommendations for future work, and one that we'd like to work more on in the future is Rainier Mesa and apply some of this existing data and perhaps additional data to drift seepage models. Rainier mesa is located--for those who don't know it--on the Nevada Test Site north of Yucca Mountain.

We also plan to utilize data from fossil hydrothermal systems, that is systems that are no longer inactive, and to use data sets from analogs that have already been studied. There are key data sets from places like Alligator Rivers and Oklo that are ready for application in models. And also we want to use these analogs to addresses issues of public confidence.

This is a comparison of hydrogeologic data at Rainier Mesa and Yucca Mountain, and I'm just using this as an illustration--it's from a report by Joe Wang--to show that there are some differences, but there are also quite a few similarities in the two sets of tuffs and the hydrogeologic data. So this allows us to go forward with the analog.

In this year we're continuing some work at Peña Blanca. I'll say a little bit more about that. We're going to work further on the transport modeling study at the Idaho lab, modeling processes at selected geothermal sites using existing data.

There will be a small field and modeling study at Paiute Ridge, which is also on the Nevada Test Site, and this is one of the fossil systems that I mentioned previously. And then we are exploring the notion of potential process modeling of data from the Krasnoyarsk, Russia site.

Back to Peña Blanca again, this is a map showing the ore deposit, in black; the region that has been altered and influenced by the ore, in grey; and then there are three red circles here, one within the ore body and two outside it. And the one within the ore body is the location of a drill hole that we plan to drill this year through the ore body downward to a depth about 200 meters.

The other two are located away from the ore body to provide some control, and eventually we want to use the data collected from the water geochemistry and from the cored borehole to--for both analysis and building the three-dimensional model I referred to earlier.

So just very briefly, I mentioned the K-26 site in Russia, and it may be an analog to Yucca Mountain coupled processes. At this location there is 50 years of data from an underground facility that's been heated by radiation. Many of you may be familiar with it. It's appeared on "60 Minutes" and a few other television programs.

Here were have ongoing coupled thermohydrologic-mechanical-chemical processes. It's the project we wish, it's a good place to investigate the stability of cement; it's also a good place to investigate radionuclide transport at above ambient temperatures, to look at preferential fracture flow, and permeability changes due to thermal processes, including mineral alteration.

So at this point we've been having discussions with the Russians to identify potential analog information and to identify data sets, and we're also going to look at the possibility of using some deep injection data that they have as well. And that's aside from the coupled process information.

So there's a number--now in terms of geothermal analogs themselves, we're going to look at selected data from geothermal fields that are under operation now, and use data from them for testing thermochemical and kinetic databases; and then as I said, to look in addition at data from fossil hydrothermal systems.

This is a table that you can examine at your leisure if you wish, but it was--appeared in the synthesis report that I mentioned, and it's a first cut at looking at the list of Yucca Mountain issues and coupled processes. And the potential sites within geothermal fields that may be used to address these issues and approaches that might be used. And this is continued onto the second page. What we'd like to do is to start with obviously fields that have the most similarities to Yucca Mountain, which are probably going to vapor dominated fields.

So in closing, I want to draw you back to a slightly similar table to the one that appeared in the very beginning of the presentation, where I talked about the principal factors. And this table is derived from that one, but what I've done is to take, in the left hand column, the factors that are important to performance.

The middle column is the process models used by Yucca Mountain, and the third column are the potential analog sites that would have relevant information which we could use to apply to those process models. And that again is continued on the second page.

So in closing, natural analogs have the potential to increase our understanding of some of the processes that are principal factors, and also in the confidence in the performance of other--the non-principal factors such as coupled processes.

And we need to investigate further analogs that could be used to increase confidence in waste package materials and the engineered barrier aspects. This a little bit more of a challenge because of the unique compositions, but a few have been identified. And then once again, the illustrative function.

That concludes my presentation.

RUNNELLS: Thank you, Ardyth. That's very interesting. I will beat the rest of the Board with a quick question here. It seems like there's another category of analogs that you haven't touched on, and those are tunnels, drifts, caves, those sorts of analogs.

I'm thinking of the excavations in the volcanic tuff of the Cappadocia region of Turkey, which I've walked through, and they look as fresh as the day they were made; Medieval mines that still stand open in the Erskebere and Cooperchie (phonetic), or places like that.

Is there any intent to use analogs, man-made anthropogenic analogs, with regard to tunnel stability, the sort of thing--I probably stole the question from Dr. Nelson here.

SIMMONS: Tee answer is yes, and to the question about Cappadocia and places like that, you're going to be hearing from John Stuckless in just a moment about that aspect.

With regard to some of the Medieval mines and old Roman nails, old Roman constructions, things like that, cements from those days which are, you know, non-mining related but nevertheless ancient anthropogenic analogs, we've talked about those to some degree in the synthesis report.

And so I think you will be getting somewhat of a flavor, and we have not intended to exclude those types of analogs.

RUNNELLS: Thank you very much. Dr. Nelson has a question.

NELSON: Thanks. Nelson, Board. Thanks for bringing us news of analogs, however you spell analogs. But I must say I was disappointed by the coverage of analogs that was included in this repository safety strategy book, which promises future activity in the realm of performance confirmation time scales as opposed to an a priori support of the site recommendation time framework.

Indeed I had the feeling that we were going to have the project completed and it could be its own analog as the project's aim. So I'm very happy to see what you talk about here, but I think since it's been around for a long time in terms of discussion and questioning the project's intention to follow through on developing analog studies as direct support for decision making on the project.

So that doesn't really require a response, I'm sure, unless you want to. But the importance of analogs to communication, to talking to the public and to explaining what is known in a framework that people can understand, but also to think about analogs as being a way to demonstrate uncertainty about systems that have already developed and be able to put the uncertainty on the project in a context, more than developing the model; also in input data and other facets of analog studies. It's very rich.

And so I for one would strongly support moving straight on ahead as soon as possible in trying to bring some of the analog information into the project for support of decisions. Thank you.

RUNNELLS: Do you wish to respond, Ardyth?

SIMMONS: Well just briefly. First of all I appreciate your comment, and I acknowledge that some of what I said in terms of building confidence in process models is a function of terminology or semantics. And I don't mean it to be confined to very narrow, let's say, you know, parameter confirmation or something like that.

It really spans the whole idea of building an understanding of your conceptual model, the bounds of the input to your numerical models, and really in a qualitative sense--and to some degree in a quantitative sense--it's just that understanding how other systems have evolved through time and what that can tell about Yucca

Mountain in the future. So we do want to include that whole round.

One other point, just briefly, is that although the project has talked about and supported the concept of use of natural analogs for quite a few years, really this past year, '99, is the first year that we've had a pretty focused effort in this. So I see that as a just the beginning, and we'll be developing these as we go along.

RUNNELLS: A question from Dan Metlay of the staff.

METLAY: Dan Metlay, Board staff. I'd like to go to slide 22. In the second bullet there's I think an interesting verb, test. And essentially the use of analogs raises a whole set of I think important epistemological philosophy of science kinds of questions.

To what extent has the project really thought about the conditions under which one can test anything, using analogs, how one interprets tests ahead of time rather than generating sort of post-facto explanations. So maybe you could give us some of the project's thinking that sort of underlies that second bullet?

SIMMONS: I'll try. A lot of work was done in the early years by people such as Rod Ewing, who addressed the philosophical aspects of the use of analogs, the degree to which they could be used, the appropriate uses of them, and so forth.

The project itself has adopted those philosophies, you know, probably not without--not with actually saying that they endorse them, but have essentially followed those approaches. And so I think there is very well thought out approach towards using analogs and their limitations and the appropriateness of their use.

In terms of the word testing in that second bullet, the testing that we're referring to is at a variety of levels, and it's part of the insert that I responded to Dr. Nelson with. It includes testing the input to one's model; it includes testing the conceptual model; it includes testing the way the numerical model has been constructed. So it's had a variety of different aspects.

METLAY: Just a quick followup, is there anything written that reflects the sort of project use, for example on Rod Ewing's thoughts on these questions, or is this just sort of informal knowledge within the project?

SIMMONS: There is one document that I would point to, and I'm not sure that it quotes Rod Ewing. I don't believe it does. But a number of years back the project had assembled a group, a peer review group essentially, and it was called the Natural Analog Review Group. And it was composed of experts in natural analog areas from around the world, and also one of our own, Abe Van Luik, was on that panel.

And it produced a document that described an approach to the use of natural analogs and it's--their application and limitations. And this has been adopted and endorsed by DOE and is available in the public record and so forth.

RUNNELLS: Another question from Carl Di Bella of the staff.

DI BELLA: This is Carl Di Bella. I've got some questions having to do with your page 26, two specific questions and one generic question. Generic question is if a tow is not shaded, does that mean that work is definitely going to go on in that natural analog area in fiscal 2000?

The two specific questions have to do with item number 10 and item number 4. Item number 10 is about waste package barriers and I see that meteorites are going to be looked at, but I want to suggest there may be an even better natural analog for the performance of C-22, and that is josephenite (phonetic). And it is a higher nickel content--nickel iron alloy, and it was actually mentioned at the June Board meeting. So perhaps the work has been done and it's been discarded, but we've not heard about it.

And the third question is row number 4, what are your specific plans in fiscal '99 or 2000 for Rainier Mesa as far as confirmation of seepage in the drifts?

SIMMONS: Okay, let's go to your first question, and I want to make sure that I understood it. You were asking about the rows that were not shaded. The rows that were not shaded are ones that we are not focusing direct work on in the year 2000. That doesn't mean that we don't intend to do something with them. But the shaded areas are part of the year 2000 current effort.

The Rainier Mesa area in block number 4, what we plan to do with that in this particular year is to use the existing data as input to the seepage into drift component model of the unsaturated zone. That hasn't been done yet but it's in the plans for this year.

In regard to box 10, yes, I'm aware of the josephenite, and we--I will acknowledge that we need to look into that more. I know that there is at least one person on the project who knows quite a bit about it, and we need to include that in the realm of our engineered barrier and waste package studies.

DI BELLA: I think my confusion is that in the printed version there aren't two different shades of grey. There's only one shade of grey, so I thought the blank meant year 2000.

SIMMONS: Oh, absolutely right--

DI BELLA: And I see that's wrong. Okay. Thank you.

SIMMONS: It originally was shaded in two shades, and I can see it didn't turn out.

RUNNELLS: Okay, thank you, Ardyth. I think we'll have to close the questioning at that point.

I was going to mention that I've detected an increasing degree of agitation on the part of our Board chairman with regard to cell phones going off. Yeah, I'm not going to say anything about that, but I formerly had my cell phone programmed to ring the William Tell Overture, and the first time it went off in a meeting I decided to leave my cell phone turned off.

Now having seen our chairman when he's really agitated, I would just suggest that folks with cell phones, you know, think about it a little bit. I'll leave the rest of it up to him.

Our next speaker is Dr. John Stuckless, who holds a Ph.D in geology from Stanford University, is an old Harvard man--I like to say the Harvard of the West. Dr. Stuckless is a senior science advisor of the U.S. Geological Survey, and is responsible for much of the oversight of scientific documents being done for DOE.

He is going to talk to us further about natural analogs with an emphasis perhaps on seepage models.

STUCKLESS: I'm sorry?

RUNNELLS: With an emphasis perhaps on how they apply to seepage models. John.

STUCKLESS: All right, this is exciting enough so that I didn't feel we needed to add the suspense of watching me learn a new mode of presentation. So I'll stick with this overhead.

The title, you all have. This going to go somewhat like your next door neighbor's slide show of their vacation. Most of these pictures are meant to be looked at quite quickly. This is going to be qualitative.

SPEAKER: Raise the mike up a little bit.

STUCKLESS: Okay. Last time somebody just asked me if I could get further away from it, like that I have a car.

We have a couple of models that have been put together by the project, mathematical models, that suggest that seepage into drifts should be a very small fraction of the infiltration flux going by the repository horizon. If this is true it should be testable by archeological and geological models or data, and that's what I'm going to focus on today.

We're actually not alone. My organization on this is going to be start with natural systems like caves and go oldest to youngest, and then I'm going to go to some of the anthropogenic systems, and I actually have fought my way through several hundred pages of Spanish on Roman mines. I don't have any good pictures from that. So I touch that very lightly, again going oldest to youngest on the anthropogenics.

It turns out that we're not alone in having developed models to explain flow around openings in the unsaturated zone. The French published on this first in '78 and then again in '84. Their models are very similar to ours in that much of the flow--and this in limestone, so it's a fracture flow very much like the welded tuff; chemically it's different, yes, but it's similar hydrologically.

Much of the flow tends to stay around the outsides of the openings. The French also note that there's a fair amount of flow down along the walls of caves. And so that's something that they didn't quantify it the way we did, but it's their explanation for why the paleolithic art still exists.

To give you an idea of how common these sites are, this concludes together from a number of different sites, but there are literally hundreds of sites in Spain and France that have paleolithic art that goes back 15,000, 20,000, 30,000 years in age.

I will talk specifically a little bit--Lascaux, which is up here, which track back down here--Chauvet, which is over here, Cosquer, which is here, Altamira in Spain, which is here. These sites are not identical to Yucca Mountain. After all nobody buried radioactive waste in any of these.

This is from Chauvet. The cave is up in this block of limestone, a very much wetter climate than we have at Yucca Mountain; someplace to the--rain to the north and south of here measures from 58 to 78 centimeters per year versus 15 centimeters per year at Yucca Mountain. I have some notes that I can't read without glasses.

One of the things that the Berkeley crowd espouses is that the size of the opening makes quite a difference as to what the infiltration flux is actually like. The larger the opening the greater the flux. It's not a linear relationship.

So Chauvet, which I'm going to talk about here, is about 500 meters in length, 10 to 30 meters in width, up to 15 meters high. I was asked at one point what about humidity. It's 99 percent relative humidity, three percent CO_2, average temperature 13.5 degrees C.

In addition to paintings, which I'm going to show you, they found 55 bear skulls well preserved in this cave. How long have they been there? Well these animals--these paintings have been dated at 32,000 years to 30,000 years, dating the charcoal.

This picture is particularly important in that it also shows the effect of water running down the wall and dissolving some of the charcoal and removing it. In other words, these things don't exist because they're insoluble. They exist because they've been kept fairly dry.

There had been some discussion that these things were done recently with old charcoal. The people at Chauvet went in and took oil smudges off the roof; dated those at 26,000 to 28,000 years. And the question that I was asked last time was how do you know that's a good age for the oil smudge. They were made with animal fat, so you don't have a bunch of inherited carbon--dead carbon.

Next one I want to look at shows Cosquer, which is down near Marseilles. It's a fairly wet environment, and in fact your cross section shows that the entrance to the cave is actually 37 meters below sea level today. Back in the glacial maximum sea level was down to 120 to 130 meters from where it is today.

The painted portions are in here. What does not show in your--is that this level is supposed to be the same. Hydrologically it's very difficult to have sea level a little higher than it would be in the caves, since they communicate.

The size of this cave is something like 37 meters by 130--175 meters. Again the humidity, very, very high; CO2 content, not so high; and in the pictures--in the book of this you will also find that you can see the high tide mark because paintings are destroyed up to high tide.

I neglected to mention for your benefit, this is good for the public because all of this stuff is available in everybody's garage and attic. It's all published in National Geographic or in coffee table magazines or books published by Harry Abrams and Company.

Paintings here date to about 17,000. Some of them go back as far as 29,000. It's the only cave I know of which has got two distinct periods of occupation. The blue on this is calcite which has been precipitated over the paintings without removing them. And the mechanism for that is that during the wet seasons the walls bloom. They literally become damp, moist. When the water evaporates the calcite precipitates.

This particular set of images comes off the Internet, which is another fine source of information for anybody who'd like to look it all up.

Altamira in Spain is another fairly damp climate. This is off the top, sitting on top of the cave itself. Within the cave--that's upside down--within the cave there's an area which is called the polychromatic chamber.

These are all painted on the roof.

You can that you've got fractures going all the way through this. This is limestone again, it's a series of limestones and clay stones. The charcoal is apparently totally -- in this case, and there's a group of Spanish hydrologists who have actually worked on this. And here's some of their results.

These are qualitative. I had to read these off a graph. They are sending me the original data. But they measured precip for 22 for months; they measured ET, actually calculated ET; they got a net infiltration; they measured, they collected all the water dripping in the cave, measured that.

We had a figure given here recently about one percent of what--10 percent of what is put on the surface infiltrates. Well here, seven liters per month infiltrate and about 6,000 liters per month is available at the surface. The overburden here is seven to nine meters thick, is all, and almost everything is diverted around this cave, 150 square meters worth.

In addition to art work, there's this clay bison. This is still soft clay; this is at Tuc d'Audoubert in France, in the Pyrenees, thought to be someplace between 14,000 and 15,000 years of age. The only damage that's occurred to this thing is a little bit of desiccation cracking.

At Neo (phonetic), which is very close by, I ran across Monday pictures of footprints in the mud at the bottom of the cave that were made by prehistoric man--still there.

Okay, about 12 years before the discovery of Altamira in Spain, which is about 1860, they began discovering rock paintings in India. These range in age from 2,000 to 10,000 years. My little sticky at the bottom is to remind me that they've recently published a compilation of over 400 sites in India with rock shelters that are painted. And they range in climates fairly drastically.

You go to National Geographic in 1999 you'll find that the largest number of known rock shelter paintings and cave paintings are on the continent of Africa--a very common thing to find these things preserved all over the world in the unsaturated zone. Give you an example from the Sahara, something that is at least qualitatively thought to be more than 4,000 years old, a painting on sandstone in a rock shelter in the Sahara.

Okay, the last of my natural examples comes from a place some of you have heard of. It's called the Sheep Range which is out the window here a little distance. 11,000 to 12,000 years old, this is a packrat midden.

Packrat middens are made of pieces of vegetation that the packrat has brought in, feces cemented with dry urine. It will not take much water to damage this, and these go back to 40,000 in age in this immediate vicinity in rock shelters and caves.

One of my oldest--or youngest, rather, natural systems, this is a cave in Israel. What have I got here--the cave size here is only two meters by two meters by 13.5 meters. Obviously the rainfall is only about 100 millimeters a year, so it's a much dryer environment now than Yucca Mountain. The stuff in here is dated at about 5,000 years old.

There are several carbon 14 dates that you can get from the Israeli Department of Antiquity, but preserved within this cave--by the way, 5,000 years ago is about when we had the dryout in the Middle East, so when that cave--things were put in that cave it was a little wetter than it is today. But there are items made of brass, ivory, that are well preserved there today. In addition cloth--this particular cloth had a skeleton wrapped in it, but the cloth is still in very good shape.

Now then, moving to natural openings, this is from the Tomb of Maketra from about 4,000 years ago--was some sort of a functionary in the pharaoh's court. There are literally hundreds of these little wooden figurines that were buried with him that carry--that are painted and they're perfectly preserved. Now again it's only about 25 millimeters of rain there per year, but still they stayed dry, they stayed preserved.

A couple of chairs from the Tomb of Tutankhamen from 1,400 B.C. These things again, well preserved; in this case t his actually carved out in the limestone. This is not

an above ground tomb, so it's very similar to an underground opening that we might mine out. It's in limestone, fracture flow--little dryer than we have, but absolutely--there are other things that are preserved in there like jugs of wine, loaves of bread, stuff like that there.

Moving on to my closest analog so far to Yucca Mountain, these are the Buddhist temples in India, west central India at Ajanta. They're carved into the Deccan basalts. They started carving these about the second century B.C. and they're all very large.

But here is one of the Buddhist temples from the second century B.C. They plastered the walls of the basalts with a mixture of mud, grasses, ground up rocks--rock dust and calcite. This particular cave is 30.5 by 12.5 meters in its extent, and things have stayed dry enough in there where the paintings have been preserved for 2200 years.

Now you will see spallation effects in all of these, but you don't see running of the colors. So there hasn't been enough water flow even there--the precipitation in that region is 80 centimeters a year, almost all of it in a four-month period. So multiply it by 30 if you really want to know what it would look like on an annualized basis.

SPEAKER: Centimeters--

RUNNELLS: Yeah, would you repeat the figure? I think we didn't hear the figure, the precipitation.

STUCKLESS: Which now?

SPEAKER: The precipitation.

RUNNELLS: The precipitation figure you gave--

STUCKLESS: Precipitation is 80 centimeters a year--87 centimeters per year, sorry. And almost all of it in a four-month period. Okay. This cave is from the sixth century A.D. It's a larger cave, if memory serves--yeah. Where am I--cave 2. This is 14 by 14 meters, there's a shrine in it that's 4 by 3 meters, but it's a large opening and things are very well preserved.

Slightly younger, and a slightly smaller opening is this one. All of these paintings had some minor damage to them. The damage was caused in 1920 to 1922 when the local rajah wanted these things preserved and he brought some Italian artists in who shellacked them. And much of what you see that's spalling off of these is due to the fact that that shellack is peeling.

All right, last summer I went to--or last fall--I went to Cappadocia. This is a perched stream between Derinkyuyu and Kaymakli, two of the underground cities. Derinkyuyu at one point in time had 15,000 to 20,000 people living underground. These are not small openings underground; they're large extensive things.

And there supposedly is a tunnel joining those two underground cities. That stream flows across the top of that tunnel about 80 or 90 meters above it, and the tunnel apparently stays dry.

This is a schematic of what Derinkyuyu looks like, and you can imagine if you had 20,000 people living down here it had to actually have a pretty good ventilation system to keep them from suffocating, and it is ventilated with these large wells that go down around 90 meters in depth.

The size of the underground openings is highly variable, but some of them are very large. This millstone is a meter and a half in diameter, and the opening into this tunnel is a meter and a half or a meter in diameter, which is tough for some people to get through. But that could be rolled across if they were attacked by the Romans.

These were started--they started building these in the second century A.D. and continued occupying this up until the ninth century A.D. There is evidence underground for water. This is my USGS colleague who's Turkish by heritage and did all our translating. He's about six foot tall.

But near the electrical--near some of the electric lights we've grown algae, and so there is some water; but we looked high and low for any evidence of current wetting of the surfaces, any fractures that had any kind of stalagmitic deposits with them, and found nothing.

Also in Cappadocia there is a region, Goreme, where the monks built churches underground from the eleventh through thirteenth century. They're carved into ash flow tuff. I took a piece of this home and gave it to our petrologist on the project, who immediately identified it as Yucca Mountain tuff. I told him it was quite a bit younger.

This is about 4 million year old ash flow tuffs.

Inside that church, the front of which has fallen away, is this fresco in the ceiling. This is a true fresco as opposed to the ones in India, which were painted on plaster and mud. This one happens to be in perfect shape. It was the only one I found like that.

More commonly they looked a bit like this. You can see areas here that have spalled and taken the painting with it, and there are areas in here which have Arabic carved over them, probably a type of damage we won't expect at a mined geologic repository.

At that same location we found a kitchen which had been in use for several hundred years, open fires in it, so everything's coated with soot. I'm not a great photographer, but someplace along in here is a break between the wall and the ceiling.

You can see the fracture coming across the ceiling and the soot is bleached out next to the fracture. There's no evidence of any kind of dripping here. The floor of course has been destroyed by millions of tourists climbing across it.

Where this thing goes down the wall on the diagonal there's obviously been some flow out of the fracture and down the wall, very much like the French models predict.

RUNNELLS: John, could you finish up in say, two minutes or so?

STUCKLESS: Real easy. Terra cotta armies in China--people thought these would be a great analog. They're a good analog if it's backfilled. These basically had the ground above them collapse in on them so that they were buried in soil. They're broken up a little bit; may have been due to vandals.

This is from the second century B.C. There was enough fragments of paint where you can actually go back and reconstruct what these things looked like before they got into a backfill situation and the paint basically has been dissolved off. 100 years later there was another batch of armies buried, and these were anatomically correct soldiers with cloth uniforms and wooden arms. The cloth and wooden arms are now gone.

The last picture is just to remind me that there's all kinds of people living underground in carved out geologic formations. In Cappadocia they still live underground in these carved tuff. In China there are areas where people have lived underground for as much as 2,000 years in loess, and they've carved out homes and then farmed over at the top of their homes for 2,000 years.

In Tunisia there are people who live underground and farm the areas above them. I found nothing hydrologic in these descriptions about how wet it was in their homes, but I can't imagine they'd stay in them very long if there was a lot of water flowing into them.

The conclusions you can read yourself, but in essence it says that things--in openings in the unsaturated zone get preserved remarkably well. On every continent except Antarctica I find examples. I can find them going back for periods of 30,000 to 40,000 years, and my feeling is that this ought to give some confidence to the public that the mathematical models that predict this type of dryness are in fact correct.

And on top of that, I agree with Brian Marshall that the figures being used for TSPA are grossly over-conservative for seepage flux.

RUNNELLS: Thank you very much, John. Very interesting. Question from the Board? Paul Arendt?

CRAIG: Yeah, I want to say--Craig, Board--I found that absolutely fascinating.

RUNNELLS: Oh--

CRAIG: I attempted to go into a half an hour discussion about Anasazi artifacts, but I only observed--you didn't mention those folk--that you can go 200 miles from here up to Blanding and go into Grand Gulch, and you can find overhangs which are sort of like what you would imagine if we were outside and this were an overhang.

And you can walk around and you can find corn cobs and you can find yucca fibers that were used to make sandals, and you can find wall art that has in some cases a striking amount of color on it. And that's all typically 1,000, 1,200 years old, right around here.

And of course there's lot of that kind of thing. So you don't have to go into a big cave. All you have to do is to go into an overhang and it's out there, not to mention all of the jugs and clay objects which are also found in rather gentle overhangs.

STUCKLESS: Almost all those examples in Africa are in rock shelters. All of them in India are in rock shelters. For some reason or another--they do have some limestone caves in India but they have found no painting in those, probably because it required light. I got an awful education in archaeology while I was doing this.

CRAIG: Very interesting.

RUNNELLS: That was Dr. Craig, by the way, not Dr. Arendt. Dick Parizek.

PARIZEK: Parizek, Board. Just a question about flow in the unsaturated zone. Implication is it sort of goes around all of these paintings and these openings and so on. Truly in the carves there, epicar (phonetic) system focuses flow.

You can have segments of caves that have been dry for very long periods of time, and actually would make good repositories, in those cave segments. Nobody would accept that as a suggestion, but caves are caves, and there's a lot of channelized flow around the caves.

Some of these other openings, are you saying again that it's a capillary barrier effect, you think, that's channelizing the flow around it, given the rain amounts that you--

STUCKLESS: That--yeah--not only is it a capillary flow barrier that's basically taking it around the half cave, if you like--which is what a rock shelter sort of is--you'll find articles written that basically say most of the stuff that's being destroyed in these rock shelters is being destroyed by wind oblation, not the effects of water.

PARIZEK: But if you were to go back into that ledge some distance would there not be pathways with water?

STUCKLESS: Oh, there may very well--

PARIZEK: So I mean--so what you see preserved is what happens to be dry for those times--

STUCKLESS: Obviously I didn't have time to give you everything that I've read in the last year, but within the Indian examples of the shelters, where banyan roots have come down along the edge of the shelter and provided a preferential pathway for water, the paintings are dissolved.

So in essence there's got to be something there that will channelize water across the painting where it will be preserved, basically the Indian archaeologists have concluded.

PARIZEK: Yeah, but then let's go back to the mesa, which was suggested as a place to go look at the in modern process. In the brief portion of the test site visit that the Board had, we had drips and we had water leaking off the ceiling and on the sides of walls of one short section of a tunnel that we visited.

So again if you go to the right places you can also make the other argument, that these damp places are wet and it's not always--there's focus flow, but there's also drips or seepage. It seems to me yet you've got to balance it with those other observations, and the program has been encouraged to look at that.

I know Dr. Simmons has been anxious to do something at the mesa, but I guess you have no money to go in the tunnels. You only have to go with the documentation of what was described before, but seems to me it's such a critical observation, and if it meant ventilating a piece or going in there in space suits for 1,000 feet or more, you could make a lot of observations and argue the other point. That's relevant to maybe the Yucca Mountain case, because that's at a higher elevation, slightly different rainfall amount.

So I think you ought to pair these two concepts, the dryness--I mean we've been to the caves, we've been to the--lot of these interesting places, and I agree with you--that lots of stuff preserved a long time in cave segments are great repositories. We have limestone caves storing records, you know, and mines that are dry, places you think would be wet. So there are these special situations, but we want to make sure we don't get fooled because of the special nature of these rocks with the wet--wet conditions that we would see--

STUCKLESS: --looked at a whole--

PARIZEK: --test site.

STUCKLESS: --spectrum of rocks from sandstones to shales to limestones to basalts to rhyolite ash flow tuffs. And a spectrum of climates. Obviously doing a literature search the archaeologists don't show you what's been destroyed, okay.

So--but Cappadocia, I went through carefully; Altamira. I know what those things look like. I don't know what the Buddhist temples actually look like in toto. Pretty spectacular.

RUNNELLS: Question from Jerry Cohon.

COHON: Do you think that the program's plans will take maximum advantage of what's out there in terms of natural and human produced analogs?

STUCKLESS: That's kind of a loaded question. I'm fortunately not one of the program planners, so I will defer that to one of the program planners.

RUNNELLS: And one closing question from Alberto Sagüés.

SAGÜÉS: Okay, it was a great presentation. I enjoyed it very much. A couple of observations perhaps, and one of them is to repeat what you said, at least that by definition the artifacts and the art work that you see is the one that survived. Of course whenever something didn't survive you didn't see it.

But in those places with human habitation over long periods of time, wouldn't that imply some kind of air renewal and therefore some sort of ventilation? And wouldn't that be different from a very close chamber kind of environment like could be occurring in the drifts in the repository? Wouldn't that make a big difference?

STUCKLESS: I don't know how much of a difference it would make. I would argue that the ventilation that we're seeing--I think Parvis Montazar, if he's around here, has been arguing forever on the behalf of Nye County, that one should ventilate this and it will stay much drier.

Certainly all the examples I looked at are ventilated, and in the case of Cappadocia, intentionally ventilated. So the analogs are not perfect. But if they go to a ventilated system they're darn close.

RUNNELLS: With that we'll close the session. Thank you very much, John. We appreciate that very interesting presentation. We will reconvene in 15 minutes, at 3:45.

(Whereupon a brief recess was taken.)

RUNNELLS: We have to move on in order to stay on schedule. We don't want to cut anybody short on their time. Our next speaker is Dr. Robert Bodnar. Dr. Bodnar has a Ph.D from Penn State University; another one of those Pennsylvania guys. His is a university distinguished professor and a C. C. Garvin professor in the department of geological sciences of Virginia Tech University.

His research focuses on the study of fluid inclusions. Today he's going to talk to us about fluid inclusions. I would like to however offer my deepest condolences to Dr. Bodnar for a double catastrophe this year during the collegiate football season--Penn State collapsing at the end of the season and Virginia Tech putting on a great effort but falling a bit short. Dr. Bodnar.

BODNAR: Thank you. At least we made it there.

SPEAKER: Absolutely.

SPEAKER: Good comeback.

RUNNELLS: And more importantly you belong there.

BODNAR: It used to be that when I would go and give a talk I would have to explain to people where Virginia Tech is. After January 4 I no longer have to do that.

I want to thank the Board for inviting me to come here and talk about fluid inclusions. Before I start, let me--hope this works--before I start let me just explain very quickly why we're interested in fluid inclusions at Yucca Mountain.

It's been proposed that there has been episodic introduction of hot ascending fluids into the repository horizon, and if this has happened episodically in the past that it might happen in the future. And fluid inclusions are one way of understanding the extent to which heated fluids may have interacted with the rocks at the repository horizon, and maybe gain some insights that will allow us to predict if this is likely to happen again in the future.

Let me also say that fluid inclusions provide very precise, very accurate quantitative results. And this is both an advantage and a disadvantage. Of course the advantage is that fluid inclusions can provide very accurate data, but the disadvantage of that is many people then use these data and make interpretations which by implication are also very accurate and very precise; and in many cases that's not true.

And what we'll look at today are some of the capabilities and limitations of fluid inclusions. And what I'll do is talk about what fluid inclusions are. I'll say a little bit about some of the information that they can give us, and then very briefly talk about some of the information that we can't get very easily from fluid inclusions.

And just to give you an idea of what we're talking about, this is a fluid inclusion. That fluid inclusion is approximately 20 microns in diameter, so this is the fluid inclusion here: it's this feature, and it contains two phases. In this case it contains a liquid phase and a vapor phase, and I'll tell you in a second how those phases come about.

This particular fluid inclusion is contained in the mineral quartz. This is not from Yucca Mountain, by the way. And we're looking at this under a microscope in a thin section of rock, looking at it at very high magnification.

Okay, so what are fluid inclusions? When minerals form by precipitation from an aqueous solution, some of that solution can be trapped in the mineral as a defect as the mineral precipitates and grows. These microscopic droplets of fluid are called fluid inclusions.

Also if a fracture develops in the mineral sometime after it forms, fluid might enter that fracture, and then as the fracture heals by later crystal precipitation, fluid inclusions can be trapped along these fractures. And let me just show you in this next slide, which is a schematic that will illustrate what I'm talking about here.

So if we have a--imagine this is a mineral growing here into some fluid phase, say a fracture, a lithophysal cavity, and we might trap some fluid in a defect here and end up with a fluid inclusion. If we look at this growing mineral surface, if we look at it at the microscopic scale, that mineral surface is often very irregular. It's not a nice, smooth surface.

And fluid enters some of these depressions, these irregularities, and then as the mineral continues to grow over that irregularity it traps some fluid, and results in fluid inclusions when we look at that mineral out--when we look at that mineral under the microscope.

And so we might have several different generations of mineral precipitation during each of these episodes trapping fluid inclusions. Those types of fluid inclusions we would refer to as primary fluid inclusions, trapped during the growth of that mineral.

Now as a result of some thermal perturbation or perhaps seismic activity we might fracture the mineral during its growth, and fluid will enter this fracture. So if we look over here, here we have a fracture, fluid enters that fracture and traps some of that fluid as fluid inclusions as the mineral continues to grow.

So these inclusions here that would be trapped along a fracture during the growth of the mineral, we would refer to as pseudo-secondary inclusions. They were not trapped when that mineral was actually precipitated, but they were trapped at some later time along a fracture, but still during the growth of the general crystal.

And then sometime long after the whole crystal has formed we might have a fracture that forms that goes through the whole crystal, and fluid could enter that fracture and form secondary fluid inclusions.

Of course these would be fluid inclusions that would not be associated at all with the formation of that crystal, but they would tell us something about the type of fluid and perhaps the temperature that existed at this location sometime after that crystal formed.

And here are some examples. This particular mineral is a pyroxene. Again I won't be showing you many examples from Yucca Mountain because I don't actually work on Yucca Mountain. And you can see, these are all fluid inclusions here, outlining former growth phases in this pyroxene crystal. So these would all be primary fluid inclusions trapped along these growth zones.

So obviously these fluid inclusions are older than the fluid inclusions along this growth zone, and likewise these here are then younger, and then progressively younger as we go out. So by looking at the characteristics of these fluid inclusions along these different growth zones we can map out how the fluid has changed with time.

Here's another example. This is a calcite crystal from a petroleum environment. Here is a little droplet of oil that adhered to this crystal surface when this was a free crystal surface growing out into a fracture, and then as the calcite continued to precipitate it trapped that little droplet of oil as a fluid inclusion.

And again, if we form fractures in the crystal during growth we can trap secondary fluid inclusions. Here are some examples. These trails--all of these--each one of these little tiny black specks in here is a fluid inclusion going through, cutting across these minerals.

So these would have been fractures that formed after these quartz crystals formed, fluid entered those fractures, and then as the fractures healed they formed secondary fluid inclusions. Again--here's--again just planes of what we call secondary fluid inclusion representing some fluid that flowed through that rock after its formation.

Okay, what are some of the data that we can get from fluid inclusions? Temperature; pressure; and I put depth here in paren--or with a question mark, and you'll see why in a minute; fluid composition, and sometimes from the fluid composition we can infer the source of the fluid; and then fluid timing, in other words what are the different types of fluids that were in the rock and how did the fluid composition change with time.

But let's take a look first at how we get temperature. Now when we trap a fluid inclusion we assume that the fluid inclusion traps just a single fluid phase. So here's a large fluid inclusion, up at high temperature now, a couple of hundred degrees, and it's filled with liquid. It's filled with liquid, an aqueous solution at 200 degrees.

As that fluid cools, as that rock gets uplifted to the surface it nucleates a little vapor bubble, and as we continue to cool that mineral that vapor bubble gets smaller and smaller until--larger and larger until we look at that fluid inclusion today under the microscope at room temperature and it contains a liquid phase and a vapor bubble.

The reason that we generate a vapor phase in the fluid inclusion is that the host mineral, the bottle if you will, that the inclusion is contained in, is constant volume. Its volume doesn't change very much as we heat it and cool it, because the coefficient of thermal expansion for minerals is fairly small compared to fluids.

The fluid, however, when we cool it its density increases, or its volume decreases, and so it generates this vapor phase in the fluid inclusion. So what we do in the laboratory is we take this fluid inclusion and we reverse the process. We heat the fluid inclusion up.

While we're watching it under the microscope the bubble gets smaller and smaller until it disappears, and we measure that temperature under the microscope as we're heating it up, and then that temperature--which is referred to as a temperature of homogenization--is a minimum temperature for the formation of the mineral containing that fluid inclusion.

Now I should point out here that the temperature that we measure is a minimum temperature, and without going into the details, this is a temperature pressure diagram for --in this case--a water phase containing 20 weight percent NaCl, and what I want to point out is that any fluid inclusion trapped along this line, any fluid inclusion with a 20 weight percent composition trapped along this line, will homogenize at 100 degrees. We call this line an isocore or a line of constant volume.

And again, it's related to the fact that the bottle or the mineral that the fluid inclusion is contained is doesn't change it's volume as we heat it. So in fact we could have a fluid inclusion that was trapped up at 200 or 300 degrees and it would homogenize down here at 100 degrees if the pressure was high enough.

Now for Yucca Mountain we don't have to worry about this too much, because at Yucca Mountain the pressure was relatively low, a few bars to perhaps a few tens of bars. So what that means is that the temperatures that we get for homogenization temperatures of fluid inclusions at Yucca Mountain are very close to the real trapping temperatures for those fluid inclusions.

Now the other piece of information that we can get from a fluid inclusion is the pressure at trapping or at formation. However, as geologists what we really want to know is not so much what the pressure was, but what was the depth? And it's not easy to convert the pressure into a depth. This shows several models for how we can get different pressures at the same depth, but let's just use a simple example.

Let's imagine that I had a cardboard box here about this high, and if I had that cardboard box just filled with air and sitting on a scale, a balance, it would weigh some small amount. If we filled that box with water it would weigh more. If we took that water out and filled it with rocks it would weigh even more.

And so we can--that's the concept that depending on what is above that fluid inclusion, above that mineral when it forms, we can get very different pressures at the place where it formed.

So here's a place where we're forming a fluid inclusion and the fracture is filled with water up to the surface. Here's a case over here where we're forming a fluid inclusion, we have water for some depth in the fracture and then vapor or air for some depth above that. So obviously even though these two fluid inclusions are forming at the same depth, they would have different pressures.

And this is actually very relevant to Yucca Mountain because we may have a situation something like this, where we have partially water filled fractures or--that are open to the surface with air. And so we have to be careful in terms of converting a pressure to a depth.

Now fortunately again at Yucca Mountain the current depths in the mountain are probably very close, if not identical, to the depths when the minerals formed, so we can actually use the present day depth as we work with our pressures.

Okay, now composition--composition of fluid inclusions is a very important piece of information because it can tell us something about the source of the fluid, but we're faced with this problem, that we're generally working with very small amounts of fluid. A 10 micron fluid inclusion, which would be a typical fluid inclusion, contains 5 times 10 to the minus 10 grams of solution.

To put this another way, it would take two billion --that's billion with a B--two billion of these fluid inclusions to fill up a thimble, about one cubic centimeter. So we're talking about very, very small amounts of fluid--not easy to work with.

There are some techniques that we can use. The one that we use very commonly is to freeze the fluid inclusion, and the idea here is that if we freeze pure water it freezes or melts at zero degrees. If we add salt to that water we depress that freezing temperature and so we know that if we add a certain amount of salt, instead of the water melting at zero degrees it'll melt at some lower temperature.

So what we do is we take a fluid inclusion, cool it down until we freeze it, so here now it contains ice. You can see how the vapor bubble has been distorted. And we begin to heat it up, and at this point it starts to melt. You can start to see this granular texture. This temperature here tells us something about what salts are in the fluid inclusion.

Now you can see that we start to form some nice, discrete ice crystals, so this is water ice in a liquid phase. And we just continue to heat it, watching until this last tiny little ice crystal melts. We measure that temperature and we can refer that temperature then to experimental data for the depression of the freezing point and convert that into a salinity.

And of course this is relevant to Yucca Mountain because if we have pure water in the fluid inclusions at Yucca Mountain, that might tell us something different than if we had five or 10 weight percent NaCl or salt solutions in those fluid inclusions, relative to whether the fluids originated on the surface or originated at depth.

AT Yucca Mountain--now these actually are inclusions from Yucca Mountain, and I'd like to thank Yuri Dublianski for letting me borrow this slide. There are some all gas inclusions that have been recognized at Yucca Mountain, and these are two. And they don't contain any visible liquid. They just appear to contain vapor or gas.

And I think that these are probably critical to understanding the origin of the fluids at Yucca Mountain. If these turn out to be air, that has different implications concerning the origin of the fluids than if those gas inclusions contain methane or CO2 or some other gas that we might be expecting to come up from depth in hydrothermal fluids. So I think that these might be important to study to try to understand the origin of some of the fluids.

A technique that we use in my laboratory to analyze gas inclusions is Raman Spectroscopy. This is-- what we're looking at here is a microscope with a green laser coming down through it, and we can put the mineral specimen here under that microscope and zap it with an argon ion laser. That gives off a signal, a characteristic signal that we can detect and use that to tell which gases are in the fluid inclusion. So we can identify things like nitrogen and methane and carbon dioxide and other gases that might be indicators of the source of those fluids.

Getting back to this diagram, I put this up here to remind me to tell you that one of the things we can get from the fluid inclusions is the relative age of the fluids. Again, obviously primary inclusions trapped along this growth zone would have been earlier than primary fluid inclusions trapped along this growth zone.

So we can look at the relative ages of the fluids, and obviously fluid inclusions trapped along this fracture would be later than any of the primary fluid inclusions trapped anywhere in that crystal. So fluid inclusions give us a good handle on relative ages of fluids.

Now that leads me into what we can't get from fluid inclusions. And the one piece of information that we would dearly love to have for Yucca Mountain, because it would answer a lot of the unanswered questions, is the absolute age of those fluid inclusions, especially if we find fluid inclusions that indicate high temperature.

We want to know, are those fluid inclusions nine or 10 million years old and perhaps associated with the original volcanic event, or are they are few hundred thousand years old, in which case they have important implications for the safety of the repository.

The absolute age is something that's very difficult to get, and generally what we do is we try to determine the age of the host mineral that is adjacent to that fluid inclusion. But there are a lot of uncertainties associated with that, and sometimes it works and sometimes it doesn't.

And then the other piece of information which would also be very beneficial, very useful in terms of understanding whether the fluids were coming from depth and rising up, or percolating down, obviously is the source of the fluid which we might be able to get from compositional analyses in some case.

But again, because of the small size of the fluid inclusions we're limited in terms of our ability to determine the source, and even if we can determine the composition of the fluid inclusions many times that composition is equivocal. It could be interpreted either way as being of a deep source or of a surface source. It's not definite that it's one or the other. So it really doesn't answer our question.

Okay, so the question related to Yucca Mountain then is what's the probability that heated ascending fluids will reach the repository horizon in the future. This is one of the questions that we're trying to get at with fluid inclusions.

In geology there's a concept, a theory, called Uniformitarianism which says the present is the key to the past. And what that means is that we assume that processes that are working on the earth today, plate tectonics and volcanism and erosion and things like that, those processes that are working today also operated in the past.

So if we study present day systems we can extrapolate those back into the past to try to understand what happened on earth at some time in the geological past. Well I've turned this around here, and what I'm saying is the past is the key to the future.

If we can understand what went on at Yucca Mountain over the last 10 million years in terms of fluids and the thermal history, if we can understand that, that may then help us to understand what's going to happen in the future at Yucca Mountain.

Here's Yucca Mountain today, and of course many of you are familiar with this. Here's how Yucca Mountain formed, according to the propaganda that's underground at Yucca Mountain--I think this is from underground--yeah, it is--obviously a very explosive volcanic event.

So we know that the thermal history, the physical environment at Yucca Mountain has changed from the time that it originally formed until today when it's a very quiet, peaceful place. What we want to try to understand is how things changed during that 10 or 12 million years.

And some of the questions that we have, have fluids moved through Yucca Mountain in the past? What was the temperature of the fluids, and what was the source of the fluids, if there were fluids moving through there? And perhaps the most important question, when did that fluid migration occur at Yucca Mountain?

So I'm going to tell you right now that I don't have the answer to any of these. I'm going to defer those answers to Dr. Cline, who is going to follow.

But these are the questions that I think we have to answer if we want to try to understand if there's been hydrothermal activity in the past at Yucca Mountain: how episodic has that been or how common has that been; when did it occur; specifically did it occur very recently; and what is the likelihood that that could happen in the future.

I'll just finish up here. These are some of the features that of course led to the initial hypothesis that there may have been hydrothermal activity at Yucca Mountain. Many people interpret these to be the result of down-moving fluids or descending fluids. Some have interpreted these to be the result of upwelling fluids.

And again I acknowledge Yuri Dublianski for the loan of this slide and the next one, showing some of the various occurrences of calcite in the ESF in different fractures and lithophysal cavities. It's pretty clear that there were fluids there that deposited those minerals. The question is when were those minerals deposited and what was the extent of fluid activity.

And I'll finish up with this slide and the application of fluid inclusions to Yucca Mountain. What I've put on here, this is my opinion, my biased opinion, in terms of the confidence level that we can use to determine these various pieces of information that we would like to have.

And I think that we can determine the temperature of formation of the fluid inclusions and the relative age of the fluid inclusions in the calcite and the other secondary minerals at Yucca Mountain with a high degree of confidence. We can get the fluid composition and pressure, not as well perhaps as we would like to, but probably well enough to understand the source of the fluids.

Depth and source of the fluids, this probably should be moved up, because we really do know the depth since the depth is the present day according to all erosion models. Of course source of the fluids, I think we're going to have a hard time determining that. The results so far that I've seen appear to be equivocal. There's nothing diagnostic that we could point to and say yes, that had to be from the surface or that had to be from depth.

And then of course the absolute age of the inclusions, and I think that many of the people working on fluid inclusions at Yucca Mountain recognize that this is something critical to determine. I think everybody recognizes how difficult that will be, but everyone also recognizes that if we're able to do that, that this then can provide the answer to many of the questions we have about past hydrothermal activity at Yucca Mountain and the probability for future hydrothermal activity.

And with that, I'll stop. Thank you.

RUNNELLS: Thank you very much, Bob. That's very informative. We have time for questions from the Board or from the staff. Yes, Jerry Cohon.

COHON: You talk about relative age. Relative to what?

BODNAR: Relative to each other, so if we have two--we use the term fluid inclusion assemblage, and a fluid inclusion assemblage represents a group of fluid inclusions that were all trapped at the same time. We determine that based on petrography.

In other words if all of the fluid inclusions are along a growth zone we assume that all of those fluid inclusions were trapped at the same time. Or if all of the fluid inclusions are along a fracture, we assume that all the inclusions along that fracture were formed at the same time, from a geological perspective.

And so when I say relative timing, what I mean is o ne fluid inclusion assemblage, the age of that fluid inclusion assemblage, relative to some other fluid inclusion assemblage. We can say that this one is earlier or this one is earlier, so in a relative sense we know their ages but we don't know in an absolute sense whether that age is 100,000 years or one million years or 10 million years.

COHON: Just to follow up, you talked in the earlier part of your presentation about using dating of the host mineral as a way to get the absolute age. Does Yucca Mountain present particular problems in that regard or is that just the problem everywhere?

BODNAR: It's a problem everywhere, and the reason it's a problem is that I showed some idealized sketches with nice primary growth zones, and I showed you classic examples of minerals showing growth zones.

In reality I would say that 99 plus percent of all the minerals that you look at don't show those. Instead they just show a mish-mash, a random distribution of fluid inclusions, and it's very hard to determine that the fluid inclusion that you're looking at was trapped at the--was trapped when the mineral that's adjacent to it precipitated.

In other words you have a fluid inclusion. Maybe that fluid inclusion was trapped when that mineral grew there, but it could have been trapped at some time long after that, perhaps along a fracture, and we can't identify it as a fracture as such because there are so many fluid inclusions that the fracture behavior just disappears and we just see this large number of fluid inclusions that don't appear to have any constraints. They're not constrained to growth zones, they're not constrained to fractures.

So it's a problem in general with fluid inclusions. It's perhaps a little bit more of a problem at Yucca Mountain simply because we have often less mineral to work with, which means you have less opportunity to look around and find good examples of where you can say yes, this fluid inclusion was definitely trapped at the same time as the mineral that's adjacent to it.

RUNNELLS: Priscilla Nelson.

NELSON: Nelson, Board. I'm aware of some fluid inclusions that you can actually see, that there might have been a gradient, be it pressure or temperature or something that actually caused a movement, maybe solution precipitation, some sense of moving of a fluid inclusion after it's been formed in a mineral.

BODNAR: Movement after the fluid inclusion was formed?

NELSON: Yeah. Maybe some of it in salt. But in cases where there is a thermal gradient where you might actually have such a thing happen--but these are so small you wouldn't expect them to show that in Yucca Mountain, is that true?

BODNAR: Well I don't think it's the size that's a limiting factor. And you're right, that in halite--in halite you can actually watch the fluid inclusions migrate through the salt if you subject it to a thermal gradient. It's simply because salt has such a high solubility in the aqueous solution that it can do that.

For any of the minerals that are being considered at Yucca Mountain, calcite, quartz, perhaps fluorite and barite, the solubilities of those minerals are so low at temperatures less than 100 degrees that even over geological periods of time, if they were exposed to a gradient, the amount of migration would not be detectable.

So I don't think it's a problem for Yucca Mountain.

RUNNELLS: A question from Leon Reiter of the staff.

REITER: Leon Reiter, staff. Bob, I don't know if you can answer this question or Jean can, but then given all these limitations what's the strategy for getting meaningful answers out of the study?

BODNAR: Well maybe I should--maybe we should let Jean make her presentation. I want to point out the problems, but I don't want you to take that as it's impossible to get the answer. It's just that we have to be careful, and we have to be careful not to overinterpret the data.

And I think that everybody who's involved now and is working on this fluid inclusion project, I think is aware of these problems. So I don't think that those problems will be overlooked during the course of this study.

I mean I think that going into the project, I think everybody--and maybe I'm speaking out of turn here--but I think everybody understood in the back of their mind that there was the possibility that after some period of time, doing very careful, very high quality scientific work, that we still might not have an answer. Sometimes science works like that, that you just can't solve the problem using the technology that's available.

RUNNELLS: Any other questions from the Board? Yes, Alberto Sagüés.

SAGÜÉS: Yes, what other techniques, independent techniques would be there to corroborate the results of, for example, your temperature estimates? They give you a sample, you look at the bubbles, and do the test and you say okay, this formed at, for example, 85 degrees Centigrade. But is there something else that you can do with the sample that would give you -- information, maybe not as precise?

BODNAR: Yes, of course. And I think that the USGS has done a lot of this by comparing fluid inclusion temperatures with stable isotopic temperatures.

And based on the partition coefficients, which are temperature dependent, you can make an estimate of the formation temperature of the calcite from the isotopic composition. So there--there's that approach.

There are also mineral geothermometers, but I don't know that there are any of those that are really relevant and applicable at Yucca Mountain. Maybe some of the others of you who are working more on this could comment, but I don't think there are really any mineral geothermometers.

Joe, do you know of any? So I think isotopes would probably be the best technique, and it does seem to work. Again there's always the problem of, you know, which fluid inclusions were trapped at the same time as that mineral that's being analyzed.

RUNNELLS: Any other questions from the Board or from the staff?

Let me ask a question, Bob. I think you probably answered it in answering Jerry Cohon's question, but if the issue--if one of the issues is whether the fluids were moving up those veins, those fractures, or the fluids were moving down those fractures, is there anything in the shape of the fluid inclusions or the shape of the crystals that would tell you, oriented relative to the wall of the fracture, would you tell you whether the fluids were going that way or that way?

I mean have you seen examples where they grow longer down--down gradient, down the flow direction?

BODNAR: I have seen evidence, not at Yucca Mountain, but I have seen evidence in other places where we can determine direction of fluid flow. And in fact the example that I showed early on with the petroleum fluid inclusion, that's from the Monterey formation in California. And there the oil inclusions all occur on one face, on one side. They don't occur on the other side.

And the people that--this is when I worked at Chevron--and the people at Chevron who worked on flow modeling said, you know, that showed that the fluids were moving, I guess it was from the direction where the oil droplets were.

It was--the oil droplets were on the down flow side, so they were coming over the top and kind of settling out on tops of crystals. And so in that case we could get a sense of flow direction. Yucca--I guess I don't know enough about that to really say if we can do it at Yucca Mountain.

But let me just add a caution that at a given place where the fluid inclusion is forming, maybe it isn't so important whether the fluid is moving up or down, because I could imagine a scenario where we have a fluid that comes up and then moves back down the walls.

And so whether it's moving up or down at that particular place might not tell us anything about the actual source of that fluid, whether the source was there or the source was up here.

RUNNELLS: As I understand the issue though at Yucca Mountain, in these particular features that you showed in that trench, it's a question of fluids coming up those fractures and then flowing down the hillside.

BODNAR: That's correct.

RUNNELLS: Anyway, it's something that perhaps--

BODNAR: Yeah--

RUNNELLS: --somebody can look at the textures.

BODNAR: Yeah, now I don't know if anybody has found fluid inclusions in that trench 14--

RUNNELLS: Okay.

BODNAR: --or any of those surface--let's just call them surface deposits. Joe, do you know? Does any--

SPEAKER: Not that I'm aware of.

BODNAR: I don't think anybody has seen fluid inclusions in that material, because it's really fine grains and dark and not really amenable to fluid inclusion.

RUNNELLS: I think that's the answer to my question right there.

BODNAR: Thank you.

RUNNELLS: Thank you, Bob. Any other questions from the Board or staff? Okay, well thank--oh, I'm sorry, Dick Parizek.

PARIZEK: Parizek, Board. Can you tell whether it's saturated or unsaturated if you inclusions -- that?

BODNAR: Are you going to address that? Vadose zone versus phreatic. We've talked about that a lot, and can I mention--

CLINE: Sure.

BODNAR: We actually had--one of the meetings we had out here in November, we had--Jean invited Professor Goldstein from the University of Kansas, who's a real expert in vadose phreatic zone fluid flow. He works on fluid inclusions, and that's his specialty. And we invited him out.

And he pointed out a lot of textures that we could look at in the rocks which combined with the fluid inclusion could help to say something about whether it was saturated, unsaturated. And the project now, the UNLV project, is applying those tools and those techniques to the samples, and starting to see a lot of textures that are indicative one way or the other.

And it's probably not fair for me to talk about that because it's not my work. But yes, they are seeing textures that are starting to be able to distinguish between saturated and unsaturated zone trapping; textures that have been used by people in the petroleum industry and people studying shallow surface deposits have developed over the years. And many of those I think are applicable to Yucca Mountain.

RUNNELLS: Okay, well thank you again. I think we'd better close and move on to the next speaker.

The next speaker is Dr. Jean Cline. She received her Ph.D in geochemistry, also from--well not also--but from Virginia Tech University, where she worked with Professor Bodnar. In other words she is also a Hokie, and we also must offer our condolences to Jean.

She presently is an associate professor at the University of Nevada Las Vegas where her primary research interest is fluid inclusion. And her talk will be focused more directly upon the studies at Yucca Mountain. Jean?

CLINE: Thank you. I'd like to thank the Board for the opportunity to present some of the preliminary information from this project. I understand that this project actually came about a result of the Nuclear Waste Technical Review Board recommending to DOE that they consider funding such a project.

And what I'd like to do today is outline the major goals of the project. I'll tell you about the preliminary work that we have done, I'll provide you with some observations that we have made to date, and then I'll talk about some of the work that we will continue to do over the next year.

I think most of you know that this is a two-year project. We actually began work on the project in April of 1999, and work will continue until spring of 2001. I'd like to briefly tell you about the people that are working with me on this project. Nick Wilson is a post-doctorate fellow who received his Ph.D from Dalhasie (phonetic) University in Halifax.

I asked Nick to join this project. I selected him from a number of applicants based primarily on a great deal of expertise that he gained during his Ph studies in doing some very detailed petrographic work. I thought that this was really the most critically important aspect.

It was essential that the person who ended up working on this project with me fully--first of all was willing to spend a lot of time looking down a microscope, and secondly really recognized how incredibly important it was to make those observations.

Sarah Lundberg has joined the project. She is our electron microprobe technician. Sarah recently received a masters degree from New Mexico Institute of Lines and Geology in Socorro. She spent a couple years there working on a microprobe at that university.

And the third person on the project working with me is Joel Rodert. Joel is a graduate student at UNLV. Joel was very involved in the sampling that was done, our sampling program early on, and he continues to be involved in data gathering and data manipulation.

When I was constructing the proposal for this project I came up with what I thought were the foremost important questions that we needed to address and to try to answer in this project. First of all, do populations of fluid inclusions that indicate the recent influx of thermal waters into the repository site actually exist.

Secondly, if these inclusions are present, what temperatures do they tell us. If these inclusions are present when were these inclusions trapped? In other words when did this thermal influx take place? And then finally, if an influx did occur, how widespread within the repository site was this influx?

What I've done is divide the project in to five different phases, and I'd like to describe these two you. These phases are phases which the rock samples that we have collected can move through individually, so multiple phases are actually going on at the same time with different samples. So we don't just complete Phase I and then move on to Phase II and so on.

Phase I involves first of all collecting approximately 200 samples from throughout the ESF and the ECRB cross drift. We then needed to have polished sections prepared from each of these samples, and we began the search for two phase fluid inclusions with consistent liquid vapor bubbles.

Phase II is really the critically important part of this project, I believe. I can't overemphasize this enough. And it involves doing a very detailed characterization of each of the sections from each of our samples. And our goal here is to produce a time map for each of our sections that documents the progressive growth of the calcite and the other minerals in these samples.

We simply cannot constrain the timing of the fluid inclusions unless we first constrain the timing of the minerals in which these inclusions occur. So this is a critically important part of this study.

Phase II then involves continued characterization of the fluid inclusions, more detailed work, locating all of the two phase fluid inclusion assemblages, determining inclusion origins--are these inclusions primary or are they secondary, and then determining the relative ages of the assemblages based on their origins and locations within the section time maps, something that Bob referred to previously.

Phase III involves the fluid inclusion part of the study. Principally what we will be doing is conducting microthermometric studies to determine the minimum trapping temperatures and also to determine the salinity of the fluid inclusion assemblages.

We will also do some crushing studies. These are studies that are done in an effort to get at pressure of trapping. These are more difficult to do, and we may or may not be able to actually accomplish this. We also will brainstorm, see what other ideas we can come up with to do other sorts of analytical studies to try to identify inclusion fluid compositions.

Phase IV is the geochronology portion of the study, and what we will do really as we're moving through the rest of the study is to try to select samples for geochronological studies that will provide maximum and minimum ages for the primary two phase fluid inclusion assemblages.

The best we can do with secondary fluid inclusions, because they simply crosscut the mineral and are younger than the mineral itself, is to determine maximum ages for secondary fluid inclusion assemblages. And I'll explain this in a bit more detail in a little while.

We will prioritize our samples based on inclusion origin. We can constrain the primary inclusions probably better than we can the secondary inclusions. And also on inclusion location in the younger portion of the samples we recognize that it's the young ages that we're most concerned about.

So we will be looking in the younger mineral bands, and this gets back to doing this petrographic study early on. We need to be able to identify the relative ages of the mineralogic bands within these samples.

Then we hope to integrate uranium lead and uranium series dates with the other observations that we've made with stable isotope data, with petrograph, with trace element chemistry, cat. illuminescence, to further constrain inclusion ages.

When I began constructing this proposal I recognized that this particular issue is a very controversial issue. And so I thought it was worthwhile to make an effort to try to maintain communication with interested parties during the progress of this project, to try to keep interested people up to speed on what we were doing, with a goal that when the project is concluded that there is a broader understanding of what we've done, a broader understanding of the data that's been collected, and understanding of how that data was collected and perhaps a broader appreciation of some of our conclusions.

So with that goal in mind, what we are doing is holding approximately quarterly meetings. And the UNLV group is meeting with scientists that represent DOE and the State of Nevada as well as an independent expert, who is Dr. Bodnar.

And during these meetings we basically get together in my lab, we look at samples, we look at thin sections, we look at data. We will collect data together, fluid inclusion data, probably microprobe data. We discuss hypotheses, we discuss observations, interpretations; we argue about things; and we--our goal really is to, as we conduct this project, to maintain a consensus at each step during the study.

If we can continue to do this, then when the project is completed we should all be well aware of the strengths and the weaknesses of the data, and there should be some agreement.

Okay, next what I'd like to do is focus in on what we've done to date. This I'm sure you recognize as a map of the ESF and the ECRB. The numbers are not important, but they are the location numbers within the tunnels, and these numbers represent our sample locations.

Our sampling strategy was really to collect approximately 200 samples and to collect samples of every type of calcite, every type of mineralization that we observed within the tunnel. And you can see that we have a pretty good sampling density.

There are a couple areas where samples are a bit sparse. There either is no secondary mineralization in those localities or those localities are shotcreted and the walls are not available for sampling.

The color code here is based on the type of calcite that was collected. The black numbers represent calcite and secondary minerals that were collected from lithophysal cavities. The red--actually is--yeah, red color coded samples were collected from fractures, and blue color coded samples were collected from breccias.

I should point out--you're probably aware of this--we're showing the ECRB here. It actually exists right here. You can see that there is some stratigraphic and some structural control to our sampling. For example lithophysal cavity samples are quite concentrated here as well as throughout the ECRB.

This is simply where the secondary mineralization was in that area. If we look down here at the intensely fractured zone you see no lithophysal cavity samples, but fracture and breccia samples.

Okay, as I said, the next step was to have polished sections made from each of these samples. One of the two bottlenecks that we've run into on this project is getting sections prepared. This is a fairly involved procedure and needs to be carefully temperature controlled.

But I'd like to show you what two of those sections look like in general. This is a blowup of a polished section. The scale across the bottom here is about 4-1/2 centimeters, and this probably one of the more complex samples which we've collected.

What we see when we look at these more complicated samples are bands of mineral growth. Principally what we have is calcite, but there are also silica minerals present. And in looking at a number of these more complex samples, we've been able to put together a crude stratigraphy which follows through in at least some of the samples.

And that stratigraphy consists of calcite mineralization at the base, then bands of some silica minerals, calcedne, opal and quartz. Overgrowing those bands would be another zone of calcite, and then this outermost band is a very clear calcite which is generally accompanied by some clear opal bands.

I should say that all of our sections were cut parallel to the growth of the sample. Okay, so this would be the base of the sample that was collected from the lithophysal cavity. What you see down here are remnants of tuffs, and in a general way this sample grew in this direction. Older bands of mineral down here, and then you see these nice two hedrocrystals at the top there, the youngest growing surfaces.

As I said, this is sort of a generalized stratigraphy for these samples. What we know now though is that there are some complications to this stratigraphy. We've recognized textures that tell us that mineral--that replacement has occurred at least in some areas.

In other words we see textures that tell us that minerals that were originally deposited have been dissolved and removed, and that secondary minerals have replaced them. So there is a potential for some of these bands to essentially be out of place.

In other words it's not just simply old to young as you go in this direction. And this is what we really have to characterize in order to really carefully and correctly constrain the relative timing and then the absolute timing of the fluid inclusions.

To date our work to put together these time maps, if you will, for each of these sections has involved petrography. The second bottleneck that we've had has been getting the electron microprobe up and running. The instrument was delivered in July and it's only up and running as of last week. So that was quite a surprise.

But nevertheless we have begun to characterize the trace element chemistry, and we are hoping that subtle distinctions in trace element chemistry in these sections will provide clues that will help us clarify the details of the growth history.

We will also be using cathode illuminescence and also we will be doing some oxygen and carbon isotope analyses on these, both rather conventional methods, and we will try using ion probe in situ methods as well. All of these things will be done again to determine the continuity and the relative timing of these different mineral bands.

Okay, here are the fancy sections. This is what some of them look like. And these sections really tell us a lot. They texturally give us a lot of information about how those minerals grew. Here, however, is how many of the other sections look.

This is tuff, and here is a little bit of calcite--all looks pretty much the same. So not a lot of textural evidence telling us much about the growth history of that calcite. Did that calcite grow over 10 million years, did it grow over 100 years? Difficult question to answer at this point.

An initial working hypothesis we had when we started to look at the petrography of these sections was that perhaps sections like this recorded the complete history of mineralization of this calcite, and that most or perhaps even all of the bands of mineral deposition were captured by these samples. And we thought that perhaps what we saw here was one event in this other section, and what we needed to do was try to find fingerprint of some sort to figure out which event that was.

But now that we are getting close to having all of our sections, now that we have looked at most of our sections in context of the location of their sample sites within the ESF and the ECRB, what we are starting to see, perhaps, is that there are different stratigraphies in different parts of the repository site. Okay.

So maybe this is not an event that's part of that other section. Maybe it's a separate event. So that's a question that we have and that we will be attempting to answer.

Where we are today is that we have constructed growth histories for most of the sections that we have collected. What we need to do next is to try to connect those. Okay. And so this is where we'll be using trace element chemistry as well as the petrography, cathode illuminescence, isotope analyses, to try to see if there are mineralogic bands that are distinctive in some way, that have some fingerprint, some chemical fingerprints, some isotopic fingerprint, some luminescence, so that we can connect one sample site to another sample site.

If we can do that we can maybe identify timelines that are continuous across part of the repository site. And if we can construct these timelines, then we have a greater chance of trying to pin down the absolute age of some of these timeline.

Then what we can do is go back to our sections, look for the location of fluid inclusion assemblages relative to those timelines. Any inclusions that are in a mineral band that's older than that timeline would be older than that timeline. Conversely, inclusion assemblages in minerals that are younger than that timeline would be younger. And this will give us much greater control, age control, in trying to constrain the ages of these inclusions. So this is a major focus for where we're at right now.

Okay, let's look at the fluid inclusions. Okay, these are a bit subtle, but this is as good as they get. This is a fluid inclusion right here. This sort of blue line is the outline of the fluid inclusion. This region right here is filled with fluid, and here is our vapor bubble--considerably smaller than some of the inclusion bubbles that Bob just showed us.

If we look around we can see that within this section, at a different focus level unfortunately than this inclusion, we have here an inclusion and a vapor bubble, here's an inclusion and a vapor bubble, an inclusion and a vapor bubble, an inclusion and a vapor bubble--they're definitely hard to see when they're projected--here's another inclusion and a vapor bubble.

And the important observation to make on this slide is that the liquid vapor ratios within these inclusions are pretty constant. Smaller inclusion, smaller bubble. That tells us that this is probably a fluid inclusion assemblage. That means that all of these inclusions were trapped at about the same time, and they represent a legitimate set of fluid inclusion which can be used to give us a legitimate temperature.

Okay, where are we today? Today we've looked at sections from 151 samples that we have collected, and we have observed two phase inclusion assemblages in 44 percent of those samples. The location of those, we go back to our map, the sample sites for samples that contain these two phase FIAs are in some cases concentrated.

For example these lithophysal cavity samples here and here, almost all of them contain two phase fluid inclusion assemblages. However, two phase fluid inclusion assemblages are scattered pretty much throughout both the ESF and the ECRB. They are leaner in some areas, but they are nevertheless present.

Okay, where are the inclusions in individual samples? In samples that look like this, most of the fluid inclusions--most of the fluid inclusion assemblages are in the calcite that is closest to the top. So they--so most of the inclusions are in what is probably the older part of the sample, although there are still details here that we need to sort out.

In some samples, however, there are inclusion assemblages in this area and also inclusion assemblages in some of this sort of central calcite band. Okay. This very outermost calcite band, which is present in only some of the samples--not all of them--which is a very clear calcite accompanied by very clear opal, we have not identified any fluid inclusion assemblages in that particular calcite, two phase fluid inclusion assemblages.

When we look at samples that look like this, some samples have two phase FIAs, some samples do not. Here we are missing textural evidence that really tells us something about relative timing of the formation of this calcite. So these are tough samples; these are going to be tough to figure out.

Okay, where we're at today, we are continuing to do petrographic work. We've not completed that yet. We are continuing to refine our understanding of the growth history of these sections. We are completing our examination of these sections to identify the location of all of the two phase fluid inclusion assemblages.

We are just beginning the trace element geochemistry work and the cathode illuminescence; and in the next couple months we will also begin doing some carbon and oxygen isotope work to try to help understand with this growth history.

Obviously what we're ultimately moving forward is to doing some dating. We are limited--we know from prior work that the Survey has done that we are limited to what we can actually date. We can use uranium lead techniques to data uranium-bearing opal, and we can use uranium series dating methods to date some of the youngest calcite. So it's not going to be easy.

But we think that at least if we can put together some of these--if we can in some way identify how to correlate these discrete sample sites, that will help us greatly. It may be that they don't correlate. We may not be able to do this, and that will be an important finding as well.

To summarize, let's see, what I think are probably our most important observations to date, these are all things that I mentioned during the talk; but first of all--and this first one is sort of preliminary. It's really something that we're shooting at right now. But it appears that perhaps in different regions in the ESF and the ECRB there are distinct stratigraphies. So we don't know how these areas actually connect.

Secondly, this is probably an important one, two phase FIAs are present in 44 percent of the samples that we have collected. The sites of these samples are locally concentrated, but they are distributed throughout the ESF and the ECRB.

And then finally most FIAs are present in the calcite adjacent to the tuff, but some of them are in the inner calcite band and then in those samples where we really have no zoning, some of them contain two phase FIAs as well. And we really have no constraints at this point on relative timing of trapping of those inclusions.

Thank you.

RUNNELLS: Thank you, Jean. Very interesting.

Dick, would you like to ask your question about vadose versus, what? Saturated versus unsaturated zone.

COHON: Hang on--

RUNNELLS: I'll tell you what, while they're working on that, Jean, can you tell us whether you've seen evidence of saturated versus unsaturated zone precipitation?

CLINE: No. When we met with Dr. Goldstein it was very interesting, and he presented a number of diagnostic to less diagnostic textures, but suggested textures, I guess, that could suggest different things.

And these samples, while they have very interesting textures, there are no textures that tell you flat out it's like this or it's like this. We haven't found them as yet. We see things that are suggestive of certain things, of certain environments. But--that's what we really have to continue to look at. I would not--we simply don't have enough observations to put us in either camp at this point.

RUNNELLS: All right. Thank you. Dick, do you want to try one more time to--

PARIZEK: I'm on. Parizek, Board. Just to the field relationships coatings on surfaces, whether they coat the entire surface or just constrained in the tops or bottoms, that's been some observations that have been made suggesting, you know--

CLINE: Right.

PARIZEK: --vadose or unsaturated conditions versus saturated conditions, I guess whether or not any of the collections were taken from places where the field evidence, which would suggest unsaturated formation.

CLINE: Definitely. As I said we tried to collect samples from every sort of environment and every sort of type of sample that we could. We're well aware of some of the observations that the Survey people have made. They were actually accompanying us when we collected our samples.

Yes, when we collect from lithophysal cavities most of the calcite is in the base of those cavities. Sometimes it kind of creeps up the wall a little way. Those observations are valid observations, and they are highly suggestive of those environments. So I would not refute--

PARIZEK: A field form would then be helpful perhaps in seeing later on some organization to the kind of discoveries you make when you finish your other work. It may be possible to see a correlation between some of the observations you make with fluid inclusions and the field occurrences

CLINE: Absolutely.

RUNNELLS: Jerry and then Paul.

CLINE: We photographed every sample location, so we--and we described it as well. So we have a good record of that.

COHON: This is Cohon, Board. Could you put up your last slide again?

CLINE: Seems to have escaped.

COHON: The first point, I wonder if there is data that's already been collected or samples that were collected for other purposes by the program, that can help you in coming to conclusions about that first point?

CLINE: That perhaps may be the case. I think one of the things that we need to look at are samples from some of the drill core so that we get out of the horizon that we've been sampling in. I think what will be very informative would be to see--to look at drill core, if it exists, in an area where we collected from lithophysal cavities, and to see if as we go up the mineralogy changes.

I didn't mention this, but when I said the stratigraphy changes there are areas within the ESF where rather than the samples being mostly calcite they are most silicon minerals, and there's one zone where that's the case. What is that related to? Is it proximity to the surface? Is it related to fluid flow in some way?

So one of the things that came out of this observation was the decision that we've got to go and look at some of the drill core or look at some of those records and see what's happening vertically. So I think that's definitely the case.

What we have to do though is look more closely at our samples and really refine the stratigraphies for the different areas. We've only very recently gotten many of the sections, so we're really still just putting this together.

COHON: Okay. Just one more question. I think I might have missed something. I thought you said that there were five phases to the project? Or were there four?

CLINE: I think I missed phase 5. That was publish, one word, it was the bottom--

COHON: Oh, I just didn't see it.

CLINE: Thank you for asking.

COHON: Thank you.

RUNNELLS: Paul Craig.

CRAIG: Craig, Board. One of the advantages of being emeritus is that you're allowed to ask--or at least you do ask really poorly focused, ignorant questions. This is one of those. We had some briefings from the USGS about their work on the rate of dripping into the lithophysae.

RUNNELLS: Paul, could you speak into the microphone?

CRAIG: Yeah, okay. The USGS work on the rate of dripping into the lithophysaes, and that was compared with the work that Bo reported on today. And there were many orders of magnitude difference in their estimates on what the drip rates were.

Now the connection I'm trying to draw here is between their work, where they had to assume an age in order to calculate growth rates--which is one piece of information we have on calcite; the second is all the work that's been done at Devil's Hole where they've dated the growth of the layers with great precision; and your work where you're struggling to obtain some kind of an age date.

And the vague question I'm trying to formulate is, isn't it possible to make use of whatever information the USGS used in determining--in getting their estimates, and the work--and your attempt to date the bubbles?

CLINE: Um-hum. We can. I guess I want to give you two answers to that question. First of all we sort of wanted to be careful about making some assumptions that were based on information that--over which there was some disagreement on.

So we're trying to establish our own set of observations and the conclusions that we can draw based on those. However, we're certainly not going to ignore those data. We are aware that dating has been done by several people from the Survey that they have dated several bands within those samples. And so we will certainly use those to help us determine how we proceed in doing dating.

However, what we can't do is extrapolate ages from one sample to another. I would be very leery of doing that unless we can establish this correlation and really positively convince ourselves that we know what the link is from one sample site to another. Of I understand you correctly, I would find it very dangerous to do that.

RUNNELLS: Question from Leon.

REITER: Yes, Leon Reiter. Jean, in the past, I think in your press release you said something about temperatures. I wonder if you'd repeat that or whatever you want to say at this point about heat? You don't want to say?

CLINE: We did not say anything about temperature in the press release. We've not conducted any microthermometry at this point. It was only within the last 10 days or so that our QA procedure for collection microthermometric data was approved, and it's only really within the last 10 days that we are ready to go forward with that.

We'll probably start doing it next week. So we don't have any temperatures at this point in time.

REITER: I thought I--there was something about elevated temperatures that was a statement that was included in there.

CLINE: I used the word elevated temperatures or thermowaters or something like that, and I used those terms because we see inclusions that have vapor bubbles.

And so those fluids--those inclusions had to be trapped at temperatures at least in excess of 25 degrees C. They had to be trapped at some elevated temperature--we don't know what that was--so that as that fluid cooled and contracted, that vapor bubble formed and exists today. So the presence of that vapor bubble tells us that.

REITER: And one thing that you said, that the people in the USGS and State -- quarterly meetings, but isn't there also some sample sharing and that was -- just tell us a little bit about that.

CLINE: Um-hum. What we've done, we set our schedule to collect samples and we invited people to come with us. And Joe Elling was a person who was along most of the time or all of the time, and a few other Survey people were along as well. The State chose not to have someone along with us on our sample collection.

I might mention that we--because these inclusions homogenize at relatively low temperatures, and the bubbles go away when that happens, these inclusions do not renucleate a bubble after that happening. So in order to protect these inclusions for us to look at and for us to study, we had to restrict the temperature range that all of these samples could see. And so we restricted the sample temperature range to zero to 35 degrees Centigrade.

So these samples have been very carefully handled and quite carefully stored, but what we have done is hand carry these samples to a lab in Montrose, Colorado, where they are also stored under temperature controlled conditions, and it's there that an individual is making these polished sections. And from each sample he's making five polished sections, and two of those go to us, the middle one goes to the State and the other two go to the Survey.

The State so far has not taken possession of their sections. Many of them are still being prepared, but they will be held at UNLV and reserved for the State. The Survey has taken possession of their sections as they've become available, and the Survey is conducting a parallel study to the study that we are conducting.

RUNNELLS: Question from Bill Barnard.

BARNARD: Bill Barnard, Board staff. Jean, could you comment on your current schedule for completing the project?

CLINE: We are working towards our deadline. This is sort of an awkward question because I don't know the official start date of this project, so I don't actually know the official final date of the project. I'm hoping it's something like April of 2001 because that's when we actually began work on the project. But that's the date that we are working towards.

We will provide information as we gather it. We don't--we're not going to work in vacuum, we're not going to hold all the information until the end. I might add that we have proposed a session for GSA 2000, which will be in Reno next fall, and we--we and the other people involved in this we hope will be submitting abstracts for that meeting.

Those are due in June of this year, and so a short term goal is to have information available to put in those abstracts and then present at that meeting.

BARNARD: That's the fall of this year?

CLINE: That's the fall of this year.

RUNNELLS: Any other questions from the Board or from the staff? Paul Craig's comment about being professor emeritus, allowing you to ask off the wall questions, gives me courage to ask you if there's any evidence in the 151 samples studied petrographically of a preferred direction of movement of the fluid. Shapes of crystals don't tell you anything.

CLINE: Shapes of crystals tell you how the crystals grew. The calcite crystals tell us that they grew out from the tuff. They trap inclusions along growth zones, so those trappings--that trapping is really telling us about growth zones in the calcite crystals.

RUNNELLS: I was thinking more about the shapes of the crystals, say in the fractures or in the breccia zones.

CLINE: The shapes of the crystals--

RUNNELLS: The crystals--

CLINE: --rather than the inclusions.

RUNNELLS: Right, right. Petrograph of the crystals.

CLINE: Does that tell us whether fluids came up or down?

RUNNELLS: Or any preferred direction of flow.

CLINE: No, and I'm just not aware of any way to get at that. The one thing that crystals can tell you in some cases is whether they grew under the influence of gravity or not, which they feel when they are in the unsaturated zones.

So if you go in a cave for example, and you see speleothems (phonetic) that are growing on the walls, you know you get these nice ram's horns that curl up and you get gypsum that forms certain patterns, and so those textures tell you saturated or unsaturated. But I'm not aware that you can even use those to get at flow direction of a fluid.

RUNNELLS: Okay. Thank you, Jean.

CLINE: Would be nice.

RUNNELLS: Any other questions? Well thank you very much, very interesting. We'll wait with bated breath for further updates.

Okay, our final speaker for the afternoon is Dr. Paul Dixon. Dr. Dixon has a Ph.D in geochemistry from Yale University, and he is currently the M&O technical lead for unsaturated zone and saturated zone geochemistry for the Natural Environment Program Office.

Today Dr. Dixon is going to update us on Busted Butte studies and some site scale flow and transport modeling. Paul, welcome.

DIXON: Thank you. I guess I get the ostatious privilege of being the last speaker today, and I see most people are still awake.

RUNNELLS: Yeah, I think that's a great compliment. Most the audience is still here. That's wonderful.

DIXON: --done well here, and I have to follow Jean. So I guess what I would take from Jean's talk that I'd like to parlay into the talk I'm going to give on Busted Butte is that there's a lot of pieces of data that have to be collected to pull together to get to an answer.

And as you heard from Jean and listening to that, it isn't just going in and looking at one thing. That's one of the things the Busted Butte test brings. We're trying to look at a multitude of things and from those studies try to get back to the basic question of how radionuclides will move through the rocks underneath the repository.

So what I'd like to do today is kind of review what we're going to--what were ultimate goals of this test when we started out. This is a review for most people, the Board, but it's basically we wanted to look at the influence of heterogeneities on flow and transport; evaluate the aspects of the site, including fracture-matrix interactions and permeability contrast--permeability contrast being boundaries within the rock where you have different layers of the rock, and how fluids flow through those different boundary layers, between different types of rock or different depositional; consider colloid migration in the unsaturated zone, which in this large test we can do; test the use of laboratory sorption data at the field scale; calibrate and validate site-scale flow and transport models, which you heard Bo talk about some of the work we're doing there; and address scaling issues.

You know, one of the things is most of the experiments have been done on sorptions and transport have been done at the bench top. In the block there for Busted Butte, for those in the audience, the block here, this is roughly 10 meters by 10 meters by about five meters high, so this is a very large scale test. Next slide.

Progress towards goals--the test was broken into two phases. There was the Phase 1 tests which were short, three-meter boreholes, some were just injection with no collection, and some were injection collection. And then in Phase 2 is the large block you saw there that had multiple injection and collection boreholes.

In the Phase 1 test it provided very good insights that Bo is using about flow and transport around heterogeneities. Also indicated that capillarity and matrix dominated flow regimes exist in the vitric Calico Hills; and that subunit and unit contacts are important for diverting fluid flow depending on the level of mineralization of these contacts.

Phase 2 is expected to provide additional insights into flow and transport, heterogeneities, as migration results near faults are analyzed. So within the Phase 2 test block we have faulting within the unsaturated vitric tuff there, and we can look at how that affects. Phase 2 will provide larger scale, three-dimensional comparisons to the smaller scale Phase 1 results. Next slide.

The analytical technique to detect microsphere, i.e., the colloid surrogate that we used in this test, is nearing completion. There was a lot of analytical difficulty in developing a technique to get the microspheres off of the pads reliably, and we believe that we will start the beginning of this next month actually analyzing the pads and some of the rocks for microspheres.

Insights into the sorption parameters and the site scale model validation obtained through analysis of reactive lithium and non-reactive tracers, reactive metals, radionuclides analogs. We haven't looked for the reactive metals yet, but we have been able to get insights from these other things that we've seen on the pads, the lithium and the conservative tracers. And scaling issues are being addressed by this test and giving us some idea of the timeframes. Next slide.

Now deliverables, everybody--the question has been asked, how--do these results mean, where are they going. Revision 00 of the transport properties AMR is currently in checking. That will be part of Bo Bodvarsson's PMR on UZ flow and transport. That AMR consists of work by Ines Triay and now Jim Conka, Wolfgang Randes and his work, all of the Seawell's work as well as all the Busted Butte work. So it's a very large volume or document of work.

And Revision 1 of that is scheduled for completion the end of this summer, as well as the revision of the colloids AMR which Jim Conka is working on. It's due sometime the end of this summer--both of those.

I know the last time you guys met--poor Mark Peters. I don't know if he's still standing around here, but you guys had a long, lengthy discussion about the applicability of the Calico Hills and Busted Butte versus repository. Like to do a general review here. We can take it up in question and answer for more.

But it's--the Calico Hills at the repository is variable. It ranges from zeolitic, non-zeolitized rocks in the southern portion of the repository, to zeolitized rocks in the northern portion. And that's known from the site scale model and from the limited borehole information that we have, the Busted Butte vitric with a relatively low abundance of clay or zeolite alteration.

So at Busted Butte there's not much clay and there's not much zeolitic alteration there. And it looks more like the southern portion of the repository section--in fact the lower Topopah Springs, upper Calico Hills section, observed in the H-5 drill hole and SD-6 look very similar to what we see at Busted Butte. And the relative portions of glass and zeolites are very similar to what was determined in the H-5 borehole.

Retardation of the Calico Hills under the repository can occur due to sorption, fracture-matrix interactions, and matrix diffusion processes. The Busted Butte studies are quantifying the retardation mechanisms in the vitric portion of the Calico Hills.

We're not dealing with any of the zeolitic type of fracture flow because we have a good idea from work that's been done in the past that fracture flow in the more zeolitized zones is very similar to the fracture flow that we're seeing in the Topopah, and we're using some of those analogies in the flow and transport modeling at LBNL.

And flow and transport models developed for SR and LA will be consistent with the Busted Butte results. In fact we have a very tight integration with Dr. Bodvarsson in the generation of his flow and transport codes to make sure the information's coming out is consistent with what he's been developing thus far.

I put this viewgraph in for you guys to refer to as I go through the next parts of the talk. What I wanted to do, because up to this point in time with Busted Butte we've kind of given you little bits of data. The rest of the talk now is actually presenting the data we've collected up to now that's included in the AMR, that's in checking at LBNL, to give you a flavor of what sort of information exists for the Rev. 0 version of the AMR related to Busted Butte.

And just go back one--I want to point out that on here all the drill holes are numbered, so that when you see the next sections as we come along, we'll do things. The next slide we're going to head to, we're actually going to look at the ground penetrating radar results.

And for those of you in the audience, ground penetrating radar is basically radar that's at a long enough wave length that it imbibes into the rock. You can look at moisture, different moisture contents using ground penetrating radar.

The resolution on this is about 10 centimeters. Most of the images we have are two-dimensional, and what you see here, we're going to look at the results of 46-16, so if you refer back to your last diagram, it's a vertical slide from the top of the block to the bottom of the block.

And what I'd like to do now is I'll do--run through an animation here as we sit, and we'll show you guys basically what we saw over a time step, over a tim period of --as you can watch the time change, sitting up there--what we saw from basically '98 through '99.

In other words how the fluids came in, and noting that as you add more fluid to the system your resistivity increases, or the radar velocities decrease and therefore that's why you see a lightening of the thing. You want to run that again and we'll play it once more just to give you a visualization of how this technique is showing things.

These are--the injection boreholes are up here, the high level injection boreholes, and these are the low level injection boreholes. This is borehole 46. This would be in the--if you're orienting yourself, this is in the test alcove here, this region, and then this region out here is on the main adit, this borehole in 48-16.

SAGÜÉS: Where are you injecting?

DIXON: The fluid is injected where you have the white dots here, and the white dots there. So there's fluid injection at a high plane and a low plane.

SAGÜÉS: At the same time?

DIXON: At the same time, yes. In fact if you flip to the back of material in the back there's actually a diagram that shows you collection injection borehole in a three-dimensional picture. Priscilla, you look confused.

NELSON: What is being plotted here?

DIXON: What is being plotted here is the ground penetrating radar data time step through time. So starting in 9/1 of '98 up through 3/3 of '99--this is work by Ken Williams at Lawrence Berkeley--and we're looking at a series of time steps of how the moisture front is changing over that time period, every time they went into this borehole and measured the ground penetrating radar--use ground penetrating radar to measure the fluid migration.

NELSON: And the plot is changes in velocity?

DIXON: We're looking at changes in velocity, but changes in velocity as related to fluid content of the rock. I'm sorry?

NELSON: No, that's fine.

DIXON: Okay.

SAGÜÉS: What is the difference in the graph on the left and the graph on the right?

DIXON: The graph on the left is just--that was the starting point in September 1st. That's what the--if you took the borehole, that's what the starting composition was when we first started the entire block. That's just a single orientated fissure, and then this is just a time step from that point on until 3/3/99.

SAGÜÉS: So that thing on the left is a plat or an elevation? I don't quite--

DIXON: It's the same slice as this here. It's just rotated 90 degrees.

SAGÜÉS: Um-hum.

DIXON: Roughly.

SAGÜÉS: Okay, only the one on the right is not a perfect rectangle where the one on the left is this or not?

DIXON: It is. This one here is the graphical representations of--

SAGÜÉS: Okay, Phase 2.

DIXON: Sorry to confuse you.

COHON: Alberto, use your microphone if you're going to keep talking.

SAGÜÉS: Okay. Looks like the one on the left is also--is not only rotated but it's also flipped. Is that right?

DIXON: No. If you go back to the beginning of this, this figure--well before she started--this figure when it starts out is exactly this figure here. It's just--that's just the starting, what it looked like for the initial snapshot, the preinjection of fluid into the block, what was the initial conditions.

SAGÜÉS: And what do you get out of this?

DIXON: What do we get out of this? Because when you first start the test you have a series of collection boreholes that you'll notice on the figure there. We're looking for when the fluid first appears.

In a totally blind test, because we didn't know the rates of things, we used geophysical techniques to give us an idea of the rate at which the fluid is migrating to the block and giving us an idea of where in that block we might expect the collection boreholes to start showing fluid arrival times. Next slide.

RUNNELLS: Paul, we'll give you a little extra time at the end because of these clarification questions.

DIXON: This is fine. I'd rather get clarified now while we're on the slide than move on. I am the last talk, so it's fine.

These are, as Mark pointed out earlier, these are electrical resistivity images. This is another geophysical technique that we're using, and here--it's probably more clear on the diagram you have in front of you--is the baseline of the electrical resistivity of the block. In other words this gives you a full three-dimensional picture. It covers the entire test block as opposed to a 2-D slice you're getting in the GPR.

And the resolution here is a little bit coarse, so it's about a half meter. But you can see here, here's two different time slices, and then this slice here is broken up into different depths in the blocks. You can look at again--if you think about the tracer fluid being electrolytic, you can actually look at the movement of the tracer fluid using this technique.

The GPR looks at the movement of a moisture front. This looks at the movement of probably the tracer, because it has a different electrical conductivity than the pore waters in the rock.

CRAIG: I'm sorry, I'm absolutely unable to tell what message I'm supposed to take away from this.

DIXON: I'm sorry. The message here is again this is another device for looking at how the fluid's moving through the rock. This is just one time slice versus the baseline, and again from this we can tell how the fluid is moving through the rock in different sections of the rock, in relationship to what we're collecting on the pads in the collection boreholes.

CRAIG: So how is it in fact moving?

DIXON: Well as you increase the ionic strength of the solution with the tracer solution, basically you get more and more negative resistivity in the rock, electrical conductivity. And so basically as the color becomes darker, the more blue, that means that basically where you're seeing fluid increases or tracer movement in the block.

Well I mean this is--this is the same thing that Mark was showing in the drift scale test where they're using ERT to look at fluid fronts moving out. There you're looking at just pore water movement. Here you actually can tell the difference between pore water and the tracer because they have very different ionic strengths, and therefore the electrical conductivity of the tracer fluid shows up very clearly in this sort of a geophysical technique.

This is just another--this is a visualization tool used and will become quantitative to compare with the pad data that we collect in the boreholes. This was initially--this is a visualization tool to tell us which pads and areas the fluid was moving through the block and how it moves through the block in three dimensions without mining back, without physically going in--

CRAIG: When I look, it's visualization tool, but my problem is that I can't tell what kind of a message. I can't even tell--I can't tell where the flow is going. I don't know how to read it. It's too complicated--

DIXON: Well, this--

CRAIG: Don't do it now. Don't do it now.

DIXON: It's just--that's--these are depths, so if you go eight meters back into the block. It's just slices through the block. This has to in a 3-D cube. Next slide.

What I'd like to talk a little bit now is that there has been the laboratory experiments that went on with tracers as well as--so what we used in the field, so they've done not only the real radionuclides in the labs, the neptunium, plutonium and americium, but they've also looked at the analog tracers so you can compare results from the field and the radionuclides with the analog tracers in the field.

And in your backup section there's actually some actual data tables, but on the next slide is to point out that the measured sorption values of Busted Butte vitric rocks are much greater than we currently using in our models. What we've measured at Busted Butte, the values are much greater.

Preliminary sorption results indicate that smectite is an important component, trace component in the vitric rocks, and there's a strong relationship of plutonium to the smectitic content, the sorption coefficient. Americium shows only a weak variation; and as for neptunium, the values that we're getting from Busted Butte are about a factor of 20 higher than we're currently using in our models -- so considerably different value for neptunium in these rocks.

The next slide I wanted to put up because it's one of the few examples on the project here where we've looked at pore waters. And we've actually quantified them, and what you have in this table is four different samples and then the average of those samples, and compared to J-13 water.

And I put it up here to show you that the pore water composition in the unsaturated zone vitric rocks is considerably different than that of J-13. And what that means is that the significance to the lab studies that have only been done with J-13 and the solubility things, that now has to be determined and evaluated, the impact of this sort of data. How much does that impact the solubility, different things when you change the composition the way that you see in the pore waters there.

And the last thing is that this work could be extended to include pore waters and partially welded to even some of the welded rocks. People have been trying to get fluids out of those. Next slide.

I wanted to step through a little bit of the Phase 1B results, and point out that again in the Phase 1B was--if you go back to your figure--earlier figure--these were--you had an injection borehole with one injection point, and you had a collection borehole, and that collection borehole had a series of paths along it.

And what you're looking at here is depth into the borehole and then so this would be the surface of the wall, this would be 190 centimeters back into the borehole. And what you're looking at here is the time at which those paths were sampled and looked at for different compositions. So the paths were periodically pulled out and analyzed.

So as you can see, early on there was nothing, nothing, and then all of a sudden eventually you start seeing some fluorescein breakthrough. And that breakthrough occurs pretty much along the plane of where the fracture is. Next slide.

The tracer shows strong expected breakthrough patterns during the Phase 1B injection. The breakthrough is slightly ahead of predicted matrix flow only, meaning that even though you have a great degree of capillary and flow in the matrix as you inject these fluids, the fracture is influencing how the fluid comes through the non-welded Calico Hills rocks here.

There's a lot of lateral spreading, and this here is bromide, and this is the polychlorinated benzoic acids. You see similar behavior between these two and fluorescein, which you would expect in a conservative tracer.

Lithium, on the next slide, which is a slightly non-considered tracer, shows a much more basically retarded behavior which you would expect of lithium, being that it's being imbibed and held in the rock. Again, lithium in these rocks has a Kd of about one; neptunium in these rocks measured in the laboratory has a Kd of about 20. Next slide.

NELSON: Nelson--

DIXON: Yes.

NELSON: --Board. What do you think of the saturation conditions in the rock as a function of time through these tests?

DIXON: The rock goes up to a certain pore saturation, and then it capillaries. You don't saturate the rock, per se. You reach a level of saturation. I think the level of saturation here is about 35 or 40 percent in these rocks.

So it's an unsaturated test to this point, but you're--you know, you imbibe under capillarity of the fluids out but you don't completely saturate the rock where you're actually draining under gravity.

This slide here was just to show that for the test block for Phase 2, which is a 10 by 10 by 8 meter block, we have actually gridded that block and we've run tests with both conservative and nonconservative tracers.

This is to give you an idea of a conservative tracer at a one-year time step, how far we would have expected that conservative tracer to have went in one year based on the--our understanding of what the rocks are at Busted Butte, the non-welded rocks, and the characteristics that are currently being used in the UZ flow and transport model as it stands today. Next slide.

In this slide here we're looking at a spatial comparison of bottle predictions of a conservative tracer against fluorescein breakthrough in the Phase 2 test. And the predictions match both observations with the exception of one borehole, and that's borehole 10.

If you look back to your earlier cross section map, borehole 10 is very close to a fault, and therefore it's a working hypothesis now, it has to be proved out, but there appears to be some communication along that fault, giving different breakthrough results with borehole 10.

If we go to the next slide, which is just predicted time of breakthrough versus the measured time in days, what you notice again is that borehole 10 lies way up here at the top. It's an apparent outlier in this. Prediction again matched pretty well, and again borehole 9 tends to plot off; borehole 9 down lower is one that's near the fault.

And currently according--talking to Jake Turin and Wendy Solva working on this, boreholes 46 and 48, because of their angle to the injection boreholes, they're within about six or seven inches, and they're not sure if you're looking at direct communication on those or whether or not we've had borehole collapse in some areas, giving you direct communications between the injection and the collection borehole. Next slide.

What was tried to be done over the next thing here is we're going to look at some of the results from Phase 1B. I did show you the time step, the actually just static picture of date versus time. What I wanted to show you was they've actually--we'll step through a series of pictures here, looking at the bromide concentration in the 1B test to give you an idea of how it comes out in the pad and then moves up and down the pad, in time.

What you looked at was a cumulative curve of data. What we'll look at now is the time step through there. And if you watch, the date will--you'll see the date standing here, and you can start watching as the bromide starts to come through the system here and fills in as we step through time.

So you notice there as you step along it isn't just one fracture that controls things. It tends to come down in one area but then it will shift with time slightly to the right or left, depending on what becomes the more prominent path or flow during that time period.

The next thing we will look at is total moisture content, and again this is a 10 milliliter per hour/minute injection hole. This is a one milliliter per hour injection hole. And what you'll notice is that in the one milliliter, you really don't see any difference in the moisture content. You didn't see any bromide in the last one. It was just too slow and now the fluid was imbibed during the timeframe of the test. You only saw results in the 10.

SAGÜÉS: Can I ask you again with respect to that figure, you're injecting something on the top boreholes?

DIXON: Yes, we're injecting here from a single point injection point--

SAGÜÉS: From the center of it? It's not like--

DIXON: Yes.

SAGÜÉS: --all along, but just--

DIXON: No, from a single point. I showed you 1B test earlier--

SAGÜÉS: Okay.

DIXON: --along--

SAGÜÉS: And that happens also in the other one, injected both 5 and 7, is that correct?

DIXON: 5 and 7 are injected from a single point, roughly midway into the borehole along what we perceived--what we identified as a fracture zone.

SAGÜÉS: Okay, now on the previous animation, the one that you just finished, there was something happening only on collection 6 but not in collection 8. Is that--did I see that correctly?

DIXON: Yes, and that's because this, as I've just mentioned, was an injection rate of one milliliter per hour. This was 10 milliliters per hour. And so at the slower injection rate, even though this distance here is only about a half a meter, we didn't see enough drive at the one milliliter per hour injection rate to give us breakthrough into the collection pad.

SAGÜÉS: All right, thank you.

DIXON: Next slide. Oh, you're just stepping through the colloidal moisture now. What I'd like to do now is--what we were just looking at was the Phase 1B test. I tried to make this into an animation. It didn't work. What this is these the collection boreholes that stand out here in the tunnel. This is your line of sight. You're looking at these collection boreholes: the red here are the injection boreholes.

What we're doing here is every time we roll out the collection liners they go and roll them back out; they go over it with a UV light and they look for the first appearance of fluorescein, the first appearance of fluorescein that will fluoresce with a black light. That gives them a clue of which pads are important to analyze for tracers.

What I'd like to do is just time step from August 1998 when we started to the present day to give you an idea of how the block is saturating up and things are moving around. And we can just time step through this.

Now what you notice there was as placed turned on and off as we were going through. And that's an interesting phenomena, yet to be explained, but it is one that as you look through your color viewgraphs it's something that we have to figure out; because in some places where, even though it doesn't show that it's on with the fluorescein, we're still seeing in those paths continued tracer deposition of both the conservative tracers--things like lithium, bromide, some of the polychlorinated benzoic acids.

So we're not sure what all this means yet. It's in the preliminary stages of being interpreted, but we do have the data and it is currently being collected and analyzed.

I guess I'd like to kind of conclude with porous media flow dominates in the vitric Calico Hills. The data from the boreholes surrounding the repository results from Busted Butte are expected to build confidence in the UZ flow and transport model.

Preliminary sorption results indicate that smectite is potentially important to performance in the vitric rocks, as well as other parts of the repository, and that the current Kds being used in the flow and transport models are very conservative. We're seeing much, much higher sorptive capabilities in the vitric Calico than was expected.

And data and analysis from tests will continue to be considered as part of the basis for the preparation of the the site recommendation consideration report and the license application as we iterate through.

And I think what I will go to now is just to point out the AECL removed two blocks from the Busted Butte this year. Those blocks are up in Canada and those blocks are going to be analyzed for two different experiments.

The first experiment's going to be an unsaturated flow experiment where they use real radionuclides and they try to mimic with real radionuclides in a large one-meter scale block what's going on, opposed to try to mimic some of the--with real radionuclides what we're seeing at Busted Butte with the analog tracers on an intermediate scale.

And the next slide, a smaller block taken from there is actually going to be used--saturated, and they're going to do saturated zone flow and transport tests through the non-welded type of tuff rock, to look at how that occurs. So they're going to do both those with radionuclides.

And I think that's--we're done, finito.

RUNNELLS: Okay, good. Thank you, Paul. Yeah, let me just ask a quickie because it's the last thing he touched on. What evidence do we have or what data do we have to show that the analogs that were chosen are in fact the appropriate analogs for neptunium, for example, neptunium plus 5, we're using a nickel plus 2 analog. I mean where does that come from?

DIXON: That comes from years of laboratory research by people like Ines Triay and others around the world.

RUNNELLS: Okay.

DIXON: And it's been--there was a series of things, and those--you have to understand that there are things that might be closer, of an analog, to neptunium that aren't neptunium or radioactive, but they may have health risks and therefore would not be permittable to use in a test like this.

RUNNELLS: Well the work you're doing in Canada will show how close--

DIXON: Right.

RUNNELLS: --many of these are.

DIXON: Correct.

RUNNELLS: Okay, good. Thank you. Alberto, question?

SAGÜÉS: Yeah, I found the table on page 13 interesting where you show the--specifically the colloid contents. This would be number 13, if we have it there.

DIXON: It's going to be--it should be close to 13 on yours.

SAGÜÉS: And looks like the colloid contents were like
--there is--they were about three times higher or so than J-13, and also the chloride is significantly higher. It's about 2 ppm compared with -- ppm. Is this--does this have any relevance to what would happen in the repository area, or is this sort of like--

DIXON: Well all I can say is that vitric non-welded rocks have this sort of a pore water chemistry. The indication from this and from what we've seen other places is that the Topopah Springs pore waters are going to probably be slightly different than J-13 like these, to significantly different with certain elements. But until we actually go and measure those, that's an unknown thing at this time right now, Alberto.

SAGÜÉS: Okay.

DIXON: But until you measure that, the best thing that we've used in the project, and what we've always done, is use J-13 as our closest approximation. You can see that J-13 does have significant differences in certain areas from what we see in a pore water in a non-welded rock at least.

SAGÜÉS: Okay. Because from the corrosion standpoint, 3x increase in the colloid content is something interesting, to say the least.

DIXON: Yes.

RUNNELLS: Jerry?

COHON: Cohon, Board. Can we look at slide 24 please, the conclusion slide?

DIXON: That one?

COHON: No, 24, next one.

DIXON: Well these are going to be times--what--you want the conclusion--

COHON: Conclusions.

DIXON: Conclusion slide. I'm sorry. Because some of these were done in sequence--

COHON: Well we get to see it again--

DIXON: --versus--what's that?

COHON: We get to see the animation again. Now it's much clearer.

DIXON: Clear as mud is always good.

COHON: I think we've skipped it.

DIXON: No, that's it there. Yes, sir?

COHON: The last bullet.

DIXON: Yes, sir.

COHON: We heard earlier in an earlier presentation that there's a freeze on data for SRCR, and your last point seems to contradict that.

DIXON: What we worked out with Dr. Bodvarsson and his modelers in collaboration with what we'd done at Los Alamos, we had a freeze date basically of November 10 for things that we were including while we were developing this AMR. This was all data collected up through about November 10 that was being pulled together for that AMR.

And that was sent to Dr. Bodvarsson and his modeling team, and the different areas used different parts of this, from the Kd data to the different flow and the porosity permeability data that I have you last time.

COHON: So everything after November 10 will have impact on the project--

DIXON: We'll go--

COHON: --after SCRC.

DIXON: It'll go under Rev. 1. It'll go under Rev. 1 which will go under the November CR. It will be reported in late summer of this year.

COHON: All right.

DIXON: It will be in time for--

COHON: Well what I'm--I'm in stereo here, and it's mostly agreeing. But Rev. 1 of what?

DIXON: Of the AMRs and PMRs.

COHON: But that has no impact on SRCR.

DIXON: Yeah, because it's done before November.

COHON: Talk in your mike.

DIXON: You just need to listen--

COHON: I'm sorry, which--November of which year?

DIXON: November of this year.

COHON: November 2000.

DIXON: 2000, yes.

COHON: Oh, I'm sorry. Okay.

DIXON: And in July of 2000 will be the final Rev. 1 update with all this information that's been collected up through April. April we will have a cutoff date and then it will be rewritten, updated and incorporated by July of this year into the new flow and transport PMR Rev. 1, and that's what will go into TSPA in early August, mid-August, and that will be updated for the November submission.

COHON: Well let me ask the question before someone else does. How did you work out the special deal and no one else can? Why do we--

DIXON: The importance--

COHON: --push the--

DIXON: --data to flow and transport, since we had no information on flow and transport in the unsaturated zone, led us to initially the Busted Butte test because of where the modeling was being done--was going to be done in-house.

So when it moved to Berkeley from Los Alamos we just carried on the way that we were going to incorporate testing as we were developing the models and things with Berkeley, and that was a mutual agreement with Dr. Bodvarsson.

COHON: Thanks.

RUNNELLS: Did you get your question answered, Jerry, from Dan Bullen and Paul Dixon?

COHON: We're going to find out right now.

RUNNELLS: Okay, Dan Bullen--

BULLEN: Bullen, Board, I need a point of clarification because I asked Mark Peters the same question and he told me --the answer that I thought I heard was that they have until summer of this year to get data for November, which is the final SRCR release. And so I was under the impression that Rev. 0 locked in last year, Rev. 1 ends in the summer, and that Rev. 1 data will be the data that they'll need.

And if you'll remember from yesterday when we heard all of the nice--actually I guess it was Jack Bailey this morning telling us about how the revisions are going. Rev. 1 is one of those stuck in there, but there's still time to get data in, which is why I asked Mark that question.

RUNNELLS: Dick? Dick, did you have a question?

PARIZEK: Well--Parizek, Board--it has to do I guess with the modeling flow in the saturated zones? I guess Kds can be upgraded? Back in October I heard that everything was frozen, you know, for the site recommendation work. But from what you're saying now, it's not quite frozen--

DIXON: There are certain places where we will add data or we could do sensitivities and stuff for Rev. 0 and show importances. Mark's standing here. You wanted to say something?

PETERS: Mark Peters, M&O.

BULLEN: Was I wrong?

PETERS: No, you're right. There's the SRCR, and then there's the SR.

BULLEN: Yes.

PETERS: Okay. So the SR--we're talking data freezes for SRCR, those have basically past. What Paul was saying was--I was saying summer time; that's true; but in the case of Busted Butte we took a couple more months to make sure we got as much data as we could in for SRCR. But Rev. 1 is the same as final SR.

Does that clear it up?

COHON: Mark, and Rev. 1 is summer 2001, spring 2001? What's the--

PETERS: The data that we collect up into the summer time frame will go into the--

COHON: No, I'm sorry. I mean the SR itself.

PETERS: Is summer of 2001.

COHON: 2001, right, thanks.

PETERS: But we're mixing up data feeds with reports.

COHON: That's right.

PETERS: The SRCR report is November '00?

COHON: That's right.

PETERS: Yes, this November. So we're coming up on that--

COHON: And the data other than Busted Butte will be frozen summer--

PETERS: For the final SR.

COHON: Was frozen summer '99.

PETERS: Well, it--

DIXON: It was--most of it--of the information by August of 1999 that went into the SRCR was--that's where the data cutoff was. We extended it by several months, as Mark said, for Busted Butte because of the importance of that data and the necessity to have some of the actual field test, because Busted Butte had been going for a while and we wanted to make sure we had some of that information--

COHON: Okay, let me interrupt you. You extended it to November '99?

DIXON: Yes--
PETERS: Right.
DIXON: --yes.
COHON: Okay.
DIXON: That was--
COHON: Now, I'm sorry, we're back to where we started. So how do you say that will continue to be considered as part of the basis for SRCR? November '99 is gone, right?
DIXON: Yeah--
PETERS: The bullet's probably a little confusing.
COHON: It's incorrect, it's not confusing.
PETERS: Let me take one more--can I take one more?
COHON: Yeah, sure.
PETERS: We collected data for the SRCR Rev. 0, whatever you want to call it, the freeze was in the summer time frame. In the case of Busted Butte we went ahead and submitted some additional data November '99, calendar year '99.
COHON: Right.
PETERS: That's going in--that's going into the SRCR--
DIXON: And that's all the information--
PETERS: Additional data that's collected between basically November '99 and roughly spring, summer--July,

let's say--of '00 will be considered for the SR, Rev. 1.

COHON: Fine, that's fine. Now this is not nitpicking. This is wrong. You say "Data and analysis from the test will continue to be considered as part of the basis for SRCR." That's wrong. Is that--am I correct?

PETERS: That's correct.

COHON: Thank you.

RUNNELLS: Paul, do you still have a question?

CRAIG: Yeah, I'm going--I've got to go back to be confused on technical issues rather than timing issues.

Flow through the unsaturated zone is notoriously non-linear, and what I'd like to understand is the degree of extrapolation from the high water--high concentrations that you're using here so that you can get data to the concentrations that actually exist under the conditions that you believe will be out there in the natural mountain.

DIXON: I'll say that the concentrations being used in the test are higher but not orders and orders and orders of magnitude. It may be one order of magnitude higher than what we'd be expecting to see in nature for some of the stuff.

CRAIG: So that--

DIXON: So that makes the analytical part of this test difficult because we wanted to get concentrations which were more close to what we would expect for reality in these solutions. They're within a factor of 10 or less.

CRAIG: Okay, and you were getting transport times of months over distances of a few meters.

DIXON: Of the conservative tracers. We have yet to see the non-conservative tracers--

CRAIG: So that if--

DIXON: --represent the--

CRAIG: Well, water--water flow is a conservative--is conservative, right?

DIXON: Yes.

CRAIG: Right, so that's what I'm interested in, water movement.

DIXON: Right.

CRAIG: So that means that if you were to drop back by a factor of 10 on the inflow rate, that the time--the transport times over a few meters instead of being months might be tens of months or say, years?

DIXON: We have within--

CRAIG: So we should think of a velocity--so this implies a velocity of transport of water through this particular rock that you're looking at of the order of a few meters per year under realistic conditions.

DIXON: Right.

CRAIG: Is that correct?

DIXON: If the infiltration rate is high enough, yes.

CRAIG: No, no, I wanted to scale everything back by a factor of 10 because that's what you said I had to do in order to go back--to go to mountain conditions, assuming linearity, which is probably not very--a good thing to do.

DIXON: Well, I think I'm mixing apples and oranges with you here. I was talking concentrations of solutes in the injection fluid. The injection fluids were injected at rates of one, 10, 50 milliliters per year at different horizons. Where we have the higher injection rates, i.e., 10 to 50, we are seeing the most movement and the most travel flow. Where we have the one milliliter per hour injection rates we have seen considerable less movement.

The actual spatial--you know, the actual ratio of that, I can't give you right here and now. I don't have that at the top of my head, but we can probably determine that and get--

CRAIG: Yeah, well what I'd like to understand is how I go about taking your data and going back to the kinds of injection rates which you would get--expect to get in the naturally operating mountain so that I can get some qualitative feel--

DIXON: Tens--10 mill--

CRAIG: --for the transportation rates.

DIXON: Well 10 milliliter per hour injection rate is fairly close to I believe about 30 milliliters of infiltration per year.

CRAIG: Okay, that's the right direction. We'll discuss it later.

RUNNELLS: Abe Van Luik would like to clarify a point on the previous question.

VAN LUIK: I think on the question of schedule--this is Abe Van Luik, DOE--unfortunately this bullet is not as untrue as it may seem. The data feeds that were supposed to be frozen last year, some of them have just been settled, you know, within the last few weeks. And so we've had to do a lot of work arounds to make sure that we still get our products out on time.

And the idea that there is a sharp cutoff and that no new information will come in is probably true for the official quality assured transfer of data. But it is not true if something in this test shows or calls into question previous data, you know, we would have to stop the press and restart on some of these things.

So this may be more true than it should be, is my point. And when we say the cutoff is this month, it's been our experience that that's basically when people start saying "Oh, we should prepare something to turn in," you know. So things have not worked out as clean and crisp as we'd like to, and most of the AMRs are a little bit behind where we'd like them to be, because the data feeds haven't come in on time.

RUNNELLS: We have time for I think two more questions. Dave, and then Dick.

DIODATO: Yeah, Diodato, staff. In your page 9, getting back to the GPR figures, the GPR--the velocities pictured here, just so I get my understanding straight, the lower velocities correspond to places where you have lower water saturation--

DIXON: No, higher water saturation--

DIODATO: Higher water sat--

DIXON: Because you're slowing the velocity of the radar wave as it goes into the rock, as it goes into the water.

DIODATO: Okay.

DIXON: Because it accelerates through the highly dense rock, then de-accelerates when it gets into a higher moisture content. Does that make sense? In other words, if you had a rock mass and water sitting next to it and you clanked something, when you're in air and you hit something it has a certain ring. You're underwater, it's louder; if you put your ear against a rock and hit it, it's very loud because of the rate at which it comes through.

DIODATO: So the velocity orders are rock, air, water, or air, rock water?

DIXON: It's air--it's air, water, rock, where air being--

DIODATO: Air, water, rock, okay.

DIXON: --being--

DIODATO: --fastest. Air's fastest.

DIXON: Rock being fastest--

DIODATO: Rock is the fastest, air is the slowest.

DIXON: --then water would be the next fastest, then air would be the slowest.

DIODATO: Slowest. Okay. So now on this plot, you've got here this one zone of slow velocities, which I guess now we're agreeing corresponds to lower water saturations, higher air saturations--

DIXON: --mean the green--

DIODATO: On the left hand side, let's say.

DIXON: What's that?

DIODATO: On the left hand plot there.

DIXON: Ahh--

DIODATO: Left hand plot.

DIXON: Left, over here?

DIODATO: Left hand--other plot.

VARIOUS SPEAKERS: The initial--other left.

DIODATO: Other plot.

SPEAKER: You're the man.

DIXON: This one.

DIODATO: Yeah, okay--

DIXON: This one--if you take this plot here and take that point, that corresponds to that point.

DIODATO: Okay. So--but let's stay on the left hand plot--

DIXON: Okay.

DIODATO: --a second. And there's a line that goes up about 45 degrees, that line there, yeah, which corresponds to then lower water saturations, higher air saturations, correct?

DIXON: That--it goes--

DIODATO: It's a low velocity--

DIXON: --it goes from very, very low velocity, yes.

DIODATO: Okay. So is that in any way--are you inferring any correlation with geologic structures or some other heterogeneity which--

DIXON: At this point in time, this--if--this would imply that there's some geological structure or zone in there. That has not been identified as a fracture when we mapped, but with video camera of the boreholes--

DIODATO: Right.

DIXON: --that doesn't mean that there's not a zone of permeability there, and that's what that appears to be. In talking with Ken Williams and stuff, until we do some other coring or limited mine-back into this test when it's finished, the answer to that question will never be clearly elucidated.

But you can hypothesize probably fairly--fairly large degree of confidence that that is a zone of higher permeability whether it's a fault that's not identified within the boreholes drilled today, or whether it's just a zone where you have less cementation or less compaction.

DIODATO: Okay, I understand. Now in terms of correlating the velocity structure with the moisture contents or saturations, have you done any measurements with neutron access tubes, for example, or something like that--

DIXON: We have--I didn't mention, but we also have neutron logs of all the boreholes, and so between the three geophysical techniques and what we know from the rock based on actually measuring things, we have a pretty good idea; and using basically standardizing the techniques on some of the rocks we have a pretty good idea of what the different velocities mean and water contents.

DIODATO: Yeah, so that would be a nice--nice thing to display. Then the question becomes, in your conclusion slide, you're talking about porous media flow dominates in the vitric Calico Hills.

DIXON: Right.

DIODATO: So some questions I have are, one, vitric rocks would be more brittle, is that correct?

DIXON: No. Less brittle.

DIODATO: Vitric rocks are less brittle.

DIXON: In other words they're not welded as much. Vitric rocks--think of them being as like a pumice block, a series of little pumice grains, just stacked, rather than pumice grains that were heated and melted together, which make a welded tuff.

DIODATO: I see. All right, thank you. Well borehole 10, how does that--you thought that you might have some structural heterogeneity--

DIXON: Can you flip to slide 8?

RUNNELLS: Gentlemen, can we keep the remainder of this very short, because we're getting close to public comment time--

DIODATO: Yeah.

DIXON: All I was going to say is there is--

RUNNELLS: --cut into the public's time.

DIXON: --there is a measured fault with offset. Borehole 10 is relatively close to that, and there appears to be a higher degree of fluids, conservative tracers being imbibed into that borehole. And we believe it's because of its proximity to the fault.

DIODATO: Thank you.

RUNNELLS: I do not want to cut into the public time. I know there are two people who want to ask questions. I'm going to defer to the chair.

PARIZEK: Real brief.

RUNNELLS: Real brief. Dick, real brief, and then-- there was somebody who wanted to clarify that timing thing again.

PARIZEK: Yeah, Parizek, Board. I guess, deals with Alberto's question of Jack Bailey earlier this morning, about the natural barriers only versus natural barriers plus waste package.

DIXON: Right.

PARIZEK: It didn't look like he got an awful lot of credit for the geology. Now with the new information you have, I'm not sure whether or not the natural barriers runs included your new information, say on the role of Calico Hills, as an example, and Kd information in the alluvium.

DIXON: In the site--in the plan that Jack Bailey presented you this morning, it does not have the data that I presented here today.

PARIZEK: So geology's better than--

DIXON: The geology is better. I mean we've been very conservative up to this point.

PARIZEK: So I just want Alberto to realize that metals are great but geology's better.

RUNNELLS: That would be a wonderful comment to end on, Dick, but unfortunately we have a gentleman who wanted to clarify further that issue of timing. Where did he go?

COHON: I think we're okay.

RUNNELLS: We're okay. Okay, then thank you very much to all of the speakers. Our great appreciation for the preparation that went into these presentations. They were excellent. Thank you for your time.

And I'll turn it back to Dr. Cohon.

COHON: Thank you very much, Don, for doing such an excellent job of chairing; and my thanks to all the speakers for a good session.

We have three people who would like to speak. We'll start with Jerry Szymanski.

SZYMANSKI: How much time do I have?

COHON: Ten minutes. Is that adequate?

SZYMANSKI: Oh, yes.

COHON: Okay.

SZYMANSKI: My name is Jerry Szymanski. On this particular meeting I am representing attorney general of the State of Nevada. It seems to me that the Board is uniquely positioned to advise the Congress, the President, what to do with this project. The key, in my judgment, is information.

It is my understanding the Board had received a letter from attorney general explaining to the Board what would be the wishes of the State of Nevada, and it seems to ask that develop a schedule whereby UNLV projects runs its course, the unanimous report is released and analyzed, and after that issue final assessment, environmental impact statement and site consideration, suitability consideration report.

It is our view that business--that DOE has no business whatsoever to travel the country, inform the public and the decision makers about the potential environmental impacts unless this question is resolved. That seems to me straightforward.

I would like to present to the Board four documents to aid the Board to understand the scientific basis for our recommendation. Upon reviewing this report it may be that the Board would choose to advise the Secretary and the Congress to reschedule these two crucial documents. After all, if these minerals are young and hot, if these minerals were being deposited intermittently over the last 10 million years, what are we looking at?

We are looking at potential catastrophe.

Now we are looking at the issue which is 20 years old. It is to the credit of this Board that project which Dr. Cline is chairing came to fruition. I credit the Board, and it is a crucially important piece of information. Everything else is irrelevant.

Some of these titanium umbrellas, they might be effective if water is dripping--if it is dripping at all. But how good they would be if we would be looking at an explosion, a behavior which is not dissimilar to what we can observe today at Yellowstone.

Now my interest here in passing these documents is to inform the Board, to provide them maybe one-sided view, agglomerate scientific data which in my judgment, saying wait, wait a minute here. Let them finish the work. That work cannot be rushed. Jean Cline, Dr. Bodnar are showing a lot of diligence in trying to obtain data which are secure beyond reasonable doubt, very meticulously documenting.

There are three parties involved. That process cannot be rushed. So there's only one solution: postpone this two bloody (phonetic) reports. That seems to me straightforward.

And second, Yucca Mountain, its geology is extremely complex. It relates more to nonlinear thermodynamics than it relates to water supply hydrology, or engineering rock mechanics.

These subjects have nothing to do with understanding dynamics, behavior and evolution of mountain. We are looking at the fundamental tectonic processes which are uniquely present at Yucca Mountain and very few other places in United States.

The circumstances have to be understood through integration of a huge amount of data. We have to look at the velocity, distribution in the mantle, we have to understand phase transformations in the mantle, we have to understand the behavior of gases and the origin of gases which are coming out of this mountain. And now we can start putting a picture together.

This cannot be done by applying the silly darcy law (phonetic) to that mountain. This is silly. That pertains to a water supply. It does not belong into a siting of the repository in a tectonically, that is fault ruptured, volcanically, that is the mantle melting in instability. It just doesn't belong there.

I'm not interested in getting comments. Most of them are not too pleasant to me for last 20 years. I'm not interested in it. My interest is to inform the Board. I do not think or do not believe that a lot of good will come out from getting again a few consultants, so-called experts, which neither know Yucca Mountain, they are not willing to digest $7.6 billion worth of geological data collected at that mountain. There's no mountain in the world which has so much data.

And moreover in that pile of data there is an understanding which is unique. You will not find an understanding in the books which were written elsewhere, some professors in Michigan. They were never exposed to this amount of data. We never had it, nowhere.

Therefore I am not interested in repeating this two failed review process. Specifically I am referring '92 National Science Academy, and the more recent review of the document which I have forwarded to the Board two years ago.

To continue with this is to invite litigation. We at the office of attorney general wish, pray, that we can resolve this issue short of litigation because it is our belief--which is very firm--the result of it would be serious embarrassment to the Congress and to the administration.

Therefore it seems very logical to me, just postpone these two reports--it's not a big deal--and allow the process at UNLV to be completed. It is a very fair process. I am committed that I will accept the results. Dr. Dublianski's committed to accept the results. I think

Dr. Bodnar is serving in a very useful role as a referee, and there can be the database developed.

And I hope that the Board members, each of them, will read the documents, especially this one in the binder which pertains to fluid inclusions, pertain to--it is in a bullet form. It's very easy to read. But it provides the Board with the information which I think is crucially important, and I think the Board is lacking this. We can be talking about this uncertainty until hell freezes over.

But I look at it--it is a joke. Having that business, when you go into the tunnel, experienced geologists immediately see hydraulic fracturing. That tells me that somewhere in that mountain there is a supercharged body of water which is hot, and charged with gas, small perturbation causes catastrophic release of the gas, and the hydraulic fractures.

-- talking about--we don't know the ages of these minerals. We do. We have an unprecedently large database pertaining to these minerals. We have lead 207, uranium, we do have very extensive database pertaining to -- uranium --, we can compute probabilities, we do know what are--and we are in agreement how hot are those minerals. Some of them are up to 85 degrees C--

COHON: Dr. Szymanski, I'm very sorry to interrupt.

SZYMANSKI: Well--

COHON: We're closing in on 15 minutes, and I wonder if you can wrap it up?

SZYMANSKI: I can wrap it up right now.

COHON: Thank you.

SZYMANSKI: Thank you very much for opportunity to express these views.

COHON: Thank you, Dr. Szymanski. And you'll give us these documents? You can just give them to Dr. Bullen there.

Thank you.

SZYMANSKI: Thank you.

COHON: Sally Devlin. Ms. Devlin.

DEVLIN: Again, Mr.--Dr. Cohon, thank you again for coming to Nevada, and I hope you'll be here very soon. I have my notes that I gave--I had in my pocket from this morning on my questions. And I really do hope they'll be answered, like the change in the map and so on.

This has been a most informative meeting, and I say that because I introduce you to the SEC and I hope I hear back from you on what they had to say, how Yucca Mountain will affect the markets and the potential for disaster.

In the EPA book, I'm giving the numbers of what the foreign countries have, except for China and Russia, and their nuclear waste piles. Everybody seems to be sitting around seeing if we're going to blow ourselves up, and it's a very serious question.

The other thing that is never mentioned, we did get one--we got a number, we got a $3 billion number for the costs of the things. And that's very important, and I think the public needs more numbers on everything. I gave you numbers in my little film, but the most important thing is confidence that we do get answers--(coughing)-- I'm sorry--I'm just so tired--to our questions and so on. And again I just want to say thank you.

The only other thing I have to ask is, nobody mentioned my bugs, and my microbic invasion I think since the Livermore study came out should be looked into. I can't understand why all this metallic stuff and the bugs eat the metal, and on the other things that you're talking about with the canisters--(pause)--

COHON: Ms. Devlin, I think they're still working on bugs. Are you still working on bugs? Yeah, DOE's nodding its head.

DEVLIN: You're working on my bugs, good. My bugs are on everything and in everything, so I'm looking forward to my bugs having more reports because they can eat the rock and the rock will collapse, and God knows what happens. They can eat the metal and so forth, and that's terribly important.

And the only other thing I have to ask is I was told at the NRC conference that this stuff is going to be put in the mountain robotically. I know nothing about that, and I'd like to learn; and that concludes it.

Again, thank you for coming.

COHON: Thank you, Ms. Devlin. Tom McGowan.

MCGOWAN: Testing one, two. Huh? Oh, okay. Self-explanatory so far up there on the wall, and I am very impressed with the art work and the major five and six and seven color renditions on many of the presentations. These presentations are becoming more professional by the nanosecond, and that's commendable because that may be about the best there is, so far.

Now--Tom McGowan--consistent with the--Dr. Bodnar's presentation, which I enjoyed thoroughly, I am firmly convinced that all women passengers on the same airplane were born on the same day and are securely interrelated, much like the inclusions on the same crystalline structure.

Dr. Cline's presentation was also highly commendable, and uniquely enlightening, since none of the samples were apparently collected in any of the 100 miles of proposed repository drifts or from the intermediate field, regional area. But then it would be inappropriate apparently to create perturbations in the whole region. On the other hand there is a limited desirability of having all the information possible about the access tunnel only.

Dr. Stuckless' presentation provides proof positive that the best underground repository for nuclear waste would be in a cavernous art gallery in an exotic foreign land such as Turkey, or perhaps even Peon, New Jersey.

Tom McGowan, Las Vegas, Nevada--I think I said that. Good afternoon. As Milton Berle would say, "Someday everybody who knows you and hates you, doctor, will be gathered in one place. And now that you're all here--no, seriously, good afternoon, ladies and gentlemen. The rest of you know who you are."

In this segment I'll address the nuclear waste priesthood element of my proposed alternative to underground storage that I referenced in the last public comment segment.

In -- Dr. Van Luik advised me that my previously referenced proposal elements are virtually identical to a current DOE program entitled ATW, which I never heard of before. True story. And that's an acronym indicative of Accelerated Transportation of Waste.

And I'm heartened by the fact that DOE is responding to congressional directives and -- start up funding. Undoubtedly in consequence of the urgings of Senator Pete Domenici of New Mexico, as advisoried by my personal acquaintances, Drs. Bowman and Vanneri of Los Alamos National Laboratory, Nobel Laureate Dr. Carlos Rubio of Italy, and other eminent nuclear physicists in Oak Ridge, Havana River, Argon Laboratories, Brookhaven, Lawrence Livermore, Moscow, Tokyo, United Kingdom, and elsewhere in the expanding universe of accelerator driven transportation technology, ADTT, which did not just fall of the truck, but in fact started quite some time ago.

My proposal was first submitted 10 years ago, which responds to your advisory about my having some kind of access to your ATW--never heard of it, doc. You're going to send to me in the mail; we can compare notes on that to other matters. So in January of 1990, yes, that was proposed by me--which is neither here nor there.

It was ignored by the state and local jurisdictions in their wisdom, but was subsequently welcomed and heartily endorsed by the First International Symposium on Accelerator Driven Transportation Technology held at the MGM Grand Hotel, just micrometers from here. In fact transportation technology had its inception in the United States in 1947. It was subprioritized while other competing interests received the bulk of research development funding. Not surprisingly.

In any case, better late than never, since a monumental task looms inevitable on a national and world wide scale. So congratulations, Dr. Van Luik, for coming into the real world apparently just in the nick of time.

And also in the interests of giving credit where credit is due, which I will always do, the phrase Nuclear Waste Priesthood reflects artistic license with reference to the earlier iteration, Nuclear Priesthood, originated by Dr. Alvin Weinberg, which was nuclear energy specific rather than nuclear waste specific. And that clarifies anything like that--we'd hate to have Dr. Van Luik sit up all night and wonder about where the hell that phrase came from. We're clear on that, right, doctor? God bless you, my son.

Comes now my full plan of viewgraph narratives like magic, summarized outline of my proposal element entitled Nuclear Waste Priesthood, which is straightforward, essentially comprised of a broadly diverse, entirely voluntary pan-denominational, non-compensated but intensely dedicated non-secular corps of individuals uniquely attained to utmost ensured quality slash integrity, context in terms of ethics, morality, reason, integrity, responsibility, and above all, conscience. That is the key, that compound right there is the key determinate between the man and the money, so to speak--or men and whatever those other things are out there.

In surplice service to the genuine best public interest inclusively and intergenerationally. And thereas pursuant to the ensured effect of safe, secure human intrusion and accessibility impervious, stewardship, management and monitoring of high level nuclear waste over hundred of thousands of successive generations, ergo essentially in perpetuity.

Ad hoc and pro tem the discharge of the duty or responsibility to securely isolate, to immobilize that level nuclear waste pending transportation based reduction and to eventual natural civilization. End of problem.

The Nuclear Waste Priesthood recognizes the absence and indeed the impossibility of ensured effective institutional controls, either extant or impending, as reasonably foreseeable.

And thereas realistically projected as ensuing within and sustainable over any enduring term, as recognized as the compelling need for it and advisability of an independent human infrastructure, aka the ad hocracy, attained to context is virtually immortal and thereas charged with the solemn duty and responsibility and so on exclusively dedicated to the preservation of integrity of the high level nuclear waste in perpetuity or until obviation or stability is attained completely and permanently, nationally and world wide.

The priesthood would be self-regenerated and self-replicated over an expanding base, and would be an independent supranational sovereign entity ascribed to the highest attainable standards of human spiritual quality, integrity, consistent with divine will, as is abundantly evident throughout the naturally ordered universe. Take a look sometime. It works perfectly whether we're here or not.

The priesthood will voluntarily ascribe to the strictest military discipline and would remain subject to self-imposed severe penalties, including capital punishment, in the instance of non-compliance with its voluntarily adopted and uniquely unforgiving code of conduct on behavioral boundaries, parameters and constraints, without exception.

In conclusion, doctor--in conclusion, doctor, vesper services will begin at 7:00 p.m. in the Yucca Mountain memorial catacombs for those of you who are dedicated to this particular pursuit. I said unforgiving, and I meant it. Unforgiving means if you don't care about this, you'd better care about something else because you ain't going to get past me, period. That's simple.

Okay, and I love you, doctor--I love all of you. But that has nothing to do with it. This is not above love. It's about life and death--not ours--theirs, and they're not here at all to talk about it. So I'll talk for them.

Thank you very much. And bye bye.

COHON: Thank you, Mr. McGowan. Is there anybody else who cares to make a comment?

Seeing no takers, let me close the meeting by thanking again all of our speakers over the last two days. They were especially high quality presentations, I think, from both within the program and from outside.

I want to thank our outstanding staff for their great job in organizing this meeting, the two Lindas who are still working at it in the back, all of our staff. But I want to single out Dan Fehringer, who is the one who coordinated the substance of this. He did a fantastic job.

Thank you, Dan.

Thank you all very much. We stand adjourned.

(Whereupon the meeting was concluded at 6:30 p.m.)