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NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING 2004 BOARD MEETING

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Dr. Daniel B. Bullen
Dr. Thure Cerling
Dr. Norman Christensen
Dr. David Duquette, Morning Session Chair
Dr. Ronald Latanision, Afternoon Session Chair
Dr. Priscilla Nelson
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DUQUETTE: Good morning. For those of you who may be new--there aren't too many of you--I'm David Duquette, Chair of the Executive Committee of the Board. We have a very busy day today, continuing on the erosion behavior of the canister and thermal pulse. Most of the, I think all of today's presentations will be by the Department of Energy and I'm not going to take too much time this morning except to say that I hope before the day is over the environment at least will be described, the environment that we're dealing with in this particular case, and whether there is inhibition or not inhibition, whether the nitrate is important or not important, whether the dust is acid or basic or whatever the situation is. I hope all of that will be addressed this morning. So, without any further adieu, I'm going to introduce the first speaker. Margaret, would you come up and make a few comments on regulatory requirements?

CHU: Good morning. There are a lot less people today than yesterday.

DUQUETTE: Yes.

CHU: It was a long day yesterday. It's going to be longer.

You know, the reason I asked for 10 minutes to make a few remarks this morning is--can you hear okay?

Okay. Well, ever since I received the letter from the Board on October 21st, I have been thinking about a statement in the letter, which actually turned out it was on the Board's view graph yesterday. The quote, unquote, "Total system performance assessment should not be used to dismiss corrosion concerns." But yesterday's presentation gave me a little more insight as to what that means but it really is the reason I want to spend 10 minutes.

You know, I indicated in my response back to the Board that we would not dismiss the corrosion concerns and I also pointed out that performance assessment is a required part of the demonstration of compliance with the safety requirements established by the NRC. And then, I would like to take a few minutes today to expand on this point because I think that's a very important point. I want to make sure we are all on the same page.

You know, under the law, the repository has to meet a safety standard defined by the EPA and implemented by NRC on 10 CFR 63. This standard is philosophically okay. The framework is a risk-based--no, I'm sorry, risk-informed--the NRC, they don't like the word risk-based. Risk-informed and performance-based standard. So I'm going to kind of dissect those two parts a little bit.

We all know the definition of risk. You know, we call it a risk triplet. What it means is what can go wrong, how likely it is, and what is the consequence. Everybody knows that.

So the regulation requires that we consider the probability as well as the consequences of all features, events and processes that could affect the performance of the repository. So this is the risk-informed part.

The second part is the performance-based. As for the performance-based part, the regulation focuses on the overall performance of the repository at a system level rather than on the behavior of the individual components separately. As you all know, the post-closure safety standard is an annual dose not exceeding 15 millirem to the reasonably maximally exposed individual. So that is the overall system level performance objective, 15 millirem dose.

And then there's another important point in 10 CFR 63. It requires us to demonstrate that there is a reasonable expectation, not an absolute certainty, that the repository will meet that standard, that requirement, 15 millirem. Section 63.304 states that absolute proof is impossible to obtain due to the uncertainty of projecting long-term performance. 10,000 years is just too far for us to know for certain. "Reasonable expectation", that quote in the rules, focuses on the full range of defensible and reasonable parameter distributions. And then it specifically says, please do not use extreme physical situations and parameter values. And so, they recognize the uncertainty and say, you're uncertain, you have to be defensible and reasonable. Don't go to the extreme.

And then, last Fall there was a presentation from NRC's Chair, Mr. Diaz, and he made a speech on this whole concept. He made a very good point. He said if you go to the extreme, are ultra conservative, go to, you know, the end point of a scenario, your answer is going to be masked by that scenario and you actually would divert your resource on looking at what's really important, what the real risks are. So, there is a balance on how much uncertainty you put in. And so, you know, I personally am pleased that we're starting to putting back, for example, yesterday you heard the presentation on seismic end because that really bothered me, when it's unlimited, unbound, because everybody knew that it was unreasonable. We started to pull it back because the reasonableness test is an important part.

So, and then the regulation says a total system performance, TSPA, is required to project a long-term dose from releases from the repository. So the regulations say this is how, you're going to show us how to demonstrate our 15 millirem, okay, of the level.

And then please bear with me. I'm going to quote you a little bit. NRC describes performance assessment this way: He says "It's a systematic--that's the important words--systematic analysis that identifies the features, events and processes that might affect performance of the geological repository." You're supposed to examine their effects on performance and estimate the radiological exposures, including incorporating the probability that the estimated exposures will occur to the reasonably maximally exposed individual during the 10,000-year compliance period.

The regulation also says, you know, if it's very unlikely then, less than 10 to minus 8, never mind. Don't worry about that. Okay, so there's some second tier.

So from this description is it clear that features, events and processes are the underlying foundation for the development of a TSPA. So the TSPA is the methodology used to roll up and integrate the effects of all the features, events and processes, and to calculate their combined impact on the expected dose. This roll-up takes into account the probability, the consequences and the uncertainties. I know a lot of people know what this TSPA, this composite thing means. It is a different concept because the final result of TSPA is a distribution of outcome. There's no single story in the final outcome of TSPA. You can't say, you can't shoot a movie paid from time zero to time 10,000 years, say this is what I expect will happen. Rather, it's a combination of a possible spectrum of evolution, of the behavior of the repository. So it's a composite story you've got to see because we can't predict something uncertain, so we can't describe the evolution and say this is how many packages is going to corrode because of this and that because spectrum things can happen. There are certainty of chemical condition, you know, and everything, so therefore, so when you do TSPA, you take that uncertainty, the probability of consideration, you sample them, that reflects the uncertainty and probability and then you roll all of them up. All of a sudden you see this big distribution of dose, and then this is really representing this whole spectrum of possibilities.

So my point is TSPA itself cannot be used to dismiss anything. If you do it right, okay? One can argue and debate on the technical basis, or the lack of it, of underlying processes that are used to--you know, before it got rolled up. And this is what we're doing later today, is the individual processes that we are presenting.

And then this important point: If a process is important to the overall safety of the repository in this composite picture, you will and should show up as important to the TSPA, unless you do it right, unless the technical basis is important. And, if something is important to certain conditions, but if it turns out that it's a minor effect out of the whole scheme, you would not show up as very impacting in the big picture.

In fact, you know, a detailed understanding of the importance of a particular engineered or natural of the are to the performance of the overall repository system is frequently best understood through TSPA. I remember when I was working on WIPP, we used TSPA to determine data sufficiency, to determine, to prioritize R and D work, and so on. Because it can be a very powerful tool to put you in the right perspective at the system.

After saying all that, TSPA is not the only factor in compliance determination by NRC. NRC will also consider several other important factors. The first one is multiple barriers. NRC says because of the inherent uncertainties, the natural barrier and the engineered barrier system need to work in combination to enhance the resiliency of the geological repository. So therefore, if some component turns out it's not important to TSPA--waste pack for example, that there's some questions, can you ignore it? No. NRC says you have to have that because of multiple barrier concept.

Now the second factor is the Quality Assurance Program, and we're all very familiar--I want to make sure absolutely sure you have done what you say you have done, it's well documented, following this rigorous procedure. So that's the confidence NRC needs to have.

The third factor is the performance confirmation program. Is that you have putting all these assumptions in the data and analysis in your license application. It may look good during the license application time, and then they require us to plan a long-term performance confirmation program and say we want verification, we want you to monitor the important assumptions in data, so we know because 50 years down the road, we know there's nothing wrong.

And finally, you all remember that there's a retrievability program required by NRC in case everything you have done is wrong, okay? People all did the wrong stuff. They want to have a retrievability option open, can always go and retrieve everything so there will be no disasters. So this is sort of the whole scheme of the philosophy of 10 CFR 63.

Now, today we're going to focus on some of the key corrosion-related processes that are considered in the TSPA. You're going to hear today what we consider as the technical basis for the treatment of the in-drift environment and corrosion.

I want to re-emphasize that the fact that a process like certain waste package corrosion mechanisms might occur in the repository justifies the inclusion of the process in the determination of compliance with safety requirements. However, the existence of such a process does not by itself determine whether the repository will or will not comply with the standard. Both the probability and the consequences of that process, together with associated uncertainties must be taken into account through the TSPA to determine how important that particular process is in the context of all other processes that contribute to the total risk from the repository. So I just want to remind everybody, today is very important. Corrosion is a very important mechanism. But it is the one process out of very many eventually we have to roll out. And then, this is the essence of the risk-informed performance-based standard that the repository must meet.

So in conclusion, you will hear technical presentations related to corrosion during thermal period later, we will not present TSPA today. However, today's presentations are the underlying processes that we will need to roll up together with all other possible processes into the TSPA. And we will discuss the TSPA conclusions in more detail when we present our work to the Board in our September meeting. Thank you.

Any questions? Is my time up?

DUQUETTE: Duquette, Board. Margaret, thank you very much for your comments. I'm speaking for myself when I say this, certainly not the Board. But I don't think you should take the comment in the letter about TSPA out of context. I don't think anyone on the Board wants to dismiss TSPA under any circumstances. I think the letter identifies a potentially avoidable problem with the repository. And, I think that was really the thrust of the letter. Again, based on information that had been given to us, it isn't clear, for example, whether the environment will inhibit corrosion. The data that was available to us both from the center in San Antonio and from your own researchers who indicated that there was a potential for corrosion. I think that's all the letter identifies at this particular point. I'm really glad to see the effort that the department, that EPRI, the other, that the state and NRC to put into addressing this problem. But I want to say one more time it's my opinion that TSPA is not dismissed by the Board under any circumstances. We fully understand that TSPA can be satisfied without even considering the corrosion issue, but I as an individual am very concerned about compromising any of the barriers if it can be avoided. And that really is where I think the letter was coming from.

CHU: Thank you, and, like I say, yesterday morning, and we really appreciate that, and it really helped us to focus some the work, new work, we're actually turning out some new work to address some of the issues. And what I want to emphasize today is these are eventually, it's due to roll-up of all the process that's going to determine the ultimate outcome, but of course, in the meantime, we want to have the best technical defense of all the underlying processes. Corrosion is a prime, critical process that we are addressing. You know, we take those technical issues very seriously.

DUQUETTE: Thank you, Margaret.

CHU: Thank you.

DUQUETTE: Dr. Latanision?

LATANISION: Latanision, Board. I just want to echo what Dave Duquette has said. You know, as an engineer I'm pretty pragmatic and I look at TSPA as being obviously a very important and powerful tool, but if I'm in a position where I have the choice as an engineer of avoiding a problem, which I believe I can do through a design decision, and I have as the alternative using a predictive model, a predictive process to help me in assessing whether some event has a high or low probability and what the consequences might be, I'm more apt to choose to avoid the problem.

CHU: I understand.

LATANISION: And I think that's really in a nutshell where we're at. I mean it's not dismissing the importance of TSPA. We recognize that it's an important tool, it's part of the compliance process and we're fully on-board in that context.

CHU: I fully appreciate that point. Thanks.

MR. LATANISION: Thank you.

DUQUETTE: Thank you, Margaret.

CHU: Thank you.

DUQUETTE: I think we'll get the meeting back on schedule. The next presentation is an introduction to presentations addressing the Nuclear Waste Technical Review Board's comments in a November 25th, 2003 letter, which is what we're talking about. That's going to be presented by--Robert or Bob?

ANDREWS: Bob.

DUQUETTE: Bob Andrews. Okay.

ANDREWS: Okay, thank you and good morning. It's an honor and pleasure to be here to introduce these three fine young men who are going to be following me with the actual technical discussions. You can pay me later for the compliment. Bo, Carl and Joe, and I'll talk about what they are going to say as an introduction and they will provide the technical details and bases for their conclusions and the additional data and analyses that have been conducted, not only to address the Board's questions but to address, more importantly, the role of the environment and the role of the waste package and the corrosion degradation mechanisms as they affect the repository facility, not only from a performance assessment, a total system performance assessment perspective, but from how it's going to be documented in the safety and analysis report that John Arthur talked to you about yesterday.

So if I can have my first slide. What I want to do is just give the background, which I think was gone over a little bit yesterday, but I want to go sort of point by point through the sequence of events that led us to the November--led us all to the November 25th letter, and to today, essentially, go to what we're going to talk about today and what we're not going to talk about today, and then introduce the subject matter and the speakers in the context of comments made in the letter itself, and in the executive summary of that letter so we can put it into direct focus with respect to the comments that we are addressing today. Next slide, please.

As was discussed yesterday, the Board was presented with a lot of information last January, January, 2003, and May of 2003. January kind of focused on corrosion, corrosion testing, information available as of

January of 2003 with respect to corrosion testing in a range of environments.

That was followed up by the May presentations which generally focused on three parts, which focused on seepage in a nature environment that affects seepage into drifts because if there is no seepage then the effects of the chemistry in the rock are somewhat mitigated because the chemistry of the rock never get into the drift. So we had the discussion on seepage and thermal seepage. We followed that with a discussion on chemistry, chemistry in the rock and chemistry in the drift and the evolution of that chemistry in the rock and in the drift as it affects the degradation modes of the materials that are in the drift.

Then we followed that with a third presentation in May on the data available at that time on testing in a range of environments with localized corrosion testing, crevice corrosion and general corrosion and evaluations of Alloy 22 degradation in the range of environments.

Those presentations the board commented on in a letter in June identifying some potential questions, potential, I think the way they phrased it was some initial reactions associated with the presentations that were in May. I think the department then--next slide, John--responded to that in October concluding that there was insignificant corrosion above the boiling point, in large part, due to the lack of seepage during the thermal period, and the primarily benign deliquescent brines. When things did deliquesce, those brines were generally of a non-corrosive nature, i.e., high in nitrate concentration. And they also acknowledge, or the Department acknowledged that there was insignificant corrosion below the boiling point of water primarily because of benign seepage conditions. So when you got to that point in the thermal hydrologic evolution where seepage could occur, the chemistry of the fluids in the rock that could come into contact with materials in the drift was also benign.

October 21st, the board provided a letter to the department summarizing their conclusions related to corrosion. October 27th, we responded noting that the corrosion testing that was presented at the May meeting and some subsequent testing that I think had been provided to the Board subsequent to that May meeting through its series of interchanges with the Board provided an incomplete picture of the overall corrosion story in the range of likely environments that should be considered.

On November 25th, the board did write their report which they had mentioned in the October letter, and it's that report with its conclusions that we want to address today. It had a cover letter and then the actual body of the report, and as noted yesterday, there was an attachment to the report by one of the board members acknowledging three other issues that he felt were relevant to the discussion of deliquescent brines and the effects, potential effects of deliquescent brines on the performance.

So today, what we want to do is answer the questions, the concerns, the issues raised in that November 25th transmittal from the board to the department and to particularly focus on those aspects during the thermal pulse, as it has been described, although we haven't defined thermal pulse. The repository is in a thermal period for its whole time period, will be in a thermal period through its whole time period no matter what thermal loading strategy might be, so we will focus on information that affects repository performance during the whole thermal period today.

We've broken this series of presentations into four presentations, and let me walk through the logic of why we broke it into four. First off, the degradation characteristics of Alloy 22, I think everybody acknowledges our function of the environment in which the waste package sits. That environment is a thermal environment, it's a hydrologic environment, it's a chemical environment, and in fact, it's a mechanical environment as well. And those thermal, hydromechanical processes and conditions do affect the degradation characteristics of the Alloy 22, as do some other engineering aspects associated with the formation, construction, fabrication of the waste package itself.

So, but we don't want to jump into Alloy 22 degradation. We want to put it into the context of the environment in which it sits. That environment is, first off, affected by the chemical environment but the chemical environment is in fact affected by the thermal environment. And we broke the chemical environment discussion into two separate parts that Carl is going to talk about. The part this afternoon is reserved for kind of the aqueous chemistry, the chemistry evolution in the rock and the chemistry evolution in the drift in an aqueous type environment, i.e., generally in the below-boiling type conditions.

This morning we're going to directly focus, because the board's letter and executive summary focused on the deliquescent part of the chemistry. That is the period when it is not only dry, there's not aqueous conditions from the rock in the drift, but that we do have dust and dust will deliquesce. Salts in the dust will deliquesce at a range of different humidities and have different conditions with respect to the corrosion mechanisms on Alloy 22.

So Carl Steefel will talk specifically about the dust issue, the chemistry of the dust within the thermal hydrologic environment that exists in the drift because you cannot separate how the dust behaves from the temperature and humidity conditions in which that dust and the salts within that dust are sitting. So we wanted to walk through the hydrology and thermal hydrology first, then go to the dust part of the chemistry second, go to the aqueous part of the chemistry third and finally wrap it up with what does all those environment issues mean with respect to the degradation characteristics of Alloy 22. And then I will have some concluding comments at the end of the day.

If I can go to the next slide.

In addition to talking about what we are going to talk about, it's useful to talk about what we're not going to talk about so we can also put this into context.

We're not going to discuss all of the board's questions in the report. Our aim was in the time allotted today was to focus on what was deemed to be the most crucial aspects in the cover letter and in the executive summary, which was the deliquescence of brines and thermal evolution that affects that deliquescent of salts in the repository environment during thermal pulse.

So there are a number of other aspects raised in the Board's letter, and we're not going to focus on--or the report, I should say, and we're not going to focus on a number of those other elements. We would be pleased to do that at a later time if desired by the Board.

We're also not going to focus on other elements of repository performance. I'm going to have a brief introduction to some of them here in a few slides, but our focus is not on other elements of the system. I think the Board recognized both in the cover letter and in the report itself that there are other elements of the system that affect the overall repository performance, that were also not the focus of that letter, that could affect the results of the total system performance assessment, and in fact do affect the results. So we're not going to discuss those.

And I think it's also important to point out that we're not going to discuss the low probability, disruptive type events that could affect the characteristics of the drift after the repository is closed. One of those we talked about a little bit yesterday. That's the seismic event. We talked about the initiation part of the seismic event yesterday, the consequences, the effects and consequences of a seismic event, potentially a low probability seismic and high velocity seismic event which can affect the degradation of the drift and can affect the thermal environment and can affect the performance of the drip shield and the waste package in that environment, are not going to be discussed. Our focus is on the most likely performance, i.e., the probabilities of 99 percent, 99.9 percent of the time, not the .1 percent of the time that may occur if a 10 to the minus six seismic event, for example, occurred in the first thousand years of repository operation. So our focus is going to be on what we think is the most likely and the uncertainty in that most likely occurrence, not the low probability features, events, things that also have to be considered in the post-performance assessment.

And I'm going to say that we have a day here. And a day might seem a long time, but I can tell you each one of these speakers, we had to trim down their stuff to get it into a day. So there is additional information available. That additional information is being provided in the updates of the analysis and model reports that John Arthur talked about yesterday, and actually would be included in the SAR, the safety analysis report. So, we're focused on answering the Board's questions in the time allotted and there is additional information available that sometimes the members will talk to. Next slide.

Okay, it's probably useful to put this into some overall context. I don't want to say system performance assessment, but a system's context, to first acknowledge that the whole repository system is built of a large number of features and those features act as barriers and there are three requirements for barriers. Has nothing to do with system performance, but has to do with keeping water away from the waste, limiting radionuclide release and delaying radionuclide transports, so there are a number of features in this system that provide one or more of those functions from a repository performance perspective.

We're going to focus in on one of those features today. That feature is the waste package, effectively. And that waste package is affected by the environment in which it sits. It is affected by the Allow 22 degradation, it's affected by the temperature, by the humidity, by the hydrology, by the mechanics, i.e., the stresses that it sees, and by the chemistry. But we're not going to talk about the other features, but rest assured those other features and components are included in the overall post-closure safety analysis that will be described in the safety and analysis report.

Within each of those features, and in particular the features in the drift, there are a number of processes. Thermal mechanical, hydrologic chemical processes that go on. They go on with time. They will go on with time no matter what design is chosen for this facility, things will evolve. Temperatures will evolve, chemistries will evolve, hydrology will evolve. Take a simple example of climate.

We know climate will change and the evolution of that climate change has to be factored in and included in the assessment of how the repository behaves during the 10,000-year regulatory time period.

These processes are all describe by models. Those models are built on data. Those data are, in some cases, in-situ data when that's available, sometimes by laboratory test type data, sometimes by analog information, and sometimes by other literature information. And in some cases those models are substantiated and supported by other lines of evidence including work done by others. And some of our presenters today will present some work done by others, not the project, to support and evaluate the applicability of their models with respect to other people's tests.

Bo, for example will talk about some tests done at the Center with respect to thermal hydrology and thermal seepage, and a comparison of our model with their test and its information and the relevance of that test and information to our projections of repository behavior.

Joe will present some information also from the Center on their testing with respect to localized corrosion and critical potentials and corrosion potentials.

Carl will present some information from Catholic University and the relevance of that with respect to the environments that we think are going to evolve within the drift. So we're not just using our own data and information, we're using that information that's available from others as a comparison and a test, if you will, of our own models and analyses.

There is uncertainty in all of the above. All of the models have uncertainty, the data have uncertainty, the parameters have uncertainty, and those uncertainties need to be addressed in the assessment of compliance with the Part 63 requirements.

There's also, as I said earlier, some unlikely events that have to be assessed if their probability is greater than 10 to the minus 4 of occurring during the 10,000-year time period. And all of that, as Margaret said, has to ultimately be rolled up into an assessment of risk because the compliance measure for Part 63, for Yucca Mountain, is a risk-based performance type standard, so the uncertainties, the low likelihood of features and events have to be considered and addressed and included or appropriately excluded from the assessment of the repository performance.

So if I can go on to the next slide. And what I'm going to do is on the left side have the actual Board's quotes from either the executive summary or from the letter itself, I think Dr. Latanision yesterday morning summarized the Board comments and I think the speakers, Dr. Cerling followed that up with then summaries of these, but I've put the actual quotes that are going to be the focus of our following discussion.

Okay, I think I will read these actually, even though they are up there. "The Board believes"--this is in the cover letter--"that all the conditions necessary to initiate localized corrosion of the waste package will likely be present during the thermal pulse because of the deliquescence of salts on waste package surfaces, and thus it is likely that deliquescence-induced localized corrosion will be initiated during the thermal pulse."

They go on to say, "Limited data examined to date indicate that dust, which would be present on the proposed tunnels and which would be deposited on waste packages, contains calcium chloride and magnesium chloride salts in amounts sufficient for the development of concentrated brines through deliquescence."

And finally, "Corrosion experiments indicate that localized corrosion is likely to be initiated if waste package surface temperatures are above 140 C and if concentrated brines such as would be formed by the deliquescence of calcium and magnesium chloride are present."

So for each one of those, we've broken it up into the relevant parts of those sentences and the relevant parts of the talks. It's impossible to talk about deliquescence of salts without understanding the temperature and humidity in which those salts are sitting. So Bo will present the hydrology part, including the temperature and humidity and the range of likely temperature, humidity profiles that are existent in the rock. And, in the drift on the package.

And, it's important to put those into context. These are results that are plotted in a little bit different way. We'll see when you get to the presentations and what we've presented last May, fundamentally they are the same. There's a little more uncertainty included in thermal hydrologic response that Bo will talk about, but we're going to plot them as temperature humidity profiles rather than temperature time or humidity time profiles because each package will essentially see its own temperature/humidity relationship and it's that package and its response to that temperature and humidity that's in fact important that we want to talk about. So Bo will put that into context, so stay tuned for the temperature and humidity relationships as they change with time through the repository life.

Carl will then present information that neither calcium nor magnesium chloride salts are present in the

Yucca Mountain dusts. So the premise that there are calcium chloride or magnesium chloride salts sitting on the package Carl will discuss and discuss why that's not true.

Carl will also present information that, even if those salts existed in our environment so we had calcium chloride or magnesium chloride salt sitting on the waste package that would quickly transition to a more stable phase, either through acid gas evolution at higher temperatures or even under nominal temperatures and humidities, i.e., ambient temperatures and humidity. It would evolve into a more stable phase.

Joe will then present information that, given those conditions, that the probability of localized corrosion being initiated during this time period is extremely low and that widespread corrosion, localized corrosion as discussed in the Board's comment does not occur, for a variety of reasons, and Joe will present the data that support those conclusions.

Going on to the next Board comment in the executive summary, the Board had comments associated with the thermal effects, and I'm presenting this in the order that they are in the letter rather than ranging it by temperature and chemistry and corrosion because that's the way the letter was written. "The board believes that temperature calculations may be inaccurate because (1) the

DOE's rock mass thermal conductivity estimates for the lower lith may be too high, the insulating effect of rockfall and drift degradation are not included and the effects of in-drift, in-rock natural ventilation and air circulation have not been accounted for."

Bo will present additional information today with respect to the thermal conductivity and the incorporation of thermal conductivity and its uncertainty in the projections of the thermal hydrologic response. Mark presented some of the data upon which Bo will describe the models yesterday and those data are data available in the lower lith at a range of different scales. Bo will also talk about the scale effect and the appropriateness of scale when analyzing the thermal hydrologic response over the scale of meters and tens of meters rather than centimeters or tens of centimeters.

Bo will also discuss how the effects of drift degradation, especially the high probability drift degradation effects are included and addressed within the thermal hydrologic and thermal seepage and seepage assessments themselves.

Bo will not discuss, as I said earlier, the effects of the very low probability of drift degradation responses. I think it's fair to say that extremely low probabilities of 10 to the minus 6 over, per year which is

10 to the minus 2 over the lifetime of the repository at very high peak ground velocities. I think the Board has been presented results in the past that show that drifts can have a significant degree of degradation. The probability of that occurring during the thermal pulse, during, let's just say for the first 1,000 years, to make our calculation easier, is .1 percent. So we're going to focus, as I said, on the 99.9 percent of the time, not that .1 percent of the time.

And finally, with respect to in-drift convection and natural convection, the effects of in-drift convection we're not going to talk about. There are condensation effects. We have condensation included in the analyses, but the effects of condensation on chemistry and the effects of condensation on, you know, hydrology in the drift are not relevant, so we're going to delay that discussion, too.

And the effects of natural ventilation through the rock on the thermal hydrologic response, I think as acknowledged by Dr. Cerling yesterday, probably have the effect of decreasing the predicted temperatures from the temperatures that we're going to project. I don't know if that's conservative or non-conservative, to be honest with you, but it has the effect of over-predicting the temperatures, so we're not going to discuss the effects of natural ventilation, natural convection on reducing the temperature projections.

The next one, seepage chemistry, and we're hitting on these because, even though seepage chemistry does not affect the deliquescence, they are two separate issues of how the dust deliquesces or salts in the dust deliquesce in the absence of liquid water coming in is one part of an issue and seepage chemistry and aqueous chemistry is another part of the issue. The Board did raise it as a question in their executive summary. It wasn't in the letter itself, but it was in the executive summary. We thought it was important to put this in a holistic context rather than only focus on dust deliquescence, but to broaden it to the whole evolution during the thermal period, which can include seepage chemistry.

"The Board believes the possibility of seepage where the rocks are above boiling cannot be excluded, but that seepage would most likely be limited. The DOE's analyses of water chemistries and their corrosive potential are extremely complex and suffer from empirical and theoretical weaknesses. Thus, the Board does not have a high degree of confidence in DOE's conclusion that any seepage water would be dilute or noncorrosive, because the methods used in the DOE's analyses have significant technical uncertainties."

With respect to this particular question, Bo will first off address the thermal aspects and thermal seepage aspects, present additional evidence in comparison to alternative models, in this case to an NRC representation of their test data, so using multiple lines of evidence to evaluate the thermal hydrologic, thermal seepage evolution in the rock.

Carl will go on and present additional information to support the theoretical basis behind the evolution of chemistry in the rock and the evolution of chemistry in the drift were that chemistry to come into the drift, to provide that additional information to support the models that have been used in the propagation of the analyses of those models that have been used.

Finally, to put it into the context of what does this mean to corrosion, Joe will provide additional information of the degradation characteristics of Alloy 22 in that thermal environment with that chemistry and the range of chemistries that are likely. The uncertainty in those chemistries that are likely and have to be considered. Next slide.

With respect to significance of localized corrosion, "The Board believes that experimental evidence is not adequate to demonstrate the corrosive conditions which will be present only briefly. The DOE has not established whether nitrate will inhibit localized corrosion over the entire range of temperatures in which brines could exist. Furthermore, based on the DOE's estimates, the Board believes there is ample chloride to cause a significant amount of localized corrosion."

And they go in the cover letter to say, "Crevices are widespread on the waste packages, arising from their design as well as from contacts between the metal and dust particles."

So before Joe can talk about localized corrosion, as I said several times, we have to put it into the context of what are those environments. So Bo and Carl will present the environments, thermal environments, humidity environments, the seepage environments and the chemistry environments, both during the above-boiling phase and below-boiling phase.

Carl will discuss the amount of chloride that actually is present during the thermal period in the rock and in the drift if it were a salt, salt bed and deliquesced.

Then, that will be followed with Joe presenting the corrosion information. There's not a lot of additional information collected since last May on a range of corrosion potentials and critical potentials, in a range of environments to better define where localized corrosion is likely to be initiated and where localized corrosion is unlikely to be initiated.

Joe will also discuss whether the relevance of dust with respect to severe crevices and a comparison of the dust against the severe crevices that are used within the laboratory testing program.

The Board summarized their comments in their cover letter by saying, "Thus, the board believes that under the conditions associated with DOE's current high-temperature repository design, widespread corrosion of the waste package is likely to be initiated during the thermal pulse."

Obviously, from all the previous slides that I've presented, and the information that will be presented following me, from Carl and Bo and Joe, the DOE will conclude that widespread corrosion of the waste package will not occur during the thermal pulse, so we disagree, obviously, with your conclusion.

In summary, the Board's, as I think has been noted several times yesterday and again this morning, the Board's letter and cover letter, executive summary and report reflected the information that were available to the Board essentially last May. There was some additional information presented through last summer that essentially reflected a snapshot of available information through last May and into last summer. There's a lot of additional information that has been collected, a lot of simulation, a lot of integration of information that was maybe difficult to present last May when the analyses and model reports upon which the information that was to be presented were in fact not even in draft form at that particular time.

And, there were some issues that were not explicitly presented in May that I've listed here. For example, we did not explicitly talk last May about the effects and magnitude of drift degradation. I think there had been previous earlier presentations to the Board prior to last May on drift degradation effects that were preliminary analyses that have since been revised with updated models and analysis.

We did not explicitly last May talk about thermal hydrologic response. It was implicit in starting with the temperature time profiles and humidity time profiles, but we did not talk about the thermal hydrologic models, the bases of thermal hydrologic models, the validation of the thermal hydrologic models or the uncertainty propagation of the thermal hydrologic models last May.

Nor did we talk about the composition of the deliquescent brines. We acknowledged that there was salt last May. We acknowledged there was chemistry in that salt last May, but we did not explicitly talk about what that chemistry was, what the salts were and how those salts evolved and could deliquesce and what their likely chemical condition would be were they to deliquesce. It's interesting to go back and look at the transcript of that meeting and see there's a lot of lively discussion on this particular issue towards the end of the meeting that kind of got chopped sort of at the end of the meeting, and hopefully that won't happen today. But, we never explicitly talked about the chemistry evolution of the dust on the surface of the waste package.

And, in addition, the last bullet there, there's a lot of additional information that we're pleased to present today to the Board to address their specific questions but that additional information also improves the technical bases of the entire safety analysis and entire technical basis behind the total system performance assessment and ultimately improve the technical bases of the safety analysis report that will be submitted to NRC at the end of the year.

So with that I'm going to stop and entertain any questions for me or we can wait for the more technical presentations.

DUQUETTE: Questions from the Board? Dan Bullen?

BULLEN: Bullen, Board. Can we go to slide 2 on the left-hand screen, please?

First, let me compliment you on a very detailed and complex TSPA that I know you guys are putting together for the license application, but I kind of have a question for you and we'll put it in the context of, we'll take it out of the time frame necessary to end this fiscal year or calendar year, and say, in the future, if some unnamed regulatory agency should ask you, how much effort would it take to evaluate a cooler design? What significant changes would you have to make to this TSPA and how long would that take if you got an RAI from that organization?

ANDREWS: Let's not answer the time part, but it is fair to answer what things would change. The thermal response of the repository affects the degradation of the rock mass around the repository. It affects the seepage and thermal seepage analyses, it affects the chemistry evolution in the rock mass, and in the drifts. So those aspects which would be in, either by the way, but a technical basis document--we had that presentation to the Board last Fall, so technical basis documents 3, 4, 5 and 6 would be impacted and the analyses and model upon which they are based would be impacted.

I do not believe personally any additional model validation, you know, is required. I think the models are valid over a range of thermal conditions. But additional analyses and calculations and designs would be required, which would take a significant amount of time. And, just to set the record straight, the TSPA might be complete, but it's not complex. And I think we have an opportunity to talk to the Board in September/October time frame about the TSPA and we are going to welcome that opportunity.

BULLEN: Bullen, Board. I guess just as a follow-up to that, I'm a little bit confused by the fact that you didn't want to address time. If the repository at some point is going to be cool and you need to model a cool repository, how difficult is it to do that at a different time frame in the lifetime of the repository?

ANDREWS: Well, I'm not sure I exactly understand the question, Dan, but how it got to cool makes a difference.

BULLEN: I understand. But if it never got to hot, is there a very large change in the model requirements and the time necessary to do that?

ANDREWS: No, not the model--that's what I said, the models are essentially unchanged, but the analytical bases has to still be performed. In other words, the calculations of how you think it responds as a function of time. Because the repository will respond, you know. There will be--you are stressing the repository by the opening of holes and introduction of waste that has heat, so you have to analyze that response mechanically, thermally, hydrologically and chemically because that affects how the repository behaves.

BULLEN: Bullen, Board. I guess the follow-up to that question is is the analysis for a cooler repository significantly different, more difficult, less difficult?

ANDREWS: The same.

BULLEN: Okay, thank you.

PARIZEK: Parizek, Board. Bob, I found your presentation very clear and more or less the whole road map of how the day is going to proceed. And for one, it would be helpful if, as each presenter gives their part of the story, that they indicate what's new, what new data, because you imply that you have done a lot of additional work, and that's helpful to the Board because we're analyzing on the basis of what we had. If a lot of new information is to be provided, if the speakers could sort of remind us of what's new because there's a lot going to happen today and it will be kind of hard to keep track of all of this.

ANDREWS: I'm sure they will.

PARIZEK: And then I hope we will get a copy of Margaret's comments as well.

ABKOWITZ: Abkowitz, Board. Bob, I was just reflecting on the EPRI presentation yesterday and some of your opening remarks and I've sort of leafed through some of the summary positions that the remainder of the speakers today will be talking about. And, you know, there seems to be a very emphatic statement coming back to the Board that this is a non-starter. And so I was curious that if the uncertainty in all of this area has now been removed, is there any consideration that you'll go to a hotter design? And if not, why not?

ANDREWS: Well, I won't speak for the Department on whether they're considering a hotter design or not. I can tell you the bases for the license application, which is our near-term goal and objective, is the design that was presented to the Board in January, and the analytical bases for that has been presented to the Board in a number of presentations, including the ones today. So there's no short-term, you know, plan to, if you will, go to a hotter design. Is there any reason why you wouldn't go to a hotter design? No technical reason necessarily. There might be some point in the temperature profile where you don't want to be for other reasons, you know, design operation type reasons, ventilation type reasons, or whatever. But I'm trying to analyze the design that we have and provide the bases for the performance assessment and the barrier analyses that are required for the safety analysis report right now.

BULLEN: Bullen, Board. Just to follow up on that one. I think there still is a temperature limit, isn't there, at the clad credit? I mean do we want to not go above 350 for the cladding so that's sort of the peak repository temperature and does that not drive it or is there--

ANDREWS: There are some design requirements of that nature for the cladding and for the drift wall that are not performance-based design requirements, though, right now.

BULLEN: Thank you.

DUQUETTE: Questions or comments from the audience?

None. I want to thank you for not only being on time, but being early. I think this is the first time in this Board's history since I've been on it.

The next speaker is on Thermal Hydrologic Environments and Thermal Seepage by Bo Bodvarsson, who is the Director of Earth Sciences Division of the Lawrence Berkeley National Laboratory. Bo?

I would remind the speakers, by the way, to use the microphone as much as possible. These microphones have very little throwing power.

BODVARSSON: Okay. Good morning and thank you for the introduction. Just like Bob said, it's an honor to be here and talk about the recent Board topic. It was clear, like Margaret said this morning, we have taken your comments very, very seriously and her instructions to me and the others is to do a lot of work and also show you all the new things we have done since our last meeting. So I'm going to try to emphasize those. If the Chairman allows, I don't mind questions during the presentation at all. It's up to you, to clarify things as we to along. I would actually prefer that. It's up to you.

DUQUETTE: Unless it's really a burning question, I'd just as soon wait until afterwards.

BODVARSSON: Okay. Now, the outline of my talk is shown there on the right-hand side, on the left-hand side. It's very, very simple. I'm going to talk about the capillary barrier, I'm going to talk about the vaporization barrier, and I'm going to talk about in-drift thermal and humidity conditions.

The Board had a lot of comments about all these factors in their last letter. Like Bob said, some of it is due to the fact that we have clarified our position with new plots, new paper, new information, and hopefully I'll show you most of those today.

So, first let's go into the capillary barrier. These man, Yvonne Tsang helped me prepare this talk. Jens Birkholzer and Tom Buscheck and Stefan Finsterle of LDL have been really instrumental in the analysis of the capillary barrier, thermal barrier and in-drift PH conditions. Next one.

So you see there on the left--oh, can we go back on the left-hand side there. Yeah. First, I wanted to show that we have done really extensive testing of seepage in the repository area, and if you can click it again you can see the other niches, in all the locations really have done testing. So I want to really highlight that there are multiple occasions where we have done tests and on each of these locations we have done multiple tests of those locations. They include permeability ranges over pore water ranging from 10 to 100 darcies down to a few millidarcies. So we have covered a very wide range of hydrological conditions.

You will also see in the red there that we have done extensive testing in the lower lithophysal. This was one of the misunderstandings by the Board was that we didn't do seepage testing in the lower lithophysal. We have done lots of testing in the lower lithophysal. This I'll explain to you a little bit later. So I just want to impress on you that we have done many, many tests at various locations, different heterogeneities and different rock conditions. Next one.

So, the capillary barriers and the thermal barriers, I want to explain these a little bit to you. On the left is a capillary barrier, on the right is what we call vaporization barriers. These are redundant barriers.

That means they can both help us. The one on the left, this one here, the fact is that water does not want to go into the drift. It wants to flow around the drift as is indicated by the water and it's simply because water doesn't like to go into big openings. This is very well known in physics and I think the Board acknowledges this, that this is going to happen and we have done very many tests. These are very, very conservative because are testing basically, we put water right on top of the niches and boreholes just about a meter from the niches, so we are almost forcing the water to go into the niches during testing. So the testing is very, very conservative when we calculate seepage into the drifts.

The other barrier is also quite interesting because it occurs at a different location. Here is where you have the thermal dry-out zone where the rock is basically dry, where the saturation of liquid water in the matrix is practically zero. The same in the fractures. And the water trying to get into this zone will just simply vaporize again and move away from the drift. So there's a lot of shedding and I'll show you later that the saturation increases over here on top, and the fractures are going to be very, very small because the water is just going to go up and then shed around the niches. So almost all of the vaporized water that forms a steam and condenses out here is shedded away between the drifts. Well, this is a very, very powerful barrier. The temperatures here, on the order of 120, 140 degrees and out here might be cooler, on the order of 96 degrees or so.

The other thing I wanted to mention a little bit here also is gas pressures in the drift here. There has been some suggestion about the gas pressure would increase substantially in the drift. That's not the case. It will only increase by 10s of 100s of Pascals. This is very, very small, and all of our models, including the multi-scale model, including the thermal hydrology model, they all agree with this, that this pressure increase is going to be very, very slight, indeed.

The other thing also worth mentioning here is that the conditions in the drift during this boiling period are going to be primarily steep. There's going to be very little air there. It's all going to be displaced by steam. So the mass fraction of steam here is going to be about 95 percent or so, so very little oxygen is going to be available there or here because it's all steam, because steam is generated in the rock here and steam flows into the drift so this is full of steam. So next slide, please.

So I'm going to start and try to clarify some of the concerns of the Board that they raised in their letter. First one is this one, capillarity is a well-recognized phenomenon in unsaturated rocks. So they acknowledge that this occurs. But DOE has not demonstrated that the conditions required for a capillary barrier to form are satisfied throughout the drifts. The DOE view is based on insufficient data in modeling. And my first slide on the left-hand side there that shows you all the data that we have collected, you kind of tend to disagree with this. We have collected a lot of information over a wide variety of test conditions, different permeabilities varying from four orders of magnitude, so we think the data that we have are substantial.

Next one on the other side, please. Right side.

Well, this is again the data. So I just wanted to show you again all the information and all the data that we've collected. So now I want to go into these slides here which is the left-hand side and go in details responding to or trying to clarify some of the concerns from the Board.

Actually, this is not the right one, John. This is one on the thermal seepage. Must have gotten out of order. Where's the one on the ambient seepage list? Can you go--just the--Slide 9? Yeah, that's excellent.

So the Board has raised all of these issues regarding the roughness of the drifts, mass balances, seepage thresholds and all of those, so what I plan to do, if it's okay, I'm going to go through all of those separately as we go along.

First of all, though, we believe that the seepage data that we have collected is very sound and actually very conservative because we put water right above the drifts. We think that the liquid release test that provides seepage relevant data of the right scale, they reflect all the processes that are included, they reflect the roughness of the drifts because all of our drifts are rough. We think the heater test data have also corroborated the seepage results. We also believe that our modeling approach in terms of using a the permeability model is the most defensible way of analyzing the data from Yucca Mountain. We think there is a great consistency between the data and the calibration, and we think we have a proper validation as well as a proper propagation in TSPA, because all of this data needs not only to be analyzed and validated, but it has to be the correct level, uncertainty propagated, all of it through TSPA.

So, the testing and models that the testing has provided data with various degree of heterogeneity. We have looked at evaporation potential, looked at the right boundary conditions and we have calibrated the models. The models reflect the right geometry, the evaporation effects, transient effects, heterogeneity, unsaturated flow, obviously, capillary barrier effects. And then these together, we used the models to predict storage in the rock flow diversion, around the niches, evaporation and ultimately seepage obviously. This model takes into account in an effective model way capillarity, roughness effect, film flow, and then drift effects.

Okay, so let's address this first question there on the left-hand side, emplacement drifts do not have a regular curvature or profile? And that's absolutely correct. All of our niches are rough. All of our niches have fractures intersecting the niches, all of our tests have dealt with an actual in-situ conditions, so we believe that our testing and models accurately take this into account. And we also take into account the drift degradation effects, and their impact on seepage has also been evaluated using the seepage models.

This will tell you a little bit about drift degradation and as Bob told you before we are not going to spend a lot of time on this. There is a slide in the back-up if you're interested in results in drift degradation that talks about that a little bit more in terms of thermal hydrology. A few months ago, I think about a year ago, our analysis suggested that the drift degradation would be very large, that actually some of the drifts would double in size due to thermal cohesion effects and due to seismic events. This is not the case any more. I show you here a few cases for the lower lithophysal rock and you see for most of the rock, that the shape of the drift is going to be the most impact. In some cases where you have very bad quality of the rock, there is some minor drift degradation. But this is much, much less now in our AMRs than before it was, I think it was about a year ago or so.

We actually analyzed using all of our models the case of a year ago where we thought there would be a very, very big effect, so we included this in the seepage model and included this in the thermal seepage models and all of those, and like I told you there's one slide in the back-up that you can look up. This actually shows the effect of drift degradation on the thermal hydrology models.

Just to summarize for you, even if you had drift degradation, the capillary barrier is still valid. It still holds, and there's no question about it because the capillarity in the rock around the drifts, it's much stronger than that in the drift. So the capillary barrier is still valid even if you have drift degradation. The thermal barrier, the vaporization barrier, is also still valid if you have drift degradation. I can talk about that if you go to the slide in the back.

Let's take the second issue here. Surface roughness affects seepage. We totally agree with this.

Surface roughness affects seepage. It's fully taken into account in our models and analysis. Like I told you before, all the drifts are rough surfaces. They have film flow, they have fractures intersecting them, about 50 to 100 fractures intersect the drifts from the location of the injection interval and we believe that these are all taken into account in our tests.

The effect of lithophysal cavities obviously are also taken into account because we don't go in and remove them before we test. They are there. They are in place. And when we do the test, their effect on the test is shown during the seepage in those rocks. So we believe there is a very consistent conceptual model. This is used for both the calibration and protection and fully takes into account the roughness of the drifts.

The third aspect that needed clarification in the Board's letter was the mass balance in the test. And, the Board is absolutely correct that we have not had the test that fully accounts for all the molecules of water that we inject in a given test. We have various parts to the test. We inject this Qrelease, means the injection of water, and that has to equal whatever seeps, whatever is evaporated, whatever goes into the rock mass, into the matrix and whatever goes around the rock. We believe the tests were of sufficient duration to assure that any potential seepage would have occurred, and we did these tests for different time intervals to make sure that we have steady state seepage, that normal water would seep. And this is really the basis for why we think that we have the mass balance that is required to have a real good foundation for the basis of the capillary barrier tests.

We did do an attempt to actually do a fully mass balance test in Niche No. 5 where we actually put slots around the niche. We tried to catch the water that would be diverted around the niche so we would have a complete mass balance. It was very difficult to construct those slots so that test was not as successful as we hoped it would be. So right now, the flow that is diverted around the drift is provided by the calibrated simulation model. But the main point here is that the tests are of long enough duration that we are getting all the seepage that we would get for those specific conditions.

There was a comment that no seepage tests were in the lower lithophysal unit. This again could be just a problem with our presentation in May. We want to assure you there have been lots of tests done in this important repository unit in the lower lithophysal. The Niche 5, we have two boreholes there and have done a lot of long-term tests in Niche 5. We have also done systematic testing that Yvonne Tsang has been involved and done a bunch of tests in the ECRB, which is primarily in the lower lithophysal rocks. So we have lots of testing that take into account the higher fracture frequency of the lower lithophysal rock because there are more small fractures there, as well as the lithophysal cavity aspects of that rock. Let me just show you those tests a little bit.

This is the slot I was telling you about in Niche 5 where we tried to get mass balance along the niche. That is the sign of the Niche 5 and you see the slot here on the right-hand side and the left-hand side. This is the systematic testing shown here that is done in the ECRB and in the lower lithophysal rocks in many locations. And those are the slots or those are the water collection pipe of systematic in the ECRB.

These are some of the testing in Niche No. 5. And this is the seepage model that matches these tests, calibration period and we wait until it reaches steady state until we match the data. We use a very complex heterogeneity based on the air permeability test so we fully take into account the heterogeneity and permeability before we start the testing. This is included in the model right from the start.

These are the results of some of the systematic seepage testing. Here, we calculate and measure seepage and you see the agreement is pretty good. We also monitor relative humidity to estimate the amount of evaporation that takes place, and then on top there is actually injection rate. Another test. It looks like a lot of noise in the test. There's not really that much noise. The reason for this is basically is that you have a big vessel of water and you have a very small amount of water injected and so that you get a significant, what looks like moisture, but really doesn't have very much effect at all because the average is all that counts.

So, yeah, this just shows the model validation. This shows you that we not only matched the data, but we used uncertainty that we propagate from the test to TSPA, so these are five percent to 95 percent confidence interval, so you can see that with that we are very much more conservative than the actual test results. And all of this uncertainty analysis is propagated through TSPA.

The Board made a statement that the seepage threshold of 1,000 millimeters per year is too high. And again, this could be misleading by my presentation or somebody's presentation because it was just as a point of reference is to tell you that the seepage threshold in these rocks are typically on the order of 1,000 millimeters per year. But the fact of the matter is, we don't use a single number because it depends on three major factors what the seepage threshold is, when it seeps and when it doesn't seep, and that is simply given by this diagram here which is used extensively in TSPA. This diagram says that the seepage percentage is a function of the permeability of the rock, is a function of the capillarity of the rock, and is a function of the percolation flux, which means how much water is flowing through that rock. Obviously, the higher the percolation flux, the more seepage you're going to get. The higher the permeability, though, the less seepage you're going to get because more water will flow around the niche. Less water will flow into the niche, so it's inversely proportional to permeability so you see lower values here and you see more blue color here.

Obviously, the lower the capillarity of the fracture system, that means the ability of the fractures to hold onto the water molecules, the more seeps. So basically, TSPA uses all of this so there's no single seepage threshold. If you have one location where the permeability is small, let's say a millidarcie, where the capillarity of the fractures is low, say 200 Pascals to 400 Pascals, and the percolation flux is high, we may have a threshold of 100 millimeters per year or 10 millimeters per year or much, much less. Conversely, in areas of low percolation flux, high permeability and high capillarity, you will have a much higher seepage.

I just wanted to give you some results from the seepage and to show you the amount of uncertainty that is propagated to TSPA or the actual graph here. You have the percentage of percolation flux that seeps. For the ambient case--this is only for the ambient case--and you'll see here with the current climate you have something like one and a half percentage seeps. For the monsoon, you have higher seepage, for the glacial transition, the mean is something like eight. You see the huge effect of the climate predictions and obviously it's because we don't feel that we can accurately predict climate over the next 10,000 years so we have to have a wide viability in our seepage results. So TSPA will sample all the way from 1-1/2 percent all the way to 10-15 percent or higher how the percolation flux will seep, so it's a lot of variety, a lot of uncertainty in climate.

Now, the amount of water that the waste package will see if it actually seeps is given here, and it shows here, it's like two liters or so, goes up to 20 liters, goes up the mean like 30, 40 liters. And again, you have a wide range of values due to uncertainty in climate.

And, I want to emphasize here, these are ambient results. When we superimpose on these the thermal effects, you will see there is no seepage during the first 600 years or so, in general, for most of the waste packages because most of the drifts will be above boiling. And we believe, I hope to show you data and information that will show you that if the drift wall is above boiling you will not see any seepage. So these are simply ambient things, that TSPA then looks at the temperature on the drift wall and say if it's above boiling, let me put that to zero. That's how it's handled with the TSPA. Is that clear to everybody? Let's go to the next one here.

Film flow in the operation. Again, we agree with the Board; there's going be to film flow, there's going to be evaporation. This happens to be the wall in Niche 5 where you actually see film flow on the walls. This is taken into account in our seepage testing and study because if it doesn't go into our collection system, it's not seepage. It flows around the drifts. In addition to this, as you probably have heard many times, John Stuckless have used natural analogues in tunnels in Egypt for example, which have paintings on the tunnel walls that show that for many, many thousands of years you have had no seepage in these tunnels because otherwise the paint would be gone. So we use actually lots of analogue to kind of corroborate what our testing does.

Our active fracture model, there was a comment that the active fracture model has not been fully validated. Really, the active fracture model is used heavily by the project. It's most important for transport, it's not that important for seepage, so it really doesn't apply very much to seepage. It doesn't rely on the active fracture model. Of course, in TSPA for predictions of the future we use full focusing that depends to some extent on how many of the active fractures actually flow. The active fracture model is very important for transport, and the uncertainty in this parameter, again, just like the climate uncertainty is propagated to TSPA.

Another comment, persistence of the capillary barrier continues along the drift is a question, and again, I think this is a very good comment. If you don't test every location in every drift, how do you know that you're going to have a capillary barrier everywhere. Obviously, you can't test every drift in every location and every drift. But I have to say again and emphasize again, we have tested our orders of magnitude amounting to its permeabilities, we have tested our orders of magnitude in percolation flux or input of water. We think the testing phase is sound and we think we can extrapolate that testing phase with the proper uncertainty propagation to TSPA for ambient seepage.

The other thing I wanted to point out is the ECRB test where we have actually left the bolt cap closed for years now. I think it's about three years or so. And there, that's kilometer of rock. All the heterogeneity that you're going to see in the lower lithophysal and we have seen no seepage, which is consistent with the models that we have developed for the lower lithophysal rocks. So to me that's a very long-term test that should at least give us some confidence that the capillary barriers is persistent over all the drifts.

Now, I want to move on to the thermal barrier. I've finished all the comments about the capillary barrier. I want to talk a little bit more about the thermal barrier.

The thermal barrier is, as I mentioned before, a redundant barrier. The capillary barrier does not go away if you have a thermal barrier. So even if water, small amount of water, would get to the drifts, the capillary barrier will help it move along the drift, just like it did in the ambient case. But the location of the thermal barrier is much further away. The boiling, or the dry-out zone is some five, six meters away from the drift. That's where the thermal barrier is, primarily.

I want to explain a little bit about, a little bit more because this needs to be a little bit clear, the physics of what occurs here. When you boil this water, the temperature from the boiling front moves out with time. Steam is generated, steam moves out radially and condenses in the outer region, all around the drifts. Not only above, but all around the drifts. The saturation in the fracture increases from an average of three percent to about six percent. So there's no leak or anything up there for these sites here or anywhere. There is a tiny amount of increase in saturation. Why is that? It's because the rock in general is so permeable it's basically the hardest permeability is a darcy to about 10 darcies, so 10 to the minus--11 to 10 to the minus 12 meters squared. That means any increase in saturation that I see here causes water to do one of two things. It's not going to flow up, it's going to flow either sideways or down. If it flows down, it's going to vaporize again. If it flows sideways, it's going to shift. So most of it is going to shift. The only important thing here then is the chemistry of the water. We talked about this yesterday. I made a comment yesterday, if the water here is always refreshed, in quote marks, and you condense steam, because, closed quote, the old water has already shedded, then chemical concentration in this water is going to be extremely benign because condensed water is pure water without chemicals. It has to have time to sit there in the rock and pick up the minerals in the salts.

So that's a long introduction of whatever us young guys do, talk a lot. I'm the first of the young guys, then two more.

So the general statement is this: "DOE has not demonstrated, is the conditions required for pervasive vaporization barrier to form will occur everywhere. The DOE's view is based on insufficient analysis. Future testing under in-situ conditions in Yucca Mountain may improve technical defensibility or any claim about the effectiveness or lack of effectiveness of a vaporization barrier. We certainly agree any additional data will help us, but we think we have the necessary basis to go through our license application with a solid case for a vaporization barrier", and let me tell you why.

So if you can put the next one here and the next one over there. That one should be on the left-hand side. The thermal barrier, the one you asked me for.

(Pause.)

Again, we want to talk about testing. First, what are the data we have available for thermal, and I want to emphasize once more that the vaporization barrier depends on the capillary barrier, so all of the capillary tests that we have done with the capillary barrier helped us with the thermal barrier because they are redundant barriers. In addition to that, we have had the single heater test, the long block test and the drift scale test. Very long-term testing. Drift scale test is the biggest test ever conducted in the world, I believe, and is a very important test for us. I think all of us, all of these tests will help us validate the vaporization barrier. We have also done a tremendous amount of modeling to help us understand this important barrier for us.

First of all the conceptual model has been validated against seepage test data and thermal test data so I want to start by telling you a little bit about the thermal test data we have.

The drift scale test has provided a tremendous amount of data that helps us validate not only the thermal hydrologic models, but also Carl will use it to validate all of the chemical models that we have, the THC models, as well as the thermal mechanical models. It has really helped us understand these processes.

This just happens to be some temperature profiles that we used to get confidence in our model. Next slide, please. This happens to be air permeability data that we do in many, many boreholes to give us confidence that we appropriately represent the thermal mechanical effects as well as saturation changes in the rock mass.

We had also done a lot of geophysical data to verify the existence of the dry-out zone. This happens to be Electric Resistance Tomography data. You can see it shows a similar dry-out zone. We also have Ground Penetrating Radar that shows very similar things.

Also collected water in boreholes. And the water in boreholes has been extremely useful for us to look at the chemistry and how minerals are picked up on the rock, just like I mentioned before. Takes a while for it to pick up the minerals in the rocks. We see the model--this is saturation plots for a borehole and you see that we get water into the borehole when its activation is the highest, which is consistent with our models. And then the same thing here. You get water in the boreholes so the models are consistent with when we get water in the boreholes. You don't always get water in the boreholes. You only get it at these great intervals when the condensation shown has moved to that location.

So I believe, or I think we believe jointly, that the data in the drift scale test has given us tremendous confidence in our ability to model the thermal hydrology, and like Carl will talk about a little bit later, the chemistry as well as the thermal mechanical effect. All of these effects are--so we think we have adequate basis for licensing, certainly for the vaporization barrier. Any additional tests are, of course, going to help us verify these concepts. Next one here, please.

This just shows us, we talked about not having a thermal test in the lower lithophysal. I just wanted to emphasize that the drift scale test actually includes the upper lithophysal rock even though most of the tests is in the middle or left. The boiling condensation processes extend into the upper lithophysal rocks so we have some testing in the lithophysal rocks. A lot of the boreholes are located in the upper lithophysal. So it's not strictly correct to say that there has been no testing on thermal behavior of the lithophysal rocks, because that helps us a little bit. Of course, it would help us more to have a whole test in the lithophysal. There's no question about that. Next one, please.

Yeah, so this basically talks about the same thing. The reason we also believe that our results from the drift scale tests are applicable to the lower lithophysal are explained pretty much in this slide here. The primary thing that changes the response in our heater test is the thermal properties of the rock. It's not the hydrological properties of the rock. It's not the lithophysal cavities, per se. It's the thermal properties, primarily thermal conductivity. And why is that? 90 to 95 percent of the heat transfer in the drift scale is rear conduction. It's conduction dominated, and therefore the primary response you get if you go into the lithophysal is changes in heat capacity thermal conductivity. All of those are fully taken into account in our thermal models. We have accurate measurements of heat capacities and thermal conductivities which I'll explain a little bit later, and they are applied to the right units, therefore we think that the process understanding we have obtained through the drift scale test is applicable to any of them.

There has been issues with the leaky bulkhead. And, of course, it would have been really nice to have a fully closed bulkhead, no leakage whatsoever. That is not practical, of course, because steam is a gas. It's very hard to confine it in a permeable rock. It's very hard to put a totally sealed bulkhead in there. We have taken this into account fully in our models and this is a paper by Summitt (phonetic) and Yvonne that was published in General Contaminant Hydrology that has analyzed the leaky bulkhead. They conclude that less than 30 percent of the heat and energy is used to convert water into steam. Most of it is conductio, as I told you before. It would have increased the fracture permeabilities, fracture saturation some, had the bulkhead not been leaky, so we have taken into account both the cases with the leaky bulkhead and without the leaky bulkhead, and all of our data so far confirms the model prediction for a drift scale test when we allow the proper leakage to occur in the system. So we have taken this into account in our analysis. Next one, please.

The validation of the seepage threshold, thermal seepage model has been done by using ambient seepage, and also the drift scale test as we talked about before. Next one, please.

Here are some of the results of the ambient seepage model. We are comparing it now to the ambient seepage. This was the ambient seepage of 12 percent for 120 millimeters per year, which was very, very high percolation flux. It goes up to 22 percent for 320 millimeters per year up to the climate change and then further up later on. We get no seepage obviously until boiling ends, then we get the gradual increase in seepage. This is again considerably less than the ambient seepage, but you still get seepage after the wall of the drift has gone below boiling. We do, however, conservatively reflect this benefit. We don't take it into account at all. After boiling ceases at the drift wall, we assume fully ambient seepage. No further benefit of thermal effects. So we are conservative in the analysis of this for TSPA. However, we think we have the basis to say there's no seepage during the boiling period when the temperature in the rock is above boiling. Next one, please.

This is some of the results of the model. Again, I told you the shedding of the water above here is very important. The dry-out zone is very important. And you see the different colors due to the heterogeneity of the rock. Permeability in this model there is about four inches of magnitude or so, generally to about 100 darcy or about 10 millidarcies.

The other thing I want to emphasize again, the fracture saturation here even though this looks big, the numbers are very, very small. It goes to about six percent or so, the highest saturation, so still most of the fractures contain 94 percent air. Only six percent contain water. Next one.

Again, to emphasize this point, and I'm sorry I'm emphasizing it so much, but all this here talks about leaks above the drifts and lots of water. There's almost no water there. And, we did a little exercise where we just integrate the water in the condensation zone, so we don't take into account the dry-out zone. We just look at the saturations in the fractures and in the matrix where the condensation zone is. Okay? And this is the fractures with time, this is the matrix with time. And, you see these numbers are tiny. The total amount of increased water above the drift there is .25 liters. .25 liters. .25 kilograms is almost none. So there's no water to come back and reflux. It goes and sheds--permeability of the rock.

So this is very important. The total amount of water that imbibes into the rock is only about a liter in they integrated zone over time. Why does it go up here?

Climate change. 600 years. Why does it go up here? Climate change of 2,000 years. Next one.

Geothermal analogs have also given us confidence in this model because the same processes have been used to model geothermal for 20 years or so with the same family of codes. And let me give you an example of geothermal because I think it's important. Some geothermal systems are vapor-dominated systems. That means the pressure profile in them is vapor static. It's not liquid static, it's vapor static. The pressure doesn't change. It's almost like Yucca Mountain, a uniform pressure, often at 35 bars for some reason. Yucca Mountain of course is one bar.

The injection of liquid water into those systems to enhance the systems and increase the lifetime of the system is very important. Water is injected at 20 degrees centigrade; cold, cold, cold water. Temperature in-situ is 240 degrees, similar to what we have here. The waters are producing lots of steam, the steam turns turbine, makes money. Cold water moves in the reservoir, similar to what it does here and tries to go to the production wells. But it can't get there. Why? Because it does the same thing it's going to do with Yucca Mountain. It vaporizes. So you see in the isotope signature of the water that is produced, that 100 percent of the water produced is injected cold water that vaporizes. So even if the driving force it shoots because you're producing steam, you're attracting the water to you which is totally different than Yucca Mountain, the drift does not want the water. The production well and the fuel from the reservoir wants the water because it's producing steam. Pressure gradient storage. But still the vaporization is very important. So this gives us confidence in our vaporization barrier. Next slide.

Now, real quick, the NRC experiment, many people point to the NRC experiment saying, well, it seeped there. Why did it seep there? Is that consistent with DOE models? The bottom line here, it's consistent with DOE models. We did this exercise just recently, at Margaret's request, just to show you this. This is the experiment by NRC, they are concrete slabs of those dimensions, five centimeters by five centimeters, and then they are real long. They are about 60 centimeters. This is the boreholes, you see about three fractures intercept the boreholes. You make the most conservative assumption that you can possibly make, you put water right about here and let the water go straight down. The seepage in here was never visually observed or at least I've seen no literature that says that seepage was actually observed. But there was some precipitates found in there. So we assume that seepage occurred. The experiment was basically

130 days. Lots of water put here. This is the heater output, etcetera, etcetera. Next one.

I don't know the details of this experiment not a heck of a lot. Next one.

The comparison matters though. Here is the NRC, this is Yucca Mountain. We have tuffs, they have concrete slabs which are a little less permeable. The fractures are all continuous there. We don't have that case here. Your diameters are different. Three fractures hit the drift. We have 50 to 100 fractures that actually hit difficult drift at Yucca Mountain. These are the fluxes, about 100 times more than what you have at Yucca Mountain. Heat outputs, temperatures comparative.   Next slide.

So we did this model, basically took one-fourth due to symmetry. That's a detail you don't need to know, I don't know a lot about. Next one, please.

This is what we get. We get heat pipes that were observed. We saw three sensors. You see them here along, above the heater. Boreholes, they show heat pipe conditions. And next one please.

You can see after 140 days there is heat pipe pretty much everywhere. The saturation has increased close to the drift. There is higher saturations close to the drift, and for cases, for some of the cases we got seepage into the drifts when the capillarity of the fractures is low. When the capillarity of the fractures higher, we don't get seepage into the drift. This shows there's water present everywhere. In the TSPA model for seepage, thermal seepage, we would assume full seepage for this case in our TSPA model to be conservative.

So, with respect to that, I conclude that the models that we have is consistent with NRC data. I conclude that the models we have are consistent with all the data we have. And Dan mentioned the large block test yesterday as an evidence for the thermal seepage model not to be valid. I actually think that that data shows that the thermal seepage model is valid because it did exactly what we expected. There was a huge rain storm with a huge amount of water going into a super heated region and what does it do? It does exactly like the other does. It all becomes heat pipe-dominated. So the amount of water that you got there is a large amount of water that all of a sudden went into the test bed, and our model results which are actually in a paper that has been published by Water Resources Research, shows it's very consistent with older models we have there. You're not going to get a rain storm in the middle of Topopah Springs. So I think it's all consistent with the Topopah Springs. Next slide.

So this just summarizes the test, high permeability fractures, all of those. These are conduct details you can just have in your material.

So we conclude the NRC test is consistent in our model. I've told you this before so I'm very briefly going to go over this. We did one more thing to try to validate our model, even and it was a very, very conservative single fracture that goes straight into our drift model. Alternative conceptual model, again, has been published in a refereed journal, by Jens Birkholzer. Next one, please.

Single fracture episodic flow, because the only issue we haven't addressed is episodic flow. We have assumed so far everything is pretty much there to stay except for the NRC experiment and the large block test experiment. They are both transients. Episodic flow, we do this very conservatively. We put the pulse of water all of a sudden on there and we see, can it get through this zone? What will it take for it to get through this zone? If it gets through the zone, how much will the capillary barrier help us? That's the primary issue with this. The basis is an experimental vapor collected by Grace Su. We did exactly this but were on ambient system. She did an experiment in unsaturated fractured rocks which is very, very interesting. By putting a constant flow of water on top of a fracture, because of the heterogeneity of the fracture, you get episodic flow in the fractures. Water moves down and then builds up where the fracture aquifers become small, reaches a critical head and then moves through. It shoots through. Episodic flow in a fracture under unsettling conditions.

What is really interesting about that experiment, too, is what happens when you increase the flux. When you increase that flux you don't change the amount of heat that goes up before it goes through. That's constant. So the volume of water that is needed to break through or given out at your restriction, if you will, is a constant. So the only thing that happened was, the frequency increased, so you just got this break-through quicker, and it scaled perfectly over large range of voltage, so it's very, very important. Jens used that paper and that volume of water, based upon our permeabilities and fracture distribution, he used that as the episodic flow. So when you increase the rate you increase the frequency of these things coming down, prior to breakthrough of this barrier. Next one.

Now this is as conservative as you can get. It must be because you have this one fracture through there. Then he plots up the actual flux in the dry-out zone that is trying to go through the dry-out zone, which is very large flux trying to go through the dry-out zone, and then he calculates the amount of flux that this should be pointing here. That is up to this prong. This is the flux after this prong. When the temperature, with a long, long time after emplacement, these both agree, but you see that for a short time nothing comes through at all. There's no water there, but when it starts to cool down, it becomes the same as ambient. Even in this case here, it probably wouldn't seep because the flow rates are less than the seepage threshold collectors. So again, let me emphasize, we done the barrier, vaporization barrier, if it gets through the crown, it's--

Now, I'm going to spend a little time about temperature and humidity in the drifts. And, this is my last topic. This is Tom Buschek's work at Livermore. The Board questions the DOE view that temperature and relatively humidity is adequately or conservatively modeled. They think there are significant parametric and conceptual uncertainties during the period after the repository is closed, and I think the three primary ones are flow along the drift, thermal conductivities, and there's one more. What was the other one? What was the third one? Yeah, drift degradation.

So I think all of you know the Multiscale Model that Tom has been working for quite many years. The real importance of this model is the following. Most important things are not the thermal conductivity value or any other values. Most important thing is he fully takes into account the mountain scale cooling. The mountain scale cooling is really, really important here because you have three-dimensional cooling effect so these drifts in this area are going to be the hottest. These will be cooler because they are closest to the edges. That's how he breaks down his system into different units, lithophysal and non-lithophysal with the proper properties and then he takes into account the cooling effects.

I'm almost done. Next one, please? Yeah. I'll be on time.

He has validated his model, again using the drift scale test. This is temperatures and this is neutral rocks that show the dry-out. Next one.

He has also validated it against other numerical models that are more simple that use a line source. Next one. So we believe that the model is fully validated. Then he generates temperature at the waste package, at the drift crown, everywhere in the system for all the drifts. Different location, different cooling, different X-effects, and shouldn't be in thermal conductivities and all the parameters is propagated through TSPA and sampled in a proper way. Most important parameters for Tom's model are A, the location because of X-affects, B, thermal conductivities and thermal properties just like we talked about in terms of testing; three, percolation flux or infiltration. He has done many sensitivity studies of those and I'll show you a couple of those.

So this just shows the cooler and hotter packages. Like I said, he has done sensitivity studies of all these parameters. So this is a movie he did, and it has come to a nice movie. If you can start it, it shows you how the things cool down and the hot red is when it's boiling, and then the green indicates when you go below boiling. So you can see how important the X-affects are and you can see the time on top here, 700, that's 750. This is what you have boiling now, and off he goes. After about 1,000 years or so, after about 1100, 1200 years, there's no more boiling of the drift at this location, at this time. 1200 years there's no thermal seepage barrier anywhere in the system.

CHRISTENSEN: Christensen, Board. Does that assume that the thermal output from the canisters is uniform?

BODVARSSON: No, This has different outputs from different packages.

CHRISTENSEN: Okay, so I just was wondering if the variation that we're looking at, sort of the spatial variation in the drift or is it--

BODVARSSON: This spatial variation assumes average outputs from the packages.

CHRISTENSEN: Okay, that's what I wondered.

BODVARSSON: Yeah, yeah. Your question is well taken because, really, this behavior is not going to change even if you have the right assemblages of packages because this is really average behavior. Good question.

This just shows the effect of infiltration. It has low infiltration, mean infiltration, and upper infiltration, and it shows pretty much it doesn't affect vastly the temperatures as expected, but significantly more the relative humidities.

Now a little bit more about thermal conductivities. Thermal conductivity, like I said, is the second most important parameter for this model. The other one being are they close to the edge or close to the center on all of this? We believe that we have very good measurements of thermal conductivity for these models, for the following reasons: These models do not depend on a local thermal conductivity. What really dominates the response, all these models are the large scale heterogeneities, so if you have localized 50 percent lithophysal porosity versus 10 percent, which is 20 percent over the scale of 10 meters, that doesn't really affect things very much because the long-term effect is integrated over cubic meters of volume of rock, large volume of rocks. The project has done extensive testing over the last two to three years, realizing that we can't measure them within conductivities of flux and put in place heaters to measure large scale thermal conductivities because of the lithophysal rocks, lithophysal voids because also you recognize they are very important that during the large scale heaters and measuring over large time frame, you know, the large volume of rocks, we believe our basis for our thermal conductivity values is very solid, especially with the recent data that we obtained.

So that's kind of all I wanted to say about that, so we have recent data on this. Next one.

This shows just your variability with respect to thermal conductivities and you see, you take a mean value, and mean value now is different for all lithophysal because the lithophysal rocks is different from the mineral non-lithophysal, but it's a mean for all of these units. And then we change the mean by one standard deviation up and down. So you see the effect is not very large, really, even though it we changed it quite a lot so I think the thermal conductivity has not been really a big issue here. That's my personal opinion. Next one.

So, finally, sorry how long this took, but Bob kept adding slides and--and I started at with three slides--one said capillary value is good, thermal barrier is good, relative humidity is okay. And they added all these other slides.

But to summarize, what I'm trying to tell you here is that maybe we haven't been as clear in our presentation regarding the capillary barrier and vaporization barrier as we should have been, Hopefully this is a little clearer now. Hopefully, you understand, we have done a lot of work since your letter came out and market has told me personally and all of us to take this very seriously. We want to respond to this. We have done more measurements on thermal parameters, we have done more modeling on NRC studies, more sensitivities of the thermal barrier, trying to solidify our case in all these areas. We have done much more sensitivity studies. Tom Bushek has done a little more on the in-drift environment and humidities and temperatures.

Our results and conclusions continue to be the same. We believe that the capillary barrier is sound, that we have lots of paper to help us validate and verify it. We have sufficient data in terms of heterogeneity to be able to extrapolate it over emplacement drifts. Now performance confirmation of course is going to help us with this, as all of us know. We believe that during boiling, that the--there's no seepage into the emplacement drifts, that only if you have a huge fault, with huge amounts of water going through the fault, could you break through the vaporization barrier. All of our sensitivity studies and tests point in this direction, as does the large block test, as does the NRC test that we have validated and modeled. We conservatively use ambient seepage when we go below boiling around the drifts. We account for any evaporation, which is going to continue, obviously. It's a continuous trend. There's going to be huge evaporations due to the temperature output from the waste packages after the drifts have gone below boiling. We neglected, we are conservative, we are surely to be ambient. We believe that temperature or relative humidity viability is realistically represented in TSPA, with all the uncertainties regarding thermal properties, locations, infiltration rates, etcetera. And we believe that, using modification in thermal properties, we can extrapolate the drifts scale test results through the lower lithophysal walls. And that's it. So, whenever you have a question let me know. Thank you.

DUQUETTE: Thank you. Mark.

LATANISION: Latanision, Board. Could we put up Slides 36 and 46? I'm very interested in the concept that seepage or condensed water above the drift will be mostly diverted sideways, and I'm trying to understand physically why that is the case and what the calculus--I don't know that you can make, that it's appropriate for me to make the comment that what I'm thinking of is something of an episodic event but nevertheless that illustration includes the kind of calculus that I think is important. In other words, you've got a--if you have a fracture in the rock, and obviously you do, above the drift, and you've got water evaporation to the sides, as you show here, what is the calculus that would lead you to conclude that you would not get water emerging into the drift? Is it--I mean am I understanding this properly? It's all going to be diverted sideways rather than--

BOVARDSSON: Yeah.

LATANISION: And the probability of getting water into the drift is therefore close to zero, or is it zero?

BOVARDSSON: Yeah, it's very, very low. I think it's better to explain it with this slide rather than that slide.

LATANISION: Okay.

BOVARDSSON: Because this slide really is a very, very conservative model that doesn't allow you to shed water. Let me try to explain it again here. The key to it all is that the rock mass at Yucca Mountain is heavily broken up by fractures. The average fracture spacing is some 20 to 30 centimeters. They are all over the place. They are all directions. You have the dominant vertical, you also have the dominant horizontal fractures. So the permeability in any direction is pretty large. So water can go just every direction it wants to go. Obviously, water doesn't want to go up because of gravity. It wants to go down because of gravity. But then it can also go sideways if there's a buildup in saturation in the fractures, or there's more water there and then the permeability increases there and allows it to go sideways. So if you take a typical location here, up here the compensate here is due to steam that moved up and condensed there. And it increased the saturation there from three percent to six percent. Very low amount, but it's sufficient in terms of permeability. If you do that, the relative permeability to a liquid phase, because most of it is air, will go up by a lot, by many times. We'll still have small increase in saturation because when you have ceiling water you have no permeability to water because there's no water phase, continues there. When you have three percent water saturation you have really no permeability because the surface area of the flow in a fracture is mostly air, really. So you have minimal permeability, so increasing that a little bit increases the permeability by a lot. So, the water then, somewhere it will start to go down, try to go down, plus the lithophysals. What goes down goes now into a hot region, re-boils and goes back up. But the top phase, sideways, we always let it shed. It's gone. You see what I'm saying?

LATANISION: Yeah.

BOVARDSSON: Did I explain that well?

LATANISION: Yeah. I mean I suppose then that the follow-up--Latanision, Board. The follow-up question would be are you confident enough to suggest that a drip shield is not a necessary part of the structure, of the emplacement?

BOVARDSSON: Well, I don't think I'm the right person to answer that question. The drip shield--I just don't think I--I think that question should be for Joe or somebody that knows more than I do.

LATANISION: All right, then I'll take that challenge and ask it again. I mean I think as a taxpayer there's an issue there. And therefore funds involved in this research. Right. Whatever. I mean the fact remains there's a cost involved in terms of the drip shield and if you're as confident as you seem to be, I think that question is one I'd like to have answered somewhere along the line.

BOVARDSSON: Yeah, and it's a good question. I mean of course the drip shield may be also held with rock for a little bit, and after the thermal period then you have seepage and a drip shield is still intact. That will help you not get any water on the waste packages so after the thousand years or 500 years, depending on the allocation, the drip shield kicks in, you get ambient seepage and it's going to help you.

DUQUETTE: Dan?

BULLEN: Bullen, Board. The first slide I would like to take a look at is actually slide 37. And that's a follow-on to this question about the slide on the left here. You said there's no rain storms in the Topopah Springs, Yucca Mountain horizon. What does it take to get a rain storm necessary to overcome the vaporization barrier? Have you done the calculation? How many liters per whatever? Whatever your infiltration rate might be.

BOVARDSSON: I believe we have. I just don't recall the number. That's a good question. Most of this study, like I explained, Dan, we used the Grace Su experiment for the right aquifer distribution to decide on a value. Then we used that and increased that flux. But I think we did one where we really put lots of water in and it continues to try to get it through, but I don't remember how much it was of water. Sorry about that.

BULLEN: Bullen, Board. I guess just a follow-on, is when you do that calculation or take a look at those numbers, to be able to compare to when the infiltration rate may or may not be there, I mean you may prove that even in a super pluvial condition, you'd never have enough water in-flux to overcome the vaporization barrier. I mean those kinds of things would be important to know if you wanted to make that case. I mean the argument may be, well, maybe the climate changes tomorrow and then would the vaporization barriers still work, and if you have the answer and it's yes, then that would be good to know.

BOVARDSSON: Yeah. I think the answer to that, within the changes in climate that we're assuming here, assuming that it doesn't go 10 times more than what we assume, it's going to hold. The vaporization barrier will hold. Well, the difference is is the following: If there is a huge amount of water, the distance that we need for a dry-out zone may change from 10 centimeters to 30 centimeters if you put a lot of water. See what I'm saying? But we conservatively assume in TSPA that actually the vaporization barrier breaks down at 100 degrees, not 96 degrees. So we were even conservative there. We're closer to what the NRC had yesterday. I think they have 105 degrees.

BULLEN: Bullen, Board. Could we go to slide 4 then? This is just a follow-on question to your comment about the fact that near the drift or near the waste package in a vaporization barrier condition, there would be mostly steam?

BOVARDSSON: Yeah.

BULLEN: Are there data from the drift scale test that tell me what the partial pressure of oxygen was near that to basically I guess prove that point?

BOVARDSSON: I don't recall those data, sir. I remember there were some measurements of it, but I don't recall.

BULLEN: I actually have a--Bullen, Board. I have actually a better memory than you both because I've asked that question of Bill Boyle in 1998, 1999 and 2000, and in all cases it was the partial pressure of oxygen was the same as it is in ambient air. So there never really was super-saturation of steam just because--now it may have been because of a leaky bulkhead, it may have been because of mountain breeze, but I don't think you can make the statement because based on your modeling without the data that's there from the drift scale test that says there's going to be mostly steam. I mean if there's going to be mostly steam, then we would go back to the corrosion allowance outer barrier, which is what we abandoned because we didn't think that it would work. So, that kind of statement is something that's a little bit concerning.

BOVARDSSON: Yeah.

BULLEN: But you don't have to answer anything else. The other question I had was with respect to the--

BOVARDSSON: Just one comment,

BULLEN: Oh, go ahead. That's fine.

BOVARDSSON: I think your point is really well-taken.

And because you are going to have, in the direction of the drift, you're going to have diffusive component, the dispersion that tries to equilibrate, if you will, concentration gradients, so that most of our models are two-dimensional models and don't take that into account so I think your point is well-taken. However--

BULLEN: Sure.

BOVARDSSON: One more. We actually have done models that have taken that into account and we find that in those models that it's mostly steam, too. I don't remember if it's 90 percent or so, but it's still mostly steam. But, your point about the drift scale test I need to look at. You're right. Absolutely.

BULLEN: Bullen, Board. The last drift scale question, and that is there was a brown spot that was noticed on one of the surrogate waste packages. Was that ever explained as rock bolt dripping, or what was the explanation for the discoloration on the waste package that was noted, and has that been explained?

BOVARDSSON: I think it has, but who can explain it better than I can?

BULLEN: We have any members? Mark, do you remember an explanation for that? Or Bill? Well, come up to the microphone, please. Yeah.

PETERS: Mark Peters, Special Laboratory. The question was on the Canister 304, there's a red spot back there?

BULLEN: Correct.

PETERS: It was sampled remotely, off the robot, swiped with a swab, brought out, and it was XRD and it was iron oxide. Hematite. It's currently thought, one likely thinks it could be that it was flaking off of some of the ground support in the crown, but we really have to go in and look after the test is over to get a much better idea of what we're looking at. It's not clear to us that it's evidence of any kind of discreet flow in the drift.

BULLEN: You said flaking? I did get the word? Flaking?

PETERS: Yeah. Well, I used that word. That's my word, but it is iron oxide that gathered on Canister 3 or 4.

BULLEN: Thank you.

CHRISTENSEN: Christensen, Board. Bo, one of your points I think directly related to Mark's final comment yesterday and relevant to the discussion by the Nevada contingent yesterday, and that is the effect of refluxing on potential concentration of salts. And, so I want to ask really two questions. Do we have empirical data on water chemistry that, I imagine we'll see later today.

BOVARDSSON: Yeah. Yeah.

CHRISTENSEN: That's really important. But the second--and it may make the second question somewhat irrelevant, but it seems to me that the difference in view of your--has to do with the, with the physical, what has happened physically with the water as it's evaporated out, and it seems to me that the Nevada--I'm not going to put words in their mouth, but maybe looking at the convective circulation of water at the boiling front, so they are looking back into the rock, the--and I'm trying to understand why these two different views are so different. What the potential physical explanation might be.

BOVARDSSON: Yes, sir. Good question. Let me try to explain it. Let me explain it this way: What happens--what we believe is happening at Yucca Mountain and then I'll talk about what I think they believe is happening. What we believe is happening is that we start by putting a waste package in place here, we start heating up this area. You have a boiling zone that moves with time outward. It starts next to here. That's where the matrix starts to boil, steam moves out, and in 100 years or so it's out a few meters, and then a few meters more. Maximum distance is about five to six meters. The key here is that there's all these different waters being boiled. It's not the same water being re-boiled, getting more minerals from the rock, again accumulating minerals because imagine this matrix blocks here after 10 years it starts to boil and then it boils all of this water off, leaving a tiny bit of salts there because the water is really benign. It's only 1,000 milligrams per liter total to salt solid, which is very low. There is almost no salts at the start. Then it moves to the next one and it starts to boil that water off. So there's no accumulation of salts.

Now, if steam moves out here and it condenses here, and it condenses here and starts to pick up some minerals, but it doesn't have a lot of time to pick up minerals because it will move down, okay? Steam here will have a little more time because it's directly above it. It has no more residence time. It might take up a little bit of minerals and then it's going to shift this way. And you get new steam, new condensation and again almost no mineral in the water, so we are getting rid of minerals in opposition to I think their conception of their model where we are accumulating minerals.

Now, their conceptual model says let me just consider this front stationary, and all this barrier here and all this has a heat pipe. It's not dry, it's a heat pipe now, which is not the case in our models, or in our drift scale observations, and we always condense here and pick up minerals and that water can't go anywhere because it's a closed system. It picks up minerals, then moves and brings the minerals, then boils again and it starts to concentrate fix, if you will. I think that's the difference. So we believe you are getting rid of chemicals, you are having cleaner water because you don't have time to pick up so many minerals because you're always replenishing it with more and more steam, whereas their conceptual model is a closed system where you can't get rid of anything. That's how I read it.

CHRISTENSEN: That's very helpful. Thank you.

CERLING: Cerling, Board. We've spent a lot of time on these pictures and I'm going to leave them here, too. And maybe this is better answered by Carl, but I'll ask it now because it has to do with time. And so in your model you would have the water getting cleaner for a period of time. Before boiling we'd be getting seepage water that has, let's call it ambient chemistry. So how long after the vaporization barrier disappears would it take to return to this ambient chemistry in your model?

BOVARDSSON: I think you're absolutely right. I think it's better answered by Carl. Carl is going to review the chemistry. He's going to handle the drip shield and I think it's better answered by him because you're right. Even though the seepage, we assume there's no seepage during the thermal period, even after the drift wall temperature goes below boiling, it starts to get seepage.

There are thermal hydrological processes in the rock that take place and manipulate the water so it's not the same as the ambient water. So your question is--so I think Carl can address that. Or we can address it later on.

STEEFEL: We have slides on that.

PARIZEK: Parizek, Board. Just a couple questions. One, I was having problems with the steam always being in the repository. If you're above boiling and dry out all the rocks around the opening, eventually you ought to get rid of your steam, shouldn't you? Shouldn't it be steamless after a while. Maybe on the ends?

BOVARDSSON: Yeah.

PARIZEK: You said there was always going to be steam in there.

BOVARDSSON: No, no, sorry. I apologize for that. What I think is going to happen there, and our model itself has shown this, is that we start to boil in the rock and when you boil in the rock, you start to generate steam. When you boil in the rock your pressures, your gas pressures increase in the matrix below there because it's tighter to get the steam out and therefore the gas pressures also increase slightly in the fractures, 10 to 100 Pascals or so. So steam goes both directions. It goes out in the rock, it goes back in the drift. Now, the steam generated in the drift can flow laterally to the cooler part of the drifts and condensate there and cool down, away from the packages. Then later on when boiling stops, obviously, the mass collection of air in the drift is going to return back to all air. So it would be mostly steam, I believe, even though I think Dan's comment is a good one, all steam, but there's going to be counterflow of air, of steam in the drifts because of diffusion and dispersion processes that's going to equilibrate concentrations, but it obviously will go all back to air. Does that answer your question?

PARIZEK: Right, but--Parizek, Board again. As the rock is drying out and you could have the steam go both ways, is there some distance into the rock after which the steam only wants to go away?

BOVARDSSON: No. No. There's always going to be--

PARIZEK: Always some return?

BOVARDSSON: You pick a block here that maybe increases in pressure in the matrix block by a few hundred Pascals, and then that boils away and then this block increases and so you're always going to have some gradient this way and some gradient this way. But the gradients are going to decrease with time because you arrive, because the pressure increase here is going to be similar to that here, but the distance is longer so you're going to get less steam flowing into the drift at times just because of distance. You understand what I'm saying? It is always going to be both ways. That's my conceptual model.

PARIZEK: The Board also has a concern of the axial, flow along the axis of the drifts.

BOVARDSSON: Yeah. Yeah.

PARIZEK: Because of unevenness of the heating and then the cold trap effect so, and these are some of where our points come from. On page 19 you show the seepage threshold values and the various variables that went in there. I guess in there is also lithophysal facts.

BOVARDSSON: Yes.

PARIZEK: And so that's caught up in the TSPA.

BOVARDSSON: Yes.

PARIZEK: Now, was it always in TSPA quite in that form or is this new? Are these new data?

BOVARDSSON: Well, it generally has been like this for years, that we actually had the sensitivity to those repositories, but they are dependent upon which unit they are in, what the permeability of what units they are in and all of that, so we still sample this. We also have new curves for drift degradation that took into account that the drifts were collapsed, fully collapsed and then we modified these.

PARIZEK: So that would be the new part?

BOVARDSSON: Yeah, that's totally a new part. But we always continue to modify as more data come in and our certainty reduces then we change this to reflect the new data, but the changes are subtle. They are not harsh changes.

PARIZEK: And then on Figure 21 you show films which the Board is also interested in, but on rough surfaces are there times when film flow can give rise to drips, just because of rough surfaces?

BOVARDSSON: Yeah.

PARIZEK: 21 shows, you know, in a matter of speaking.

BOVARDSSON: No, you're absolutely right, and we see that in our seepage experiments because you might, it starts out with vets in the ceiling, and then it starts to go sideways and then you form drips that collect in the seepage collection rate, so that's taken into account. Yes. Absolutely.

PARIZEK: Thank you. That could occur in any location?

BOVARDSSON: Right. Any location, yes.

PARIZEK: In the drift walls ceiling.

BOVARDSSON: Yes, so that's why our collection system for the seepage extends over the entire thing. It's not just done to where we expect the seepage to be.

PARIZEK: And you mentioned that there was no seepage observed to date in lower lith. Yesterday we saw droplets of water that Mark Peters showed us, but I guess he would attribute that to condensation and you would attribute that to condensation. That's your consistency then. That's not seepage, in your opinion.

BOVARDSSON: Yes. I think the--no, we have some geochemical information from Zell Peterman and others that indicate that the chemistry is very little in this water. It's almost no silicon, no chloride, if I remember correctly, in some of these samples. So it's not consistent to seepage water. So it's very likely to be condensation.

PARIZEK: Figure 54 was your movie, which was very nicely done.

BOVARDSSON: John Buschek's movie.

PARIZEK: But the movie didn't show anything about permeability, variability in any of the repository footprint locations, like fall effects or something. Only edge effects as a result of cooling. Does that mean there are no fast pathways anywhere in that footprint?

BOVARDSSON: No. No, there are fast pathways. The thing is, though, the thermal signature is dominated by conduction. Conduction is a diffusive process which is a large process and that's why low conduction is a thermal conductivity done not a heck of a lot because the large conductivity matters. Faults do not affect the large scale thermal properties. They are going to be the same along the faults, unless the material is different or the radishio (phonetic) or something in the faults. So the movie is not going to depend a lot on flow and faults or things of that sort because it's so dominated by heat conduction.

PARIZEK: That would mean that the Chlorine 36 issue and so on, the amount of water is trivial, I guess. You're saying it's conduction-dominated. I mean you really have damn little water flowing through that system in the time frame of your model.

BOVARDSSON: Yeah, well, I have all this stuff. The Chlorine 36 is not a big issue because such a small amount of water is actually the fast flow of Chlorine 36. So I'll always say it's not a real important issue for performance. Might be a real important issue to make sure we understand from a physics point of view, but I don't think it affects performance in a big way.

PARIZEK: And Figure 37 deal with the analog value of geothermal reservoirs. And, in that sense I guess the program has the geothermal reservoir analog somewhere in its story. Is that right?

BOVARDSSON: Yeah. Trafalgar (phonetic) has done work in Maraki in New Zealand with materials, it has done work in Yellowstone on geothermal systems, so there has been work on geothermal system and analogs to Yucca Mountain.

PARIZEK: Tied specifically to the water flow issue as you were--

BOVARDSSON: It has not been tied directly to the thermal seepage issue like I did. That's not--

PARIZEK: Okay, that's new.

BOVARDSSON: That's new.

PARIZEK: Because we would like to see the analogs used in the most direct way possible rather than--it's a list, but the listing is useful and if you can then say well, what about the analogs used for some specific process you're dealing with. I think you've just stated it today perhaps for the first time. Anyway, it's different than what's in the analog discussions.

BOVARDSSON: Yeah. That's a good point.

PARIZEK: And then one last point. As you boil the water off, my tea kettle always has something left behind and I don't know what's in there, but if you reflux water through it, I imagine the water is going to be so clean. And you stated before the water is going to be refluxed out, moved out the side, the lateral movement of it, and always the salts are going to be very dilute and very small quantities of it, so it can't change the chemistry much. But I guess we're going to hear this when we get into the drift scale tests as to what water chemistries have been collected in order to support that statement?

BOVARDSSON: Yeah, but let me clarify that a little bit. What I was explaining is the conceptual model for boiling and shedding. So we get rid of some of that water and some of these minerals with time. Certainly, in the matrix block there's going to be some salt left because you boil all the water off. But that amount of salt is very small because the water is so dilute. It's only 1,000 milligrams per liter or something like that. So it's a very small amount of salts, but when you start to reflux back and the mix-model, the TOT models show this, you pick up that water, you pick up that salt and you have high concentration in the beginning because it's coming to a little bit of salt, so I think Carl will talk about that a little bit.

PARIZEK: And that's kind of an important point because refluxing source of salts is one way which you can visualize it being high concentration where it didn't go laterally, but finally cascades back through these small little packages that have small amounts of salt accumulatively, not a lot of water, but quite a bit of salt before you're done.

BOVARDSSON: Right. Yeah.

STEEFEL: Steefel, LPL. Just to make clear, though, that use of the term refluxing by the State of Nevada seems to be somewhat different where they basically cycle reconcentrated water continually back so that it develops somehow a large pool of concentrated water. What Bo was explaining is it's basically a one-way transport so the salts that are left behind are basically whatever you could get out of that ambient pore water, precipitate those out varying rewetting which is the preferred phraseology I think to refluxing. You do pick up those salts and that's what Bo mentioned. That shows up in all the models. I'll show some slides. But it's not this heat pipe refluxing thing that the State of Nevada is discussing.

PARIZEK: Parizek, Board. The figure 46 though on the end might be your worst case. I mean one long fracture and all of the steam, a lot of steam trying to go up the fractures, maybe that vaporization zone is much longer up a fracture like that? So again you can sort of see more salt accumulation along that fracture if that holds?

BOVARDSSON: Could be. Any other questions?

DUQUETTE: Thank you, Bo. Don Shettel, do you want to comment on the interpretation of the reflux data by Livermore.

SHETTEL: Yes, I'd love to comment on this. I think there has been some mischaracterization of the refluxing zone. We believe that the refluxing zone is like a heat pipe above the repository. And Bo's explanation is not clear. He says that condensate comes back down and is dilute. Well, yes, if it condenses up in the cold rack and comes down it doesn't have much time to pick up any dissolved solids. And it's dilute. But if it's condensate coming back down, to me that seems to imply that he is allowing some refluxing. And we don't--our system is not a closed system as he has characterized it as. We believe over time as one of my slides showed, you get some condensity lost over the sides of the drifts, which is consistent with some of Bo's modeling. And, by the gradual net loss of condensate over the side, you do allow some concentration of solutions in the boiling zone. We're not saying you're getting concentrated solutions coming back down, but that the concentrated solutions develop at the boiling front. So over time, it starts out initially dilute, as he says, but we don't believe it stays that way. Over time loss of water and compensate over the side with the net percolation and refluxing that the concentration builds up, and it's not a closed system, some compensate is allowed to be lost over the side so therefore it is an open system.

DUQUETTE: Thank you very much.

BOVARDSSON: Just to clarify that a little bit, too, I think what this gentleman says would be true if this was an entire heat pipe region where actually water would go back and evaporate in and go back up. It's not. Our drift scale test results have shown this is super-heated region with no heat pipe present here, and there is therefore no heat pipe in here. There's only a heat pipe process boiling point up here. So there's always different regions of boil and therefore you cannot concentrate waters. That's our conceptual model.

DUQUETTE: Thank you.

BOVARDSSON: According to our drift scale test.

DUQUETTE: Thank you for your presentation and the discussion. Some questions?

DIODATO: Bo, thanks for your presentation. Diodato, staff. Just one quick--there are number of issues we would love to have the opportunity to discuss with you, but let's just use one, stay with the thermal hydrology question. Mark Peters yesterday shows a slide 10. John, I don't know if you can get that up, on the one side. And, compare that with your slide 36 from your presentation on the other slide. Now the slide 10 is matrix saturation. That's actual data, and so that's from the drift scale test, right? And there's, the 100 degree isotherm there and the 120 degree isotherm, there's still like 80 to 90 percent water saturation in the matrix. So first, I'm curious about how this water continues to persist at above 100 degrees and maybe that's a concentration effect being the total ionic strength is larger there, but then, why wouldn't there be a gradient between that number and then the fracture saturation on the left where you could have some matrix fracture interaction of flow? And, maybe mechanical transport. Is that possible? I mean is that, I'm just curious, I mean I look at this data, right.

BOVARDSSON: I think time is out, David.

DUQUETTE: Saved by the bell.

BOVARDSSON: Let me explain that I didn't look carefully at Mark's slides yesterday, but let me explain something here. We vary our boiling in the matrix blocks, the pressure goes above the ambient 1-bar pressures because it starts to boil and you increase volume because the volume of steam is a lot more amounts of water and then you increase pressure to get the steam out because you're continuously going, okay?

DIODATO: So this is a lower permeability, you have almost a closed system.

BOVARDSSON: Because of lower permeability. So the temperature in the fractures would be 96 degrees or close to it, but the load is higher because the pressure there would increase by 10 or 100 Pascals or something like that. So there is going to be a region here where perhaps because the pressure is a little higher, the temperature is a little higher so that you don't have a sharp interface there because you haven't boiled up all the water in those matrix blocks.

DIODATO: So there would be both the pressure and the thermal gradient between the matrix and fractures?

BOVARDSSON: That's exactly right.

DIODATO: So that would tend to transport water.

BOVARDSSON: That will transfer steam, get rid of the steam from the matrix blocks.

DIODATO: All right.

BOVARDSSON: So I think that's reflected there to some extent. That's my interpretation, although this is Mark's slide.

BOVARDSSON: That's smaller. Okay.

DIODATO: Which is model? The water saturations or the temperatures?

BOVARDSSON: Both models.

PETERS: Both models.

DIODATO: There's no drift scale data.

Next slide. Yeah, so that's data. That's less clear.

BOVARDSSON: No, I think this point is very clear.

DUQUETTE: I think we're going to stop the discussion here. We're, as usual, running a little bit late. Let's hold the break to about 10 minutes and we'll steal some time from the lunch break.

(Whereupon, a brief recess was taken.)

DUQUETTE: The next presentation this morning if people will please take their seats. Thank you. The next presentation this morning is on the in-drift chemical environment during the above boiling period presented by Carl Steefel.

STEEFEL: Okay, thank you. We're going to split here. I'm going to be presenting two talks and basically splitting what's broadly referred to as the thermal pulse into two periods. The first one, the above-boiling period and the second one will be the below-boiling period. By boiling I mean when the drift wall reaches the boiling temperature of 96 degrees. I think it divides the talks up nicely into a distinct set of key issues that makes it easier to grasp.

And so I'll make the acknowledgement. I've had a lot of help on this. I'm just mostly the spokesman here but in terms of the in-drift chemical environment, Greg Gdowski, Susan Carroll and Tom Wolery, significant contributions from Zell and Tom Wolery. And then, the THC and the issue of acid gas volatilization from the group at Lawrence Berkeley, Nick Spycher, Eric Sonnenthal and Guoxiang Zhang.

So as an overview of the presentation, I want to begin with just a short statement on the effect of the thermal hydrologic environment on the in-drift chemistry during the above-boiling period. Why is it that we've got special issues because of the thermal hydrologic regime, and then I'll launch into a first look at salt deliquescence, go on to what salt minerals in dust are found in the repository and ask the question specifically can they generate calcium chloride or magnesium chloride brines. Then look at the issue of stability of these salt minerals or salt mineral assemblages at temperature and different RH values.

And then, there's a natural lead-in from that topic into the topic of acid gas volatilization from salt assemblages at temperature and specifically what's going to be their fate in the drift environment. This is focused on the drift environment. And then return for another look at salt deliquescence with a focus on the multi-component aspects, asking the question how much water is absorbed and what composition brines can actually develop, and then I'll conclude.

So first just briefly recap the THP environment and the effect on the in-drift period, the in-drift chemistry during above-boiling.

The issue here is that focus is where the drift wall temperatures in this rock are above boiling, which includes a relatively short heating period that dries out the drift and rock and then a much more extended period in which the drift wall gradually cools to the boiling point of water in which, as Bo has made a strong case, there's no seepage of water possible. Given this, the key issue during this period are the deliquescence of salt originating from dust accumulated on the waste package and the related issue, possible generation of acid gases from the deliquescent salts and their fate in the drift environment. And, you can see here, the key to this first point about no seepage is taking credit for the vaporization barrier on top of the capillary barrier. It's not to say that no seepage will ever occur, but it's an extremely low probability, extremely rare event and so we've got a whole different set of issues, basically dust that we can focus on during this period.

And so, this is the period we're talking about. This is a slide that's going to be shown later by Joe Payer with the ventilation period number one and then a heating period after ventilation. We're basically concerned with the whole heating and cooling period until the waste package temperature--I mean rather the drift wall temperature drops down to 96 degrees, so that's right at this point. So we're concerned with something on the order of this kind of time frame.

This is an average waste packet. You can see the characteristic sort of patterns, though. The drift wall obviously is a little bit cooler than the waste package, and this important gap in temperature creates a difference in RH, of course. And the RH is shown on the waste package inverse as people have pointed out relative to the temperature with lows, the low here is about 30 percent. Some waste packages or drifts will actually drop to below 20 percent. And then basically comes up to about 65 percent RH at the end of the boiling period.

So we're going to basically be looking, if we could see the next slide, at a somewhat limited range of temperature and RH distributions here. In fact, when you take time out of it and consider, as Bo showed in that movie, that various packages move through this whole thermal history at different times, if you take time out of it and look at just RH and temperature, a lot of this variability collapses into a much more limited range of conditions. So for example the average waste package would reach the boiling point at the drift wall at this time, so we're basically in this talk concerned with a typical temperature RH trajectory up to about 65 percent, perhaps a little above that. So we're going to focus on that period.

So I want to start with this salt deliquescence.

And the issue here is that dusts originating in the repository, this is a review of course, are brought in by ventilation, may contain salts that deliquesce at relative humidity, less than 100 percent. Well, what is salt deliquescence? It's, first of all, the activity of water times 100 equates to the RH. That's the activity of water in the atmosphere. So the idea is that the activity of water and equilibrium with the salient brine is lower than one in every case. Pure water has an activity of 1. So these brines will absorb water when the RH, that is, the activity of water in the atmosphere is greater than the activity of water in the brine. And for highly deliquescent salts this also implies that a brine will always exist above the boiling point of pure H2-O. This is the so-called boiling point elevation. Next slide.

And this is just showing--I'll show this quickly here, showing here that body and vapor pressure water versus temperature for different activities of water, for example, pure water activity of one, you can see that the repository pressure boils at 96 degrees centigrade, 100 degrees obviously at sea level. You can see, though, if you reduce the activity of water, for example, the .75 then this would be a water equilibrium with a saturated sodium chloride solution, we raise the boiling point up to about 108 degrees at sea level, about 106, as I recall, at the actual repository pressure.

And so the deliquescence, you've seen some of these slides, RH for some of these salts. Shown here, for example, calcium chloride pieced together is the sum of a variety of phases of different states of hydration, but they cluster quite a bit here. This is the so-called deliquescent RH curve, below it RH values in the drift environment, below that, that salt, if it was present, is going to be dry. Any time that RH comes above that deliquescence RH curve, you're going to start to absorb water and you're going to wet up this system. You can see it's about 20 percent for calcium chloride. For these other salts halite shows very little temperature dependence right there at 75 to 74 percent. Soda niter, much stronger temperature dependence. You can see here also that the combination, the assemblage halite plus soda niter in every case will have a lower deliquescence RH that's basically the combination of those two salts there.

And this is a slide that kind of summarizes some of the chemical behavior. I'll come back to this towards the end of this talk and again in the next talk. These so-called eutectic slides used to give me headaches and igneous petrology, but I think they are worth going through briefly. The point is that if you'll look at RH versus, say nitrate chloride ratio for the system, sodium chloride, sodium nitrate, as the RH rises initially you've just got these two salts are dry. Once at a particular temperature it hits the eutectic point at 120 degrees--that would be about 55 percent--you form or start to form a solution. It absorbs water there.

However, if you had a bulk salt that was--salt that had a bulk composition of say, 0.5 just for argument's sake here, you don't actually initially form that composition. You form the so-called eutectic composition and gradually then as you raise the RH you migrate along this solvus. It's only once you get back to this bulk ratio that you can actually leave that solvus, then you've basically dissolved all your salt and you get back your bulk salt ratio. The reason I'm going on about this is really to make the point that the thermodynamics of the system is actually, in some cases, adjusting very significantly the ratio of nitrate to chloride you have in the system. So I'll come back to that point.

So on the salt minerals in the dust actually found in the repository. The point is that, due to lack of seepage during the above-boiling period, the salt-bearing dusts are the chief concern because of their potential for deliquescence. Small amounts of soluble salts are found in the repository dust. Higher amounts are expected if atmospheric dust became a more important component of those dusts, but I'll review the actual concentrations and fractions of the different cations and anions there. Chief concern of course is the chloride-bearing salt because of their potential for corrosion of the Alloy 22.

And so what has been done by the USGS as part of the project is to collect dust from the various tunnels. This is part of the dust collection in phase 2. They basically took surfaces and vacuumed them. There's one of the vacuumed surfaces. Here you can see the original dust that was developed on the rock and placard, so you're actually able to estimate amounts of dust that are coating these various surfaces. I'll make use of this a little bit later on.

And the next thing then is the actually analysis. These were analyzed, it would be nice to just go in and x-ray these and know what salt minerals were there. The problem is the salt contents are extremely low and they are below the detection limit of normal x-ray to fraction methods. So what was done is basically to determine the soluble content of the dust by subjecting the samples to a distilled water leach. And then basically--well, first of all, obviously you get out the proportions of nitrate and chloride. Chloride in that system and the various cations--I'll show you that in a second--but in addition, in terms of identifying what primary minerals were actually originally there, we follow two paths here. One is to then evaporate these leachates to yield salt minerals that are going to be present in a small enough, in large enough concentration. Now we don't have the dilution with all that inert dust, and these can actually be identified by x-ray diffraction. And a second way is to determine the primary salt minerals via calculation, using an equilibrium mass balance evaporation calculation, EQ3/6 that basically takes the water out, reverses the leaching experiment, pulls the water out to come back at it. Implicit in this is some equilibrium assumptions I'll discuss briefly later. But a key point is that it does observe a rigorous mass balance in terms of the overall system of chloride nitrate, whatever is in there. It observes that mass balance. You can kind of think of that as something like a normative salt composition.

And so, here is the average of the soluble cations and anions, and I probably should have put this on the second slide, but that's all right, just to point out a couple things, there's a fair amount of calcium, but I want you to note that there's also quite a bit--this is plotted in terms of milli-equivalents per kilogram dust. That's like mols multiplied by the charge. There's quite a bit of sulphate and carbonate. Some of those actually exceeds the calcium.

I want to also point out there's some chloride, as you would expect, but this chloride in every case is actually less than the sum total of potassium and sodium and cations, and there's also nitrate in the system. So try and keep this plot in mind and we'll come back to it as we need to.

And so here's the results of the actual x-ray diffraction analysis of these salts precipitated from the evaporated dust leachates. And it shows as the dominant salt, as you would expect, is sodium chloride occurring in all the samples. Major abundance in samples two through five. Also getting calcite in one of the samples. And also significant amounts of sulphate. This is gypsum, this is basonite, another hydrated version of calcium sulphate mineral. For some reason due to the analysis we're not able to pick up the nitrate minerals and we're actually picking up ammonia minerals whether that's anthropogenic effect from the drilling or whatever needs to be resolved. But there is some of those there, but the dominant mineralogy is this sodium chloride salts and the sulphates.

This is also corroborated in general by the calculations which predict a series of main key assemblages, salt assemblages. 68 percent is actually typified by this assemblage of sodium chloride and potassium nitrate. The bromide is basically drilling activities and testing, so in a normal repository setting, you wouldn't have significant concentrations of bromide. You can see this is a similar assemblage. We've only added the soda niter here. So no calcium chloride or magnesium chloride salts are predicted. None were observed in the actual evaporation of the leachate that I showed in the preceding slide.

And so here is a comparison of the different soluble salt contents in the ESF here, reported by Peterman, you can see we've got about .3 to .4 percent soluble salts. So it's quite low. You can see now why we don't x-ray these things. X-ray in general has a hard time below 3 to 5 percent. In contrast, atmospheric dust has a higher soluble salt concentration. So if these became more important obviously you could bring this number up.

And so the question arises then, can the salt and the dust generate these calcium chloride brines? Well, we have to say a calcium chloride brine is possible only if the following conditions and the soluble salt faction are both satisfied. Number one, calcium has to be greater than sulphate plus carbonate. The reason is that calcium sulphate, gypsum or anhydride and calcium carbonate, that's calcite, are relatively insoluble. So you'll tend to pull your calcium out of solution to the extent that those anions are present.

Secondly, chloride has to be graded in sodium plus potassium, same sort of argument. Sodium chloride and potassium chloride are less soluble than calcium chloride.

They'll precipitate earlier. 96 percent of the tunnel dust failed to satisfy this first condition. The remainder, about 4 percent, satisfy the first but not the second condition. But we don't see from the data we have that there's any way in which calcium chloride could ever be a significant part of these dusts here.

Similar arguments for the magnesium chloride, except now there's three conditions. Magnesium graded and carbonates, same sort of argument. This is an insoluble phase, drops the magnesium out. Same argument about a chloride. And then additionally a low silica environment since magnesium is likely to be tied up in silica. There was some discussion about sepiolite yesterday which is observed for example in experiments run as part of the opalinus clay study in Switzerland. Sepiolite was actually identified there. Turns out what mostly is observed in the evaporation experiments run at Livermore, is a magnesium sheet silicate, either smectite or some other sheet silicate. So there's nothing absolutely required or special about the presence of sepiolite. The point is that magnesium silicates tend to form. And this is of course observed in other environments. If you look at Mid-Ocean Ridge circulation of sea water which is loaded with magnesium, it's well known that that water when it circulates through the higher temperature rock is basically stripped quantitatively of magnesium. So I think pretty much all geochemists accept that this reactivity of magnesium with silica is a well known phenomena. But the point here is that none of these conditions occur within the repository.

Another point to be made is if the presence of calcium chloride or magnesium chloride minerals in outside dust is very unlikely. The minerals are known to exist on the earth's surface at only a few places. For example, in Antarctica. This is the type locality for the mineral antarctikite. The few known surface occurrences appear to be a ephemeral. The key point here is basically that very low RH conditions, less than about 25 percent, are required to preserve these salts in-situ. And these low RH conditions obviously are even harder to maintain during transport of salts. So in other words, even if you could make and maintain under extremely low humidity conditions a calcium chloride salt, you've also got to transport it to Yucca Mountain without it ever seeing above about 20 percent. Otherwise, it deliquesces and it solubulizes.

And let's jump forward. I did put one out of order here. Sorry about that. After all this I put one out of order. I should be up to 26, not 36.

And so this is just showing comparison of the soluble ionic ratios. This is actually--might just skip this because Don Langmuir showed various similar data basically showing the ionic ratios of tunnel dust and Nevada rainfall, which as he showed is quite similar to the atmospheric dust. But you can see in general that even if the atmospheric component goes up, still we've got very similar sorts of mol or ratios developed here in the tunnel dust forward to about 1. Here in the Nevada rainfall we get very similar sorts of ratios of nitrate and chloride. So we don't expect a completely different story if, during ventilation, the atmospheric component became more important.

And, well, the previous one was really just the intro. It's not that important, just says we're going to talk about stability of these minerals. The point here is, as I've said, although it's not expected based on the preceding arguments, the stability of calcium chloride and magnesium chloride have been evaluated experimentally. And a good portion of the calcium chloride data is new to the Board, I believe, and all of the magnesium chloride data is new. The stability of sodium chloride which is expected to be present in small amounts, as I've said, in the dust, has now also been investigated experimentally. All new data here I'm going to show. And these have been evaluated using TGA analysis under controlled temperature and RH conditions.

Here's just a quick picture of the apparatus. Basically shows the sample chamber. These were actually originally designed as corrosion experiments with the Alloy 22 in there, obviously. I'm not going to talk about that. I'll leave that to Joe Payer if he needs to. My issue concern is just the stability of these salts, but you put it in there and then you control quite closely both the temperature and the RH in that chamber so you can raise the RH and know where you're at. It's sensitive to weight changes as small as tens of micrograms.

And here is the results, as I say, some of which you've seen at least this calcium chloride at 150. Most of the rest of it I don't think you've seen. This shows both the calcium chloride and the magnesium chloride system. At 100 degrees calcium chloride is aqueous films, are stable for the duration of the test. And just to remind you what you're seeing here is basically a very rapid initial weight gain as the salt deliquesces at 22 percent RH in that temperature, and then if it's stable, obviously it's going to just stay at that with the salt, with the water maintained there. That's what's happening at 100 degrees.

And I believe Doctor Duquette was saying at 105 you could make a stable solution. This would seem to be the case. At 100 there's no problem with stability. Soon as you start going to 125 though we start to see that characteristic, weight loss, forming insoluble precipitates.

At 150 degrees down here we see the solution evolves rapidly over as little as two days forming insoluble precipitates and also generating an acid gas, HCL. It's most likely that the same thing is happening as mixed system. This is new data. Calcium chloride plus calcium nitrate with volatilization of the hydrochloric-- it's a lower rate probably just because of the dilution effect of the nitrate, calcium nitrate that's present there. You can see also that magnesium chloride basically transforms within hours at all temperatures considered in each case becoming non-deliquescent at these RH conditions. You can see there's the magnesium chloride number at 125 and the number here, the green, at 115. So there's no stability to these magnesium chloride salts.

When you look at sodium chloride, this is all new data again. Sodium chloride does appear to be stable at 105 degrees. For reference boiling point of sodium chloride saturated solution is 108.7, so we're a couple degrees under that. Seems to be some evidence for transformation of this assemblage. It's not clear at this point what that's due to. Most likely it probably is volatilization of a little bit of HCL, which is now occurring because we're able to bring this solution up to

120 instead of 105 because of the lower deliquescence point of that mixture, as I discussed earlier.

And so in terms of the characterization of these deposits, formations, electron dispersive spectroscopy indicates precipitates contains calcium chloride and oxygen. The analyses also indicate a loss of chloride, particularly in the case of the 150 degrees centigrade one. A chloride relative to calcium indicating volatilization of the hydrochloric acid as a gas. This is conducted under open systems where you continuously flush through a gas of known RH, but also with basically zero HCL in the atmosphere. Spectroscopy indicates a precipitates or not, calcium hydroxide or calcite, so it looks like it's most likely a calcium hydroxyl chloride that's been formed as a combination of the loss of the chloride, but probably more importantly the loss of this acid component which allows the PH of that solution to go up a little bit and precipitate. The point is that 22 percent RH again, this is a non-deliquescent salts. So we're not able to maintain these highly deliquescent salts at temperatures, in films under open system conditions.

And so the summary, I probably just gave the summary. We're able to to these experiments and show the calcium chloride transforms the non-deliquescent phases. Deliquescent magnesium chloride is unstable at all temperatures investigated above 100. These are the results of our HCL volatization for which there's a strong temperature dependence observed. Calcium chloride doesn't seem to volatize at 100 whereas it goes very quickly at 150. And here's a point where I was going to make here is that perhaps this is what the Board was referring to when they talked about conflicting evidence about calcium chloride as corrosion potential. I think the point here is that where nobody is questioning the potential of calcium chloride to corrode the various alloys at temperature. In other words, those are good experiments. The issue is whether those salts are stable as thin films under various RH conditions under open system. If there was purely a corrosion issue, I wouldn't even be up here. Joe Payer would be discussing it. And so it seems to be open system behavior. In other words, if you construct closed system version of this where the HCL in the atmosphere builds up and then finally came to equilibrium with HCL in the actual brine, you've shut that devolatization and transformation process off completely. So it occurs under open system conditions where you can flush that gas out of there.

It's our contention as I'll try and show in the next slides, that the drift environment is very much an open system and therefore that PGA, while the actual transport rates may not be dead on, is much closer to the real situation than a closed system. That's the key, though, I think, is that we're not saying calcium chloride doesn't corrode. It's simply that that assemblage is not stable at temperature as a thin water film. Obviously, if you made a large solution of it, you could maintain that for quite a while, and that's, of course, the way the corrosion experiments are done.

And so that basically leads naturally into the next topic, the volatilization of acid gases in the drift environment.

And basically, we've said that the instability of these salts to the extent that they are present, we don't think that they will even be present, but it would be due largely to degassing of HCL. HNLC gas is much less volatile than HCL gas so the primary issues with HCL gas, so the question is what will happen to any acid gas generated in the drift environment and how much acid gas will be generated. May be just as the back of the envelope because as I said, we don't expect under normal conditions even to find any salts there. But I'll try and address those issues in the next slide.

This is our view of what happens, would happen to an acid gas in the drift environment. Basically, under open system conditions, any HCL gas that is generated will disperse in the drift. And in fact, as I've shown you from the TGA results, normally you'd expect that if it's going to occur to happen at the highest temperatures. That's going to happen, therefore, when you've got the benefit of this vaporization barrier so the entire drift environment is going to be well above boiling. There's no possibility of condensation. What we're arguing is that this is an open system, basically doesn't look anything like a laboratory distillation experiment, and that HCL that's generated is going to migrate out through the drift environment, through these fractures until it encounters liquid water. In the high temperature case, as Bo said, that's going to be five to six meters back. And then, because of the Henry's Law Constant for HCL and all of these acid gasses, it's going to partition quantitatively almost into the aqueous phase that's present. So we're talking here, not just about dispersal, like hydrodynamic dispersion, we're actually talking about very powerful sink term in the aqueous phase that's going to scavenge those acid gasses.

And the last point to be made, I'll come back to this, that if you're at a lower temperature region where condensation would occur, the last place you would expect any condensation would be on the waste packages since they are the hottest spots within this overall drift environment.

I want to just quickly pursue this issue to try and show our view of these acid gasses with a couple of simulations that have never been shown before, just done fairly recently, but to start with the back of the envelope, calculation of what the HCL gas might be in the drift environment. So, the way we did that, very conservative estimate, was took the measured mass of dust in the tunnels as measured by the USGS, then used the measured chloride content of the dust and then, as you can see again, about 5 milli-equivalents per kilogram of dust, and then assumed, very conservative assumption, all of the chloride volitizes HCL. As I've said, normally that chloride should be bound up with sodium chloride, should be stable, but let's just look and see what happens. We inject it into the drift. Here's the waste package, here's the drift environment. The reddish orange is the rock here. Eject it into the drift under two scenarios, just to quickly look at. One, where the drift wall is just below boiling so we've just had the rewetting front appear here. But we've still got the waste package above boiling, so it's dryer there. So there's no condensation issues.

The second one, let's look at condensation where we actually have this scenario. I don't know if this is what was considered by Catholic University where down the line in the drift you've got a hotter waste package boiling away. Humid air comes down towards the somewhat cooler drift, like this one, and what happens? So we'll look at that scenario as well. Both the waste package and the drift wall slightly below boiling.

So the first one basically shows that no condensation case. What it shows is after we inject that HCL it disperses rapidly in the drift environment, within about one day due to diffusion, dispersion. Over slightly longer time periods, however, the HCL gas dissolves in the aqueous phase in the rock matrix here, thus lowering its concentration in the drift atmosphere. So you can, by two months we're basically going from about 10 to a minus 6 mol fraction, or volume fraction, down to 10 to the minus 12. So the point here is that reservoir water, wherever it occurs in the drift environment is going to be a very significant sink for any acid gases that are going to develop and this is going to operate over a fairly short time period.

And here's the other case now. With slightly humid air coming from somewhere up the drift, a hotter waste package for example, so we're going to allow for some condensation because you've got both the waste package and the drift wall below boiling.

What we see, however, is condensation exclusively on the drift wall because still its temperature is lower and the RH is higher there. We see the condensation of that lower temperature drift wall, initially because you're condensing very small amounts of water after two hours. Its PH is quite low. All of that HCL gas got scavenged into that little bit of liquid water that's developed there. After 12 hours we've condensed more water. Basically now we're diluting the low PH with that given amount of HCL. We do not have a infinite supply of HCL appearing in this model. This is actually the amount that can volatize off that waste package. We can see further dilution, so by one day we're already up to PHs of four to five. So the point again is that with water, if it condenses will not be on the waste package. It will be on the drift wall. That's the last place it's going to go. Secondly, is that it will tend to scavenge any acid gases that are present. If we followed this longer, we would see this PH signal move over the longer time periods into the water, into the rock, and even somewhat longer time periods we get mineral buffering that was described by Don Langmuir, which is that you cannot maintain the low PH plume anywhere in this environment.

So the conclusions, sort of a summary here is in the unlikely event that calcium chloride or magnesium chloride area present, the HCL gas could be generated during the above boiling period. If this occurs, however, it would be expected to be during the period when the entire drift temperature is greater than about 125 simply because, based on the TGA, we see the maximum volatilization at those high temperatures. And in that period there's no condensation anywhere in the drift environment. Simply moves out. Amounts of acid gas possible are small, placed on the mass balances, but even more importantly, any acid gases generated will be dispersed widely in the drift environment and upon dispersal these will be dissolved into liquid water in the rock beyond the drift, pulling down the concentration in the atmosphere. And if condensation of liquid water occurs accompanied by scavenging, that will take place in the coolest parts of the drift, not the waste package.

So the last topic is to take another look at salt deliquescence with a focus on these two issues just quickly, how much water is absorbed, just to give you an idea of that. And also what the composition brines can actually develop in this kind of multi-component system.

And the salts present in the dust are expected to contain both nitrate and chloride. We have reversed multi-component deliquescence experiments that are used to validate a geochemical model that we've been using to describe this deliquescence and evaporation process. I'll show you one of those. That's all new data. And this, as

I said, point out the mass of water absorbed by these deliquescent salts is very, very small. And finally, return to the issue there of high temperature and lower RH. Eutectic composition has a high nitrate chloride ratio due to the thermodynamics of deliquescence.

And this is some of the experiments done recently at Lawrence Livermore Lab at 120 degrees. Sodium chloride, potassium nitrate system. Here's the deliquescence of sodium chloride. The deliquescence point appears. Potassium nitrate, you can see the experimental data here. The red represents experiments that originally started as solid dust and you basically raised the RH, and deliquescence occurred, and then you measure the solution composition. The black initially dissolved the totally aqueous system where you actually evaporated some of it. And, so you're approaching from the other direction.

And so these are reversals of these experiments. You can see the close agreement and also the time it took to get that was on the order of 48 hours, indicating quite rapid reaction rates here. So this is some of our validation data of the geochemical model, which include the Pitzer database that's to high temperature and also high ionic strength.

This is the point I was trying to make earlier about, while concentrated brines can develop due to deliquescence, we're not talking about big oceans of water. We're talking about extremely small volumes. In fact, for a salt mass of .35 grams we're talking about something on the order of 10 to 100 micrograms, or microliters of water actually absorbed. So we're not even up to a drop or, you know, milliliter sort of volume of drop, which would be up there. To get up to that point, you've got to be all the way up to 95. So the point here is that we really don't have enough water mass, for example, to generate flow. You're just going to be holding that moisture within the capillarity of any dust that you have on the surface.

And then back to this thermodynamic control. You've seen this already, but the point is between the totally dry conditions of RH and totally aqueous solubilized conditions at a higher RH, the solution composition is going to be controlled by this so-called eutectic diagram with migration along the solvus. The point here really is that if you've got a bulk composition of nitrate, say at 0.5, the thermodynamics in this system is always going to give you actually higher nitrate chloride ratios. So it's really only when you climb way up in relative humidity that you can get out these bulk values, and those are shown in the next slide.

I've been holding this off. These are the actual data from USGS showing the nitrate-chloride ratios in the solvus. They are actually pretty much favorable anyways for the whole corrosion scenario. You can see that almost all of them are above 0.5, but the point is that in this system these ratios actually represent minimum values because at lower TH, the thermodynamics of the system drive the solution to even higher nitrate-chloride ratio.

And this observation is bundled a little bit in the next two slides. The first one, sort of a schematic that shows possible fields and relative humidity, temperature. This is an inaccessible region where the pressure of water would have to exceed atmospheric. You can't reach this because of the pressure unless you run an autoclave experiment.

Here, below this deliquescence point, the system should be totally dry. We don't obviously have calcium chloride salts in this system. We're considering this system. But the point is that there's this region here where nitrate-chloride ratios are always going to be greater than 0.5 and that's because of the thermodynamics, just not the bulk salt concentration. It's only once you get up to the 70 percent that you have a possibility of even moving below that value.

And so, on the next slide, if we look at the characteristic temperature RH, first for the repository, let's focus on the blue one. That's the most common. This is kind of the hot one. That's a much colder one. Focus on the blue one, you can see that drift wall gets to 96 degrees in about 750 years. In fact, that waste package in the drift environment spent its whole period above boiling in this region, either under totally dry conditions or if deliquescence occurs, and we're talking now about sodium chloride, not--calcium chloride is long gone and was never there, basically. You're always going to have those favorable nitrate-chloride ratios. But we don't see that an aggressive corrosive brine can develop at all during this above-boiling period basically based on that.

And so in conclusion, due to lack of seepage, as we've said, the chief issue is the deliquescent salts. Calcium chloride, magnesium chloride salts are extremely unlikely to be present as we discussed in the repository. But in any case, if present, they would rapidly transform to non-deliquescent phases due to their instability at temperature, at least as thin dust films here. Any acid gases generated due to salt deliquescence will be dispersed in the drift atmosphere at environment and then dissolve into the water in the rock. If condensation of liquid water occurs accompanied by scavenging of acid gases, it will take place in the coolest parts of the drift, not in any case on the waste packages.

Only nitrate-dominant brines will form during the above boiling period due to the thermodynamics of deliquescence in this particular system. This is over and above the natural variability of nitrate-chloride ratios in the dust which as we've shown as quite favorable generally above .5.

And lastly, the mass of water we're talking about here is very, very small, on the order of microliters. 10s of microliters. And, so that concludes the main part.

This next part is kind of a bonus track on the Catholic University distillation experiments. Their experiment involved extensive boiling of calcium magnesium chloride nitrate water and showed condensation of a low PH water leaving to metal corrosion. I don't want to go into this too much, but the key point I want to ask is this experiment relative to the drift environment.

And, this is sort of contrasted over on this side, the drift environment, our conceptual model for it in this experiment. Features of distillation experiment that do not apply, in our opinion, to the drift environment at Yucca Mountain. First and most important is the highly localized condensation due to the closed system behavior of this experiment. In fact, then later on after they take this part off and add the Soxhlet cuff, they actually have the refluxing of condensed acids, and I think Bo gave a pretty good explanation of why we do not expect refluxing in this heat pipe behavior to occur. In any case would require a very large quantity of water to humidify the environment. We don't see any evidence for that kind of seepage occurring.

A second point is that their experiments, the corroding metal coupons are held at lower temperature than the boiling brine. So obviously they'd become the locusts of condensations of those acid gases. Well, the situation in the drift is the waste package is the hottest spot in the overall environment there, even if you're talking about longitudinal transport down the drift, even then when it gets there, you're still going to find a waste package that's hotter than the surrounding drift wall in other environment.

Another point to be made, perhaps, is that there's something inherently unrealistic about an experiment that involves the equivalent of about 14,000 liters of dilute seepage water in order to conduct that experiment. There would be very much smaller masses of water that would even be there, and therefore, the amount of HCL that could be generated very much smaller. And very large temperature gradients also are not the same as in the drift environment. This of course is partly what focuses this condensation all in one spot.

And so, our argument is that the Catholic

University distillation experiment is irrelevant to the drift environment at Yucca Mountain.

DUQUETTE: Thank you. Thank you for being early. Go ahead Ron.

LATANISION: Latanision, Board. If you could go back to the previous slide where you started with the Catholic, just one I think. Oh, Latanision, Board. Thank you. Could you go back one. I guess it's No. 50 actually. That's the one, yeah.

I take your point in terms of the conversation regarding Catholic University experiments that you're asking is the experiment relevant to the drifts environment. And, I think I would have to ask the same question. Of the tests that have been done by both the project and by CNWRA on magnesium and calcium chloride brines, why are those experiments relevant? Why have we seen so much data? You may not be the right person to answer this question.

STEEFEL: No, I--maybe somebody else can answer. Are you referring to the corrosion tests?

LATANISION: Yes.

STEEFEL: Involving liquid, or are you talking about the TGA now?

LATANISION: No, I'm talking about the tests that have been performed in concentrated brines over the, you know, this is the data that led to our concern in terms of the Board's expression, both in terms of the letter and the report, so I'm just trying to put in perspective, given all that you've told us, if magnesium and calcium chloride dust is not likely to be present, what was the point of all that experimentation?

STEEFEL: I'll let Greg answer that. I could give an answer, but--

GDOWSKI: I think we have been evolving in what we--

DUQUETTE: Please identify yourself for the record.

GDOWSKI: I'm sorry. Greg Gdowski, Lawrence Livermore. I believe that we are evolving into what we believe that the environment is and now that we're getting a better characterization of the environment, the systems that we were testing at one time don't seem as relevant.

STEEFEL: Obviously, the sodium chloride and things like that are still relevant because the--

LATANISION: Latanision, Board. That would be my second comment. If we could go back to, one of your slides shows that transient--temperature time transient--I'm not sure which number it is.

STEEFEL: Do you mean way in the beginning or--

LATANISION: Yeah, way in the beginning. What I'm interested in is--

STEEFEL: Probably about 7 or 8, or--

LATANISION: What I'm interested in is having a sense of over what temperature range we will get back and what time period you would expect sodium chloride or the eutectic mix of sodium--of chlorides, nitrates to be present.

STEEFEL: One more there. Can we go back--

LATANISION: Yeah, that's probably it.

STEEFEL: Okay, that's good. The--

LATANISION: Didn't have a number. That's why I was confused.

STEEFEL: Sodium chloride would be in the high temperature period, of course. The second talk focuses much more on that issue. Sodium chloride in the below-boiling period and that's probably the key player there. But in the above-boiling period, it's basically at these low RHs, sodium chloride can be totally dry, so it's basically only when you get the deliquescence point of that mixture of those nitrate and sodium chloride salts that the RH that's going to come into play, I forget exactly where that is, but that's going to be somewhere I think about 55 percent. So you're really talking about this period from about right there, I think, up to about there, during the above-boiling period. And it might have shown that on that very last slide towards the end that you're talking--

LATANISION: That's right.

STEEFEL: That period of course then the thermodynamics are giving you back high nitrate to chloride ratios, which are going to be well above the actual sort of point where you get significant corrosion occurring.

LATANISION: Latanision, Board. That's roughly what I thought in having listened to your comments. So the corollary question then is the project planning to do tests in those environments which I think we're now coming to a sense of are important. Are you planning to do tests--

GDOWSKI: Greg Gdowski. We have done the tests and Joe Payer will present them later this afternoon.

LATANISION: Great. Thank you.

DUQUETTE: Dan Bullen?

BULLEN: Bullen, Board. Could we go to slide 21, please? The bottom point says that the remainder of the four percent don't satisfy this condition. The first condition was that the dust fails to satisfy condition 1. Is there a heterogeneity in the dust throughout the repository and so do you get variations in the types of dust, or is this just a conglomeration that all the dust, you know, doesn't meet this 96 percent requirement?

STEEFEL: I'll let Tom Wolery, he's done the most work on that. He's got a whole MR on that.

WOLERY: Okay, the data referred to here again from the tunnel dust, leachates, and basically these are processed through EQ3/6 calculations simulating evaporation. We then looked at all of the salt minerals that were predicted to form in these, and then tried to sort these things out in terms of what would control, for example, the initial deliquescence, and that kind of focused us on sodium chloride plus potassium nitrate for about 66 percent of the cases. For most of the remainder of the cases, the same assemblage plus sodium nitrate, and then in a very small number, range, four percent basically those ended up coming up also with calcium nitrate. And we never saw any calcium chloride come up in these calculations, but we do see a calcium nitrate.

STEEFEL: That was true in all of the dusts you looked at so this includes how many samples did you have? 38 or 50?

WOLERY: There's something like, depending on how you count them, about 50 some samples.

BULLEN: Bullen, Board. Were these samples--I know that they were taken at various parts of the repository. Could you tie the sample to the type of geologic strata that you were in? Is there a different characteristic of the dust in the lower lith versus the middle non-lith, versus the upper lith?

WOLERY: I can't really answer that question, but I don't think so.

BULLEN: Bullen, Board. Do you think that's just because the dust was ubiquitous or because there is no difference between those two, or those three strata?

WOLERY: There may be, but, you know, I might defer some of the answer to that question to Zell Peterman who actually took and analyzed the samples, but I know that a big part of the answer is we know that most of the dust in the tunnels is actually rock dust. That contributes particularly strongly to overall composition, but particularly the insoluble composition, but anyway, Zell, do you want to take it from there?

PETERMAN: Yeah, there's a lot of variability--

DUQUETTE: Please identify yourself.

(The comments from Zell Peterman were made away from the microphone. The following is the audible portion of that comment:)

PETERMAN: Zell Peterman, USGS. There's a lot of variability in the overall dust composition (inaudible) elements and traces. Some of it seems to be related to how close you want to (inaudible). So there's probably a component of experiments of film dust coming in there. Within the main, the lith main drift (inaudible) and the rock (inaudible). (Inaudible) and the total (inaudible). No significant gradient of (inaudible) within all the different zones, the lithophysal or non-lithophysal zones.

So that component which is, you know, 98-plus percent of the dust is (inaudible) composition.

BULLEN: Thank you, Zell. Bullen, Board. I just had a couple more questions. Can we go to slide 38, quickly. I was very intrigued by interpretation of where the acid gas is going to condense and how it would be diluted, except where I see in your model is that you're missing some structure here. You're missing the drip shield, you're missing the internal construction components of the ground support and so the question that I have is, if there were condensation under the drip shield, would you expect the acid gas to then be focused to an area where there may actually be, and again, variability, along the drift and may end up with a cool spot where I've got condensation under the drip shield and so instead of getting the acid out into the rock, I produce the acid solution even in my new quantities at the surface of the drip shield and actually either cause failure to the drip shield or failure to the waste package because what I've done is essentially concentrate all the acid gas into the area that is systemic. I have a perspective as to why I ask this question. Years ago, Rich Attebury at Livemore took a look at radiolysis effects and found that the nitrates that were produced in accelerator environments ended up on condensate water on copper piping and corroded the daylights out of it. And so, this condensation of gas is actually a key issue that if you are actually making it and it condenses in the wrong place, not in the rock, but on the drip shield, could that pose a problem?

STEEFEL: I think that, first of all, of course, that we expect if there's any volatilization it's going to occur at the highest temperatures when the drip shield would be well above the boiling point so condensation isn't going to occur. But just to follow this scenario out, it's true then that first of all that the drift wall is still going to be a lower temperature than the drip shield. So if that gas can get out it's basically going to migrate out to that zone condensed air and then work its way into the water actually held within the rock mass. Now, if you had a tight, for example, a tight drip shield, then you're going to have to rely on basically longitudinal drip transport to go down through the system and get out of the system there, but again, the drip shield is really not, you know the design is a quantitative thing that's going to capture anything there. It's still going to move longitudinally along the axis of the drift and of course, that's--isn't actually shown there.

BULLEN: Bullen, Board. Just one last quick question. Do you produce acid gas above the boiling point?

STEEFEL: Of what? Boiling point?

BULLEN: At the repository horizon. Do you produce the acid gas about that?

STEEFEL: What do you mean above--you're talking about the TGA, you see, you don't see it at 100 degrees. You see it at 125, so it's only the deliquescing ones there.

BULLEN: Bullen, Board. Then one would ask the question, why would one want to go above the boiling point?

STEEFEL: Because you've rendered these--you've rendered all these slats benign by getting rid of the HCL.

DUQUETTE: Let me take the Chair's prerogative for a minute and say, and pick up on a theme that Ron Latanision picked up on this morning. You indicated that the water is not going to get in there through seepage, it's not going to be stable for corrosion processes on the canister wall. Why on earth do you need the drip shields?

STEEFEL: Yeah, that came up before. The drip shield really plays a role in the lower boiling period. And you'll see when I get into that why the drip shield is useful. I'm not sure that you need it during this stage at all.

DUQUETTE: Terry, you have a question?

CERLING: Yeah, I have a couple of questions. One--Cerling, Board. One question, you've showed us how hydrochloric acid is generated in this. In any of your work with the nitrate compound, did you ever get any generation of nitric acid?

STEEFEL: Yeah, I'll let Greg--Greg has done--

GDOWSKI: Greg Gdowski, Lawrence Livermore. We've only done a few experiments with mixtures of chloride and nitrate. And, calcium chloride is actually less deliquescent than calcium chloride, so what we saw while we were--

CERLING: Nitrate. Nitrate.

STEEFEL: Calcium nitrate.

GDOWSKI: --is less deliquescent in calcium chloride. So what we saw was when we evolved the ACL gas from that mixed solution, we saw precipitation of calcium nitrate and this insoluble calcium hydroxic chloride. We haven't actually done it at higher RHs. But it might still remain soluble with the calcium nitrate.

CERLING: And then, if we could drag Zell Peterman back. The question that I wanted to follow up on was the one earlier about the inhomogeneity of the salts and obviously one of the sources of salts is whatever construction activity there is, and the other has to do with whatever is really contributed by the rock. And so, I was wondering if in your collection of dust samples if in areas where seepage was more likely to be involved in the contribution, is there a difference in the chemistry that you could attribute to sort of the rock end member versus the anthropogenic end member, and--

PETERMAN: Zell Peterman, USGS. No, we haven't seen that. The, you know, if we use the calcite (inaudible). Okay, is that better?

The area where we would see the greatest seepage was is under the--under drill hole (inaudible). That's where the greatest abundance of calcite deposits occurred. No, we did not see any corresponding variation in the dust, and I guess what I'm hearing was once you're in the main drift and then, you know, ventilation system, things getting pretty well mixed up, that's sort of the goal when they are collecting dust and raising dust and they see it dispersed and moving down, you know, mixing, and you can see that. The other component, well, we think the components are construction water. We can tell that by looking at the bromine, by natural pore water salt that form on the tunnel walls and then there's, you know, other things, like ground--conveyor belt. You can see a lot of black specks, which are carbon. I'm sure (inaudible) lot of different components, but once you get away from the North Portal, things are relatively intertwined. The dust composition. Both the solubles and the (inaudible) both dust compositions. I don't know if I answered, responded to your question properly.

CERLING: Pretty close. I guess following on that, do you think that you could come up with perhaps a few and perhaps only one, sort of end member composition that is contributed by the natural system? Is that possible? Or how many dust samples would it take to be able to calculate that end member value?

PETERMAN: No, I think we can. The silica rhyolites predominant rock in the tunnel and whether it's the Topopahans that got a little bit riled at it, at the T results of that rhyolite. Now, at the upper part of each of those units there's of course latite, and that has a different composition than the rhyolite. Some key elements, you know. It's higher than titanium, higher and higher. It's still got, you know, 73, 72 percent SiO2 compared to a 76 percent for the rhyolite. So it's not a really big difference, but we can pick those sort of things up. Because our paper, I think it was the high-level waste last Spring, a year ago, and just, you know, using these prior diagrams could compare with things you can see what's enriched in the dust relative to say the rhyolite.

CERLING: Yeah, I was thinking mainly in terms of the soluble component. Cerling, Board. I think thinking more of the soluble component rather than the insoluble--silica and so on.

PETERMAN: Okay. As I say, it's sort of a random--in the soluble there's sort of a random variation. Once you get away from the immediate effects of the North Portal.

CERLING: Okay. Thanks. I've got one follow-on because I can't resist. I have to ask a Bullen-Board question. So this is Cerling, Board asking a Bullen, Board question.

From your slide 6, you might be able to make the argument that perhaps a higher temperature is actually better in terms of the corrosion issue because it gives you sort of a protective period, So in asking that I would say ask the anti-Bullen, Board question. Is a higher temperature design in this particular corrosion question, is it actually better than a low temperature design?

STEEFEL: The only reason I--I mean you said that was a nice try, but the only reason I agree with you on that was that I don't expect calcium chloride and magnesium chloride salts to be there. So that volatization issue is probably a non-issue from the beginning. But the additional benefit, of course, is that extended vaporization period where you have nitrate and chloride ratios at favorable. In any case, I don't see big objections to it. It seems like these issues, they don't make it worse. You may not get a big benefit out of it.

PARIZEK: Parizek, Board. On the amount of acid that, gases that could be produced, I didn't get a sense of volumes or whatever, but if it heads for the wall early in the placement of waste packages, it sees pore water and ends good. It disappears in there. What does that do to the chemistry of that water? Does that chew away on the rocks and so, you know, we heard from Bo how nice the water chemistry could be, but is it likely to become normalized if in fact acid is produced or how much acid is it? I mean it's going to get neutralized, as you pointed out, but--

STEEFEL: I mean obviously, somebody could go out and design an experiment where they took, you know, huge quantities of HCL and dissolved small amounts of rock and you could then produce a very concentrated. The issue is that you're talking about a small amount of HCL that have been dissolved in a fair amount of water with even more rock out there. And then basically the scenario I think is, as Don Langmuir described yesterday, at that stage the HCL becomes a dilute, fairly dilute weak acid that reacts with just like weathering with the minerals and the rock. And then there's more than enough of that material there to buffer the PH and bring it out. When we actually did those earlier ones, all we did was take the HCL and put it into the water. Even there we saw only a shift of PH of a few hints, you know, to have the PH in something like that. You're talking about small amounts that the natural system shouldn't have any problem dealing with, but I don't know if, Eric, you want any comments to add to that. He has worked on the drip scale and the whole business of the reactivity of the system and--

SONNENTHAL: Eric Sonnenthal, Lawrence Berkeley Lab. Yes, actually, the PH shift is virtually not noticeable in this case. The heating of the flow waters that's coming down to the drift is actually devolatizing CO2 and increasing the PH slightly in those figures and so you can't even see the PH shift of the HCL. You have so much rock area where that gas is going through fractures and is dissolving the rock, there probably are some shifts in the reactions, but it's going to be not too noticeable.

PARIZEK: Thank you. That's trivial, sounds like, then. And, the other question about the Catholic University, all their making is to create things other than dust and that's dripping water, which then evaporates and leaves some scale on the waste packages. Now really, in the placement of the waste package underground before boiling temperatures occur in the rock wall, you've got a lot of water in there, and now you could have dripping because of this redistribution of moisture in that short time period. You folks discount then this dripping water and the possibility of developing scale, as we heard yesterday from Catholic University?

STEEFEL: This is during the heat-up period?

PARIZEK: Right. Just early in the heat-up period. I mean you're getting into the point where you don't get half this boiling rock vapor barrier thing that both says will exist that you need very protected. But to get to that point you've got water in rock early on.

STEEFEL: Bo could answer that, but I mean the capillary barrier is still in place there, so you're, you know, and you're talking about the present regime with low, you know, the climate is such that the most we're getting basically for waste package in the extreme case would be about, you know, a kilogram of water or something per year. And that doesn't even take into account the small percentage that's going to actually seep, which is somewhere, forget what it was, three to five percent. So you still have a capillary barrier.

Secondly, is that, you know, you're not going to get those concentrated brines then. You're talking about before heat-up. You're basically going to heat up that drift wall within, say a few years, assuming the waste packages are hot so you've got a very limited time to do any seepage, and actually evaporate something on the surface of the package.

PARIZEK: Parizek, Board. Again, if we look at the heater, big block heater experiment rather, there was free water outside of the shielding, but you couldn't go in after the experiment mode so you could get water because it seems to be possible, really, in these experiments. So even though you have a capillary barrier, you still had free water in at least two locations of the experiments that we saw in the field.

SONNENTHAL: Eric Sonnenthal, Lawrence Berkeley Lab.

We only saw water out in those backed-off hydrology boreholes when the temperature was very close to boiling and the vapor, the water mass fraction in the gas is extremely high. So you have a very short period of time way out in the rock, and that's the only time we ever collected water and it was only out in the rock.

PARIZEK: Parizek, Board. Was there not water on the floor just entering up to look into the gauge of whatever that heater experiment was conducted before?

SONNENTHAL: Not in the drift. We've only seen water that condensed on the outside of the bulkhead.

PARIZEK: That's the water I have in mind. The bulkhead was not totally sealed.

SONNENTHAL: Right. In the drift we've never found water.

BODVARSSON: Bo Bodvarsson, Livermore Berkeley Lab. Just to clarify, that's water outside which is condensation of steam that went through the bulkhead. So it had nothing to do with, really, the canister of water or seepage.

ABKOWITZ: Abkowitz, Board. I'd like to take this discussion to new heights, namely 50,000 feet since I'm not a member of the Yucca Mountain underground, but I do think I can contribute at that level.

What I think is that we have a situation here where corrosion has been expressed as a concern by the Board for quite some time, more than 10 years. It has survived many different transitions and board members and different political administrations and so forth. And, the Board has been sending a message for a while that this is a concern and it sort of put an exclamation point on that message last Fall. What I had subsequently heard from the conversation today is that the environment under which tests have been conducted for many years, expending many millions of dollars, is not an environment that's plausible. And that this is something that has just been learned over the last few months and is being presented to us today, and so basically the concerns of the Board are, in DOE's eyes, a non-starter.

As someone who has kind of, at least for this Board, a mantra of taking the time to do things right, it seems to me that this new information and new conclusions is a, you know, rather sudden set of accomplishments, and the project at this complex, when that pace of learning is occurring at such a high rate, it tells me there's probably a whole lot more learning that needs to go on in this area or other areas. And so, I want to, you know, I want to go on the record as expressing a concern that if we're still learning these types of things at this point in time, you know, I wonder whether or not we should be at a point where the repository design is always so well defined and ready to go. So I just express that concern and I invite anyone from the agency to respond.

ANDREWS: Yeah, this is Bob Andrews. Let me try to respond a little bit. It is true that some of the testing from corrosion perspective was initially conducted in calcium chloride type brines with varying amounts of nitrate. You'll see some recent data collected over the last six to nine month in sodium chloride type brines and other brines including sulfate--in fact, I don't think we're going to show the sulfate data this afternoon. You saw some sulfate data yesterday from NRC. So the testing, types of environments tested, the range of temperatures and range of chemistries has evolved in continuing testing of a range of environments.

However, I think the point we're trying to make, especially in Carl's talk, is the Board put some pieces of information together based on the presentations last May that were in fact in correct. That although we do observe calcium and we do observe chloride, we don't observe calcium chloride in the dusts, in the salts, present at

Yucca Mountain. There's no evidence for that. In fact, even if it were present somewhere in the southwest, and there have been sporadic, I think as Carl pointed out, observances of calcium chloride dusts in the High Sierras, I believe. They are not stable.

So, for those two reasons alone, let alone going to the volatilization at higher temperatures, the Board's premise in their November 25th report is not correct. You know, we apologize for the Board having come to that conclusion, but it was based on information, I guess the Board put together and the fact that we did not, as I said in my introduction, talk specifically about what were in those salts when we presented the information last May. That is recent information. You're absolutely correct. So, yeah, you're right. The Board had some inferences derived from some information, not the complete story, and we're trying to provide that complete story today.

ARTHUR: Also, John Arthur, Department of Energy, and I must respond also. I think if anything, many of the comments which you suggested are made by previous boards, but if anything, I think we try to be responsive to the letter and present our case today which is many of us tried to do. As far as the issue raised about designs (inaudible) we've been continuing to (inaudible) of this design and I feel it is optimized now. I feel that a selfservice (inaudible) presented yesterday. We're making very good progress and we have something (inaudible) to the Regulatory Commission. And the ball is in our Court, so I (inaudible) come up with --set of science today.

PAYER: All right, Joe Payer. (Inaudible) the other end of that and that is the corrosion strategy throughout the years as somebody who has been involved in peer panels a little bit, DOE has worked in this area over a number of years. And over the last year and a half, had the charge from the director to look at the corrosion program strategy and the way it's laid out. A basic foundation of that strategy is to identify the types of environments that may pertain at Yucca Mountain and those that won't pertain at Yucca Mountain. And that has been the focus of Carl's first talk here, and we'll continue after lunch.

The other basic premise is to identify the corrosion behavior of Alloy 22 in a range of environments so you're trying to match the realistic environments to the realistic corrosion behavior, and you want to go beyond that. The project has chosen not to go as far beyond that as the State of Nevada into boiling barite chloride and some other elements that we believe just don't need to be tested, but you know, to look at the calcium chloride environments is very relevant because some of you have asked and others as well, well, maybe we don't believe, or what if something happens where both--what would happen if Alloy 22 was exposed to these environments? That's why we're doing the tests, and we'll talk about that later this afternoon.

So, the other part of it is that Carl's talk here is talk when the thermal barrier is still in place and this afternoon he'll talk about when we can have dripping and seepage conditions. And that's a whole different ball game at that point. And so, the corrosion becomes more of an issue, we believe, in that case.

SHETTEL: Don Shettel from the State of Nevada. I have several comments. I had a comment on the drip shield, but I think I can drop that now.

Sepiolite, we would agree that in certain environments, certainly you can get magnesium removal, such as seawater running through Ocean Ridge, the salts. But the environment at Yucca Mountain is totally different.

Regarding HCL, this also has something to do with the drip shield. But if you recall one of my last slides where I had the C-22 in the boiling HCL, and we got the part that was exposed above the liquid was almost totally gone. Now, I believe if the HCL is being--is reacting with the C-22, we're not talking about diffusing way off into wall rots. It's reacting right next to where it's boiling and that's the chemical sink for it, doesn't have to diffuse away from the canister, and doesn't require cooler temperatures or anything else.

Regarding the comments on the Catholic University experiments, I think Dr. Staley covered the similarities there between the experiments and the drift. I also want to say that we don't have to concentrate these waters before they hit the drift. I mean the canisters. They have experimented. We started out, certainly, with concentrated experiments and some other things like lead, but recent experiments were very, starting with very low and essentially 1X pore water. It is possible to get corrosion with just 1X pore water, so these large quantities of water that you're referring to are not necessarily required. And, I think one of the main differences between what you're talking about, which is deliquescence of dust without any addition of seepage water and what we're considering, which is the possibility of getting seepage water through the so-called vaporization barrier during the thermal pulse is very likely because you have episodic flow of water and pulse in fractures and therefore it is a possibility. We're not saying that everywhere you get this, unlike what you are saying that everywhere you have very dilutant benign waters.

I think that's all unless you want to make a comment.

STEEFEL: Let's see what would be--oh, the one on sepiolite, I don't have much to say there. I mean at Mid-Ocean Ridge at Marvin was just thrown out as an example, but it's actually precipitation of magnesium silicate he has observed in the--

SHETTEL: Oh, yeah, yeah. I want to comment on that. The precipitation of magnesium and other clays, for instance, require aluminum and you really don't have much of that in the unsaturated zone pore water that's at other repository levels. So without aluminum, you really are quite limited in magnesium removal methods there.

STEEFEL: Yeah, that's required in the--but I am still not convinced that there's no sepiolite developed because if you've got aluminum that's presumably why, one of the reasons why sepiolite didn't form. Just simply you had a more stable magnesium clay you could form with an abundance of aluminum present in solution. So--

SHETTEL: It still has to be shown experimentally other than just modeling that you can get such a phase.

STEEFEL: If it's observed in other, you know, I've seen it--I haven't done a thorough literature search, but I'm certainly seen it in the opalinus clay experiments where sepiolite was identified via x-ray. Now, that's a hyper alkaline.

SHETTEL: I was going to say, how relevant is that to Yucca Mountain?

STEEFEL: It might be and might not be. But you're saying that it has never been observed, so-- And what was your second thing?

SHETTEL: Also, Gdowski, you did find some magnesium smectite in his experiments.

STEEFEL: Well, that's similar to the magnesium clay that's found by Carroll and Gdowski.

SHETTEL: That was an experiment that had tuff added to it. We're talking about the evaporation of pore water--

DUQUETTE: I'm going to have to--you know, if we're going to have any lunch at all, I have to stop this discussion. Now, there will be more time for discussion later on. I'm going to take two quick questions, one from my colleague, Dr. Latanision and the other from Carl DiBella.

LATANISION: Carl, you want to go first? Go ahead.

DIBELLA: Could I have your conclusion slide again? And I'm going to try to be very quick. You had two conclusion slides. This is Carl Dibella, Board Staff. And maybe the second one first and I'm going to try to work backwards, very quickly through them. It's like No. 42 or 43. No, it wasn't that. Okay, let's do the next one, we'll just work backwards. Can I have the next slide, please, 49. I'm going to work backwards.

The mass of H2O taken up in the salts is very small. I think everyone knows this. I would hope maybe when Joe talks this can be translated into, if there is corrosion, how many holes or what's the size of the holes or how deep are the holes or something of that sort, what's the relevance of that? We tried to, at least I tried to, think of what is the lower limit of corrosive material that would not be of concern, and I don't know what it is, but maybe the project does. So maybe Joe can do that.

Working backwards, this conclusion is, a conclusion I think is we can all agree with. But it's for a very different system than the system that the Board was concerned about and it's paper. The system the Board was concerned about was the calcium magnesium chloride deliquescence. This is sodium, potassium chloride, nitrate deliquescence that is simply another issue. And furthermore, as you showed, the sodium nitrate has a much lower deliquescence point than sodium chloride and so its behavior is nothing like the calcium chloride, calcium nitrate, magnesium chloride, magnesium nitrate systems. So I don't see the relevance of that conclusion to the Board's paper of last Fall.

Could we move backwards again? One slide backwards, 48. Condensation, scavenging of acid gases absolutely will take place in the coolest parts of the drift, not the waste packages. But that's not an issue that the Board brought up in its paper. We are not suggesting that the acid gas reacts with the paper, with the package in the paper. We're suggesting that the acid gas needs to be removed in order for--if the acid gas is not removed the calcium chloride stays where. That's simple thermodynamics. And, if the calcium chloride stays there, the project has shown us data that there is corrosion. So the question is does it stay there? The data shown to the Board which we put up yesterday with a thermogravimetric experience, experiments, where the hydrogen chloride was swept away by the movement of gas through the system. And therefore, it had to be continually replenished by decomposition of calcium chloride. If it's not swept away, of course, the calcium chloride stays there. So the question in the Board's paper was, please show us how the system, the condition in the hydro--thermogravimetric apparatus are the same as the conditions in the repository?

STEEFEL: Another aspect of the thermogravimetric apparatus is that it was actually operated in a relative humidity above the deliquescence relative humidity so it was forcing the reaction to the right with that point of view also, so there may well be an explanation. But we haven't received it to date. The acid gas will disperse. There's no question.

DIBELLA: The question is how fast does that happen? If we're going to disperse by the convective forces in the repository, how do they relate to the thermogravimetric apparatus, number one. They are going to disperse by plain old ordinary diffusion. How fast is that going to be? If it's very rapid, fine, if it's not rapid, we might have a problem.

STEEFEL: I'm not sure I didn't show that. That was the point of those simulations obviously. Those were preliminary in nature, but they basically showed the dispersal of that HCL gas in the drift environment using the model that's been used to describe gas migration, obviously focused on CO2 in the mountain and in all the drift scale tests. So if it, you know, I didn't present a detailed one-for-one table, but there's certainly some presentation of the transport of HCL away from that waste package.

DiBELLA: That kind of thing was fine. I think it was 120 degrees and--but that sort of simulation is a very good idea and I did notice that the time frame for dispersion was much higher, much more time is required than the gravimetric experiments. It only took like 20 hours.

Let's see, Point No. 2, the rapidly transformed and non-deliquescent phases, do they--yes, they are unstable, but again you have to remove the HCL where they become stable. And, how fast does the HCL move is a real question.

Now I get to your first point which was are the salts there in the first place? And I certainly think if they are not there, we don't have an issue. And so it's very worthwhile looking at this, but the technique you're using of dissolving the salts and then re-evaporating them assumes equilibrium of the salts, and one has to say, why equilibrium is a good assumption. And I could make an argument for why it might be a good assumption. If those salts had been exposed to a lot of rainfall before they came into the repository, fine, but you didn't make that argument.

STEEFEL: And that's--can I make a comment? That's where in that switching skipped over that slide. Can I go to about slide 24?

DeBILLA; It is 24 and you very specifically mentioned disequilibrium as being a possibility.

STEEFEL: Yeah, so that--no, I skipped over this. The point here was that, yes, you could have, if these things blew in from somewhere, they would initially be expected to be a mechanical mixture with no aqueous phase there. The argument here is that, as soon as initial deliquescence occurs, you're basically going to expect to get a transformation, for example, via some reaction like this one, calcium chloride plus the sodium sulfate goes to a more stable phase. The experiments that have been done so far indicate quite rapid action time so there doesn't seem to be a huge kinetic barrier. And secondly, the reaction products have a higher deliquescence RH and that's what causes them to dry out. But this is the argument, the secondary argument has to do with transport from any particular site or--

DIBELLA: That didn't seem to be one of your conclusions, but nevertheless, we're probably--the thermodynamics, I absolutely agree with. But the--we're dealing with solution of calcium chloride, more like calcium chloride in other stuff that is completely saturated thermodynamically. How is that reaction going to, all those things going to get together for that reaction to take place? If they do get together I agree the reaction will take place. And, you need to explain that.

Now, one last thing and that is that we didn't have the data, although the data were available, we didn't have the data on the composition. Well, the data weren't available. This is new data for me on the composition of the salts. But we did have one key piece of data and that is this experiment that was done largely in our laboratory on pore water. It was a synthetic pore water, but it was based on measured pore water where they evaporated it, not in the presents of tuff, and came up with tachyhidrite. Yes. That's a calcium, magnesium, chloride salt. That is very worrisome. You can't--you didn't mention that piece of data. Is it raw? Is that maybe only one percent of the pore water, did they not put the synthetic pore water together correctly? I don't know what the answer might be, but if that--that's a piece of data that just can't be important.

STEEFEL: Well, that was evaporation experiments of the subject of the second talk, but maybe--Greg, can you comment on--

GDOWSKI: Eric has a comment.

SONNENTHAL: Eric Sonnenthal, Lawrence Berkeley Lab. I think that one pore water that Rosenberg et al paper used was not an actual pore water composition. There were several values in that report, and that happened to be just a hypothetical case. So more in the middle of the report. It's unlike other compositions.

DiBELLA: It was based on measured pore water. Absolutely. Now, whether it was based properly on measured pore water--

STEEFEL: My understanding was that it was something of a synthetic pore water, but that there were some general samples, not from the repository horizon, but above the repository horizon that had that general signature. I don't remember exactly what, for example, silica concentration they used there. It's still--there's still a question, given such, you know, evaporation experiment that you can often form transient phases that may not be applicable over the somewhat longer time frame of interest in terms of corrosion. I mean that's the argument. Any time you apply an equilibrium analysis, it assumes some time frame and so the question is is that evaporation experiment the same as the relevant time frame within the drift environment, which normally I would expect to be longer. So, that would be my main point about that.

PYE: Pye, Board staff. It's our recommendation of the Board--excuse me the project showed that in the in-drift environment the drip shield and the invert coupled very closely. Indicia cited for condensation under the drip shield was the drip shield was cooler than the invert and that condition seemed to be fairly prevalent. I guess my question is, since I've looked at slide 38, the other piece of the EBS that's missing is the invert. So my question is, what recent assessments have been made to assess the potential floor condensation in the in-drift environment on the wall, on the drip shield and the waste package and is there an AMR you could direct us to?

STEEFEL: I can't do that, but maybe Eric can.

SONNENTHAL: Eric Sonnenthal, LBL. Regarding the first part of your question, we're plotting here the PH in the water phase. There actually is an invert in this model. But it's--you can actually see part of it is wet, down below. I think I have to leave the second part of the question--I'll take that--

ANDREWS: Yeah, John, this is Bob Andrews, BSC. There are three AMRs in fact that hit various elements of condensation and convection in the drift. One specifically is devoted totally to that subject called In-Drift Convection and Condensation Model and Analyses. Two other AMRs hit on it. The Thermal Seepage AMR and the Thermalhydrology AMR discuss the concepts of lateral flow and invection down the drift, but the actual results being used are from the first one I mentioned. In-Drift Convection and Condensation Model. As I said at the very beginning, we didn't think we had adequate time today to go into the details in the in-drift convection and condensation. It is being included in the total system performance assessment, and we'll be happy to talk about that next time.

DUQUETTE: I'm going to cut the meeting off here since we're well into the lunch hour. Let's convene in exactly an hour.

(Whereupon, the luncheon recess was taken.)

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

LATANISION: Let's take our seats, please. We have a pretty full afternoon, and so if everyone could be seated.

When we began yesterday, I made the comment that I had as a personal goal providing an opportunity for a full and objective hearing of the concerns that the Board had expressed regarding the Project's corrosion studies. And I want to thank everybody who's been a part of the conversations over this past day from the State of Nevada, from EPRI, from the NRC. I think--and, of course, from the Project. I think we've had that kind of conversation. I'm really very pleased by that. So, I do want to thank everyone for the spirit in which we've had these conversations during the past day. And I hope that we'll continue this afternoon.

We have two speakers. We'll continue with Carl Steefel who will talk about thermal chemistry and evaporative concentration evolution, and then we'll hear from Joe Payer. Carl?

STEEFEL: So, hopefully this talk will be a natural follow-on to the preceding one. We might even revisit some of these issues. But, as I say, as I said in the beginning, I think there's a natural division between some of the issues during the below-boiling period and the above-boiling period. Can we get the next slide, please?

The acknowledgements here, a similar cast of characters with talk preparation by Dave Sassani, Nic Spycher; the pore waters, Zell Peterman and Nic Spycher at Berkeley Lab; in-drift chemical environment, the same cast we had earlier; and then at least some discussion of the THC evolution where Nic Spycher and Eric Sonnenthal gave me some help. I don't know if Eric is still here. He had to--well, we'll see. If we could get the next slide?

I want to start with some of the Board's comments on seepage chemistry that provides some motivation partly for what I'm going to talk about. They said the DOE's analyses of water chemistries and their corrosive potential are extremely complex and suffer from empirical and theoretical weaknesses. Thus, the Board does not have a high degree of confidence in DOE's conclusion that any seepage water would be dilute or noncorrosive. This is from the Executive Summary.

And in response to the Board, we are presenting here an overview of the processes affecting seepage water and evaporated brine chemistry and provide a high-level description of how these processes are addressed. I don't think I'm going to be able to explain everything of what's done there, but hopefully enough to get people started so if they want to pursue the issues, they can actually go to the individual technical documents. Next slide, please.

And so, as an overview, a brief definition of the below-boiling period. What processes then we want to consider, what processes affect the chemistry of solutions that could contact the waste package, and what are the sum total of processes? One of these is what the natural variability of ambient pore fluid compositions is and how will this variability affect in-drift chemistry. And another important question to be considered, how does the thermal-hydrologic-chemical evolution, we call that the THC evolution, affect the composition of potential seepage water? And, lastly and importantly, how does evaporation in the drift affect the chemistry of solutions that could contact the waste package?

And then we want to kind of, at least, make a preliminary attempt at rolling all this up and say something about then how this chemistry, which we're going to be talking about here, is coupled to the Time-Temperature-RH histories of the emplacement drift. So, we start to get an image then of how chemistry seen by a particular waste package evolves through time and have some conclusions at the end. Next slide, please.

And, of course, the purpose here in evaluating the chemical evolution of fluids is that aqueous solution types affect Alloy 22 waste package corrosion differently. This is a kind of bubble diagram you might have seen before with decreasing RH, increasing evaporation this direction. You start with some kind of dilute water, obviously, and depending on what kind of reactions take place, concentration mechanisms, you can form different kinds of brines. And, of course, these brines will have very different affects on the corrosion of the waste package. So, that's why we need to look at this issue. Next slide.

Before I go on, though, I want to make one point about in which cases will the chemistry of seepage water be relevant. This is really a perspective slide here. Say the chemistry's really only going to be relevant when seepage can occur. Seepage can only occur when the drip shield fails. And even in the absence of a drip shield, seepage can only occur in a limited number of waste packages because of the efficiency of the capillary barrier. The point here is, if you let a geochemist get up and talk about geochemistry, they'll talk until the cows come home, but we have need to keep this kind of thing in perspective that we're talking about various scenarios that may, in fact, either not come into play at all or only come into play in a limited set of conditions.

And so back to this slide here now, the definition of below-boiling period, now we're looking at the period where boiling at the drift wall ends somewhere around, for an average waste package, 750 years or so. How far out here you take it depends on your point of view. Joe Payer is going to be showing some evidence in the next talk that really below about 90 degrees Centigrade, corrosion is not a significant issue. So you can--if you chose that, then really the window that we're looking at here is somewhat narrower. But, obviously, it corresponds to this kind of period where the RH now is going from about 65 percent, in this case, close to--yeah, about 65 percent up to as much as about 80 to 90 percent. So that's the temperature RH window that we're going to be looking at during this period. Next slide, please.

And so the processes that affect chemistry of the solutions in drift environment include the initial pore water chemistry and also the thermal-hydrologic-chemical processes in the rock. I want to make this point, hopefully, quite clearly in the presentation that it's not necessarily a simple evaporation of pore water chemistry. There's modifications to the water that we can document through these THC processes, and they have to be considered. And then, finally, the evaporative processes inside the drift. Next slide. And so to start with, let's look at the variability of pore water chemistry. Next slide.

What is the natural variability? Well, we've--we, I say the Project, has collected samples of pore fluids, collected and analyzed them from all potential repository units now. I'll be showing you some new data. I don't know if I know every last data point of what's new and what you might have seen before. I know at least some of it. But these pore waters show a substantial range of major cation proportions, especially calcium, sodium, and important anions, chloride, nitrate, sulfate, and carbonate. So, there's definitely a range involved here. But observe, for example, is that calcium generally becomes less abundant with depth due to calcite precipitation and, to a lesser extent, ion exchange with sodium. There is a depth trend there.

Nitrate/chloride ratios, which are of course important for metal corrosion, are quite variable here. So, what we want to do is take those natural pore water compositions, basically use them--I'm starting to talk about some of the procedure here--use them as boundary/initial conditions for the THC simulations such that they span the range of the entire ambient system. Next slide.

And this is one of these ternary diagrams, quadrilateral diagrams, that you've probably seen quite a bit of here. If you focus on the cations, this actually represents all the pore water samples from the actual repository horizons; the upper lith, the middle non-lith, the lower lith, and the lower non-lith. Color coded and, this way, you can see focusing, for example, on the cations that you get the middle non-lith and the upper lith clustered quite nicely but still showing some evidence for a distinct trend in terms of exchange with sodium such that when you get to the lower stratigraphic horizons, the lower lith, we've actually got significantly higher sodium in the pore waters and then even some samples from the lower non-lith that fall down here with essentially no magnesium in there at all. So, there is some evidence here of a trend to this, this sort of --this array of calcium and sodium numbers. Here, also, trends in terms of bicarbonate versus chloride, which is the dominate cation.

Now here, up here, portrayed as the same data, of course, projected onto this quadrilateral, this is where we've shown the compositions that are actually chosen for THC model input; chosen hopefully such that they span the range, encompass the range of variability observed in the natural system. We can see one of our samples. W-0 quite closely approximates some HD perm waters that were collected in the past.

Some of these waters here in the middle non-lith are actually new analyses. I don't think all of them are, but some of these are new. They actually cluster quite nicely here and define this set of samples as a bona fide type of water within the mountain there, but we've also selected compositions that cover the remainder of the range. About the only part of the range we haven't covered significantly here are some of these yellow samples from the lower non-lith. I should point out those are about three percent of the repository, so they're not all that significant. They're close, pretty close, to underneath the repository. The rest of these are not underneath it. They're well within the repository horizons. But you can see, in fact, those are sodium-rich analyses.

One of the points about this, in fact, is that these new samples which cluster quite nicely here--in fact, it's not shown on this diagram, the nitrate numbers--but, in fact, a lot of these or all of these new analyses have quite a high nitrate with very consistent values ranging between about 35 and 44 ppm. So, whether that suggests that, in fact, most of these higher chloride values should have higher nitrate, we still haven't established that. That shows up in the next slide.

As I said, quite a variable range here of nitrate/chloride ratios, but some of those I was referring to in the blue actually cluster quite nicely with significantly higher nitrate values even though their chloride is high there. So this is a natural variability in nitrate chloride along with all those other chemistry that we want to take and propagate through the natural system.

Now, this does bring up the issue of whether nitrate could be reduced or degraded in the natural system. I think it's possible that some of these reactions might occur at higher temperature as long as there's not complete dry-out. But the question is, we don't see any reason why there should be more nitrate degradation, microbial reduction, at higher temperature than we basically--than we observe now in the ambient system. In other words, I think the ambient system is probably capturing whatever degradation is going on there.

It should be pointed out in general that the nitrate reduction is not going to be favored in this kind of environment because of the presence of abundance oxygen. Basically, any electron donors of which organic carbon would be normally the most important are going to follow aerobic respiration as a preferred pathway. They're going to use oxygen. It's really usually only nitrate reduction is observed when you get a depletion of oxygen as occurs, for example, in organic-rich sediments or some environment like that. So, it doesn't seem to be a significant issue beyond what sort of levels we're observing today. Next slide.

So now we'll look at the effect of THC processes on seepage chemistry. Next slide.

The purpose here is looking specifically at fracture fluids because they're the ones that can seep, potentially seep, into the drifts. The pore water chemistry are actually only indirectly relevant because it's not necessarily driven out of those pores. Purpose is basically to propagate that range of ambient pore water compositions through the THC processes to determine the chemistry of potential seepage. So, again, rather than evaporating directly, we're processing those pore waters through the THC system.

Conceptual approach: Capture coupled effects of solution-mineral-gas reaction, gas-liquid transport, and heat transport in the mountain. And we validate it. I keep saying we, I didn't do much of this myself, but this is the royal "we." The validation is principally through the drift-scale test, and I can show you a little bit of that again, but also laboratory experiments. Next slide.

And the conceptual model for THC is, of course, this is mostly the period above-boiling, but that affects to some extent what we see later on. You get a boiling zone with CO_2 evolving along with a water vapor, recondensation of that water in the CO_2 above, so you get a CO_2 depleted zone. Of course, the boiling does lead to a zone of active silica precipitation, a certain amount of calcite precipitation, and very small amounts of salts are dumped out, much of it actually in the pores in the matrix, the rock matrix, where it's not readily accessible, but a certain amount does get formed in the fractures.

The key limit, as Bo was discussing earlier, is that you've got a fairly dilute fluid and your boiling front doesn't sit there refluxing. It basically moves back out. So, the amount of salt that you can dump out there is basically whatever is present initially in the water, which is quite a small amount, minus what's being left actually in the rock matrix. So, the concentrations of salts are somewhat limited. But this is a kind of conceptual model, and so what we're going to do is look specifically at the period where the THC now has tracked the wetting front, or we call it the rewetting front, as it comes down to the drift wall where seepage now has a chance of occurring, and see what kind of compositions emerge from that.

For that I'd--next slide--look at some of the validation experiments. You've seen this one before, the drift-scale test, which heated for four years to 200 degrees in the drift environment and now over two years of cool down; a lot of geophysical characterization and hydrologic, but also a certain amount of chemistry I'll be showing. You can see this looks like a pin cushion here. And in the next slide, I'll show you some of the results.

So, this has been used as it's used for thermal hydrologic validation, also for chemical validation, focused mainly here just because the limits of time on the CO_2, but it shows some aqueous chemistry. This, for example, shows a modeling of the CO_2 around the drift-scale test. And we can see actually in the orange-ish colors a depletion zone I mentioned earlier. And then when you get out to this region which would--blue here--correspond to the condensation zone is an enrichment in the CO_2. So, you get this kind of trend developed.

This is captured quite well in the modeling shown in blue here versus actual data points. This is CO_2 versus test duration. So, you can see a good--do a good job of capturing the evolution of these acid gases as they move out from the boiling front. And this is partly why we were saying in the earlier talk that of all the aspects, I think--Eric could correct me if he disagrees with it--but I think of all the aspects of the chemical portion of the DST that's been validated, probably the gas chemistry has been done the best. So, when we make comments, I think, about what acid gases are going to do, it's based at least on some modeling and some reality as captured in this test.

In addition, though, we do have samples of waters here showing calcium starting at about 100 ppm, and this is primarily the results of dilution in this condensation zone here. This was touched on a little bit by Bo as well. So, we are getting dilution in addition to some reactions going on. Next slide.

We also have validation in the form of laboratory experiments. Here is a two-part laboratory experiment starting with distilled water with a fixed PCO_2 in the column, pumped through a tough column of pressed-tuff to measure effluent chemistry and try and match that effluent chemistry and make sure you know what you're doing with that part of the stage. And then the second part is, basically, take what's come out of that column, put it in a reservoir, and pump it through this fracture system with a temperature gradient from 80 to 130 degrees such that you're going to get a sub-boiling region, a boiling region, and a super-heated region. So, that's this fluid coming through there. This is the kind of results you actually see. Here, a slab showing from 80 to 120 degrees. You can see right about 100 degrees the appearance of this silica precipitation front as the water gets boiled away and you re-precipitate these. Amorphisilica is the major phase there. Next slide.

And that's captured pretty well in the modeling here. The effluent chemistry is captured in the modeling quite well. And, in addition, the modeling captures in general this propagation of the fracture ceiling mechanism, this dumping out of this silica from solution. Next slide.

And so really all this validation was to provide some basis for believing what I'm going to show now, which is the THC model predictions, which then provide our potential seepage compositions. So, this is what I'm going to present in terms of conceptual understanding. In fact, it's what--the way it's actually done in the TSPA, just to give you an idea what's being done there. And so what you see now--so we're focusing now, basically, in the crown of the drift. Right above there is a function of time and how that chemistry is predicted to look.

You see, for example, focusing on chloride and fractures, that basically we pick up the variation of the natural pore water chemistry reflecting those different water chemistries. This would be that so-called W-0 water that's more chloride-rich. No seepage here, of course, during the boiling phase. That's why it's blank. There would actually, of course, be some evolution, but that's all back up in the rock. And then once boiling stops, we do see the rewetting front appear. We do see a kick in the chloride concentrations, a short-lived kick in the chloride concentration.

This basically represents that rewetting front dissolving those salts that were dumped out in the initial portion of the system when the boiling front receded from the drift. So, this is a quite short-lived front basically because of the limitations of the amount of salt that was actually left behind there. And then, basically, hopefully to answer Thure Cerling's question, we then in this case returned quite quickly to the ambient concentrations. Don't look very much different.

You see a little bit different story with the more reactive constituents: calcium bicarbonate, which of course is important in terms of this calcite divide. Generally we're well below the divide again except for a short-lived peak during rewetting here and then some gradual rebound to the ambient values.

Nitrate chloride in this panel here basically showing very little affect, if at all. And these are basically because these constituents behave largely conservatively in the overall system. In other words, when you get enough water to actually rewet and seep the system, you've already got enough water to dilute the system and dissolve all the halite and dissolve all the nitrate minerals. If you were ever down at the period where you got just the nitrate minerals in solution, the volume of water would be so minuscule you'd never drive flow there.

The CO_2 in the fracture is showing and, of course, evolution that's continuous through time, the depleted zone during boiling, and then the rebound once the rewetting comes back up. So, again, these are feeds to the evaporative in-drift chemical models that we're going to look at now next. Next slide.

In-drift chemical processes. So what processes are important there? Temperature difference probably is the key one between the drift wall and the waste package because it creates a gradient in the RH. Where seepage occurs, basically the drift wall, RH is about 100 percent, but then these drips move to lower RH inside the drift environment driving evaporation and concentration of the seepage water. And how that initial solution composition along with a temperature and RH will determine the actual reaction path that this solution follows once it starts evaporating.

It's important to point out that concentration of fluids not only leads to simple concentration, conservative concentration of constituents, it eventually leads in most cases to mineral precipitation and thereby depletion of solution in components. So it's not very easy, for example, to develop extremely high fluoride concentrations in a system which has a natural water that has calcium simply because you supersaturate with respect to fluoride, and you start pulling that fluoride out of solution. So, both of these processes are going on. Next slide.

And what we're basically building into this in-drift evaporative model, this is just a schematic of it. In fact, it's basically EQ3Q6 calculating all of the system including the activity of water as a function of temperature and relative humidity, that is the activity of water. But built into this is the kind of thing that's shown here, that the reaction path is governed by this kind of chemical divide theory where you start with a dilute water and, for example, a mineral precipitates. These don't all concentrate equally. You pull these constituents out of solution.

In one case, for example, a carbonate less in calcium, you could develop a sodium calcium brine in this, a chloride brine in this way. Or if you've got carbonate in excess of calcium, generally you deplete the system of calcium right away, and you end up with basically a sodium chloride system with mostly carbonate, excess carbonate, in solution. But this kind of process that we call the chemical divide process is captured in those geochemical models, but is done with all the minerals that could form in the system. Next slide.

And so our approach here to evaluating the chemical evolution is really a two-part one. It includes experimental investigations with detail evaporation studies of solutions, literature data on salt solubilities and deliquescence, and also heterogeneous studies of multi-phase salt systems. I think both these bullets all involve new data that the Board hasn't seen. You can correct me if I'm wrong. And combined with that, it's not called new data, but some of it is quite new. We also have new modeling that involves an updated thermochemical model, EQ3Q6, that's adapted from multi-component salt-brine systems. It has a state of the art

Pitzer database to deal with activity corrections at high ionic strength. And we're going to be showing at least some comparisons between the model and the data just to show that we're on the right track, that there's some validation. Next slide.

One of the--so this is the section on experimental studies. I'll just review this, hopefully, briefly. Next slide shows one of the apparatuses. This is basically a series of follow-on experiments to what Rosenberg had all did a few years ago. The key difference, from my understanding, is that there's a much better control on the gas chemistry over the top of the evaporating solution. This was a little bit hazy in the earlier one. So very careful gas and RH control along with the ability to sample the gas chemistry. Next slide.

And so this shows some of the results for a sulfate-type water and a carbonate-type water. You can see the model results are the lines, whereas the data are actually these points here. You can see models of a fairly simple one here, potassium nitrate going up conservatively. There's no precipitation even though you've come up to a concentration factor greater than 1,000. However, in the case of sodium chloride, you can see once we get up to this point that the chloride and the sodium level off. We basically started precipitating sodium chloride there, captured calcium and sulfate, as well.

Magnesium, we never really get up to very high concentrations of magnesium. Basically, millimolar level, there's just tens of millimolar, but we're picking up at least a little bit of a decline, it looks like, in magnesium here. One of these experiments was the one we mentioned that actually they found magnesium--this may have been later in the series--found magnesium sheet silicate developing in solution. Here's some of the modeling results comparing carbonate waters. So these are new data that are meant to corroborate, validate the validation, to corroborate the modeling that's being done here. Next slide.

Here's just a comparison of the model against solubility data from the literature. Literature being basically Chemistry Handbook Solubilities here, 25 and 100 degrees, so at temperature range, but also a very significant concentration range. You can see about five orders of magnitude. The model was able to capture those solubilities. Quite importantly, significantly, at 100 degrees doing quite well at even high concentrations of these salts here, clustered around there. So, the model is able to capture this. Also, I'm focusing on individual salts. We can see we capture both the solubility here and the deliquescence RH of the sodium chloride system, magnesium chloride as well, captured quite well except for that minor discrepancy right there. Next slide.

And the last experimental component I want to mention is just these. I've already presented at least some results on it. And these reverse deliquescence experiments involve reversal in environmental chambers where the RH and the temperature is controlled. Reversal, basically, involves starting with a solid and raising the RH and letting water absorb and then measuring the chemistry. And then the reverse, of course, evaporating, measuring the chemistry in both cases, monitoring where you are in RH space. Some of those results are shown in the next slide.

Two of the results, a sodium-chloride-potassium-nitrate system and sodium-chloride-sodium-nitrate. You can see here, again, the initially solid ones are red, initially dissolved are black. And the Pitzer model, the so-called Yucca Mountain thermal-chemical model, is the lines here. We have some discrepancy here, but we capture the eutectic point quite well in terms of RH and the mole fraction sodium. The potassium you see here would be the deliquescence point for sodium chloride.

Similarly in this system here, deliquescence of sodium chloride here, sodium nitrate down here. Some discrepancy between the eutectic point, but really only a few percent in terms of RH, and a few percent in terms of the actual mole fraction of nitrate. So, these are all reversed experiments that demonstrate we're on the right track in terms of the thermochemical model at quite high concentrations. Next slide.

So now, that was the validation, basically, the chemistry. Some I want to present, some of the key results, in terms of the in-drift chemistry analyses in the next slide. Just to summarize in advance, the main results that we want to pick out here, is that, first of all, calcium-chloride brines are very unlikely to form during the below-boiling period. And this, basically, because of dilution and mineral reaction associated with THC processes, but also then further reduction of the calcium concentration by evaporation. I'll come back to more discussion of that.

Evaporation, the second point, is that evaporation of seepage water leads to very large decreases in fluid mass. This is, again, one of those points in terms of perspective talking about fluxes that you have to keep in mind. We're not talking about big beakers of water or anything like that or oceans floating up in the drift. We're talking about quite small amounts of brine that develop, but I'll try and quantify that.

And then finally, I want to look briefly at a time-temperature-RH history for the drift and couple that to the evaporative chemical evolution. This is just one example. In fact, this is kind of a snapshot of what's actually done in the TSPA. Next slide.

And so this key point: The calcium chloride seepage brines are unlikely to form during the below-boiling period. In fact, our analysis shows that you can form calcium chloride to some extent in the above-boiling period, but basically up in the rock and also to some extent in the invert where you basically do something more like direct evaporation pore water. But I think a key reason that these brines do not form in our analyses and I think in a reality, as well, is that we need to take into account these THC processes which are acting to reduce the calcium in the potential seepage fluid basically due to a combination of dilution and mineral precipitation.

Minerals precipitating in that stage are mostly calcite in fractures, but also a certain amount of stellerite, zeolite in fractures. Both of those minerals are actually observed at Yucca Mountain. But some of that is captured, I think, the dilution especially is captured, again, in showing this Borehole 59 result which shows that you're reducing calcium concentration to these dilution effects that I think Bo was touching on previously. That, of course, becomes the potential seepage water.

We get further decrease in calcium and solution due to mineral precipitation. In the drift, especially, the key players that we see are calcite and fluorite evaporation.

Basically, it drops the calcium out to nothing. So, from our analyses is that these seepage brines, despite the appearance of some of these chemical divide plots, is that they actually don't form. It's a little bit misleading. And I think it has to do with the fact that those generally consider direct evaporation of pore water, whereas the THC is important in modifying some of these concentrations. Next slide.

And the second point I want to make is basically that evaporation vastly reduces the fluid mass. Perhaps that's self-evident, but I think it's still useful to get some perspective here. If you focus now back up in the drift wall, we've got potential seepage water, quite dilute, millimolar level. It's at a 100 percent RH. If this just came in, there'd be no problem; it's benign, it's dilute, but obviously it's going to concentrate. But you need to keep in mind what happens to those potential seepage fluxes. For example, here during the monsoon and glacial transition period, the seepage flux would actually be 1400 and 3100 times respectively the width of this bar graph, which then shows flux and kilograms per year per waste package. So, it's somewhere in the 10 to 30 kilogram per year that might actually be sitting up above you.

If we were to focus, for example, now on this period, the potential seepage flux, it's still a low probability because of capillary barrier. We're talking basically about one to two kilograms of water in the rock that might seep on us. That's something on the order of this volume of water that's sitting up there in the drift that has a potential to seep over one waste package over the span of a year. However, if you start looking at the volume of evaporated brine that can develop, you need basically a medicine dropper to represent the amount that could contact that waste package over the space of a year. And that's basically what's shown here.

Once we get down to this level of evaporation, in terms of the RH, which is going to be the conditions in the drift, that's what's driving it, we're going to get, at the most, seepage fluxes on the order of tens of milliliters per year develop there. So, we're talking about very small amounts of water that actually have the potential to accumulate there. And I think in the next slide that's reinforced with looking at actually what this evaporation does to the fluid flux through time.

Here the procedure is basically to calculate the evaporative fluid mass reduction using the actual waste package history through time. And what we see then is that the RH really is the dominant control. Of course, there's some temperature. But it's really the RH that determines the extent of evaporation. And so here's a typical waste package through time. What you'd see is that those seepage fluxes, if we--fluids, if we follow them through time, those are shown through--shown here.

This is basically what Bo showed earlier, the mean seepage flux starts quite low at about two kilograms per year. This is just what could seep again. It doesn't mean that we're going to get--all the packages are going to see that. And then we get the monsoonal and then the glacial transition. That translates basically to an evaporated brine flux that's three to four orders of magnitude smaller, somewhere around, as I say, milliliter to tens of milliliters flux rate accumulation potentially on the waste package. One to a hundred grams of brine, basically, per year per waste package.

So, I was trying to make the point is that--well, I'll make that better, I think, in the next slide--is that when you follow out this time-temperature-RH history, and here's where we're going to try and wrap it all together, and look at the RH evolution. Of course, that corresponds to a seepage evolution and also an evaporative brine flux, but very small. So you do get obviously concentration of those seepage fluids here in the seepage compositions. I'd say 650 years, you've got chloride and nitrate in the millimolar level, quite dilute. You could drink this stuff with the nitrate chloride ratio of something like that.

You do get brine compositions then or, basically, much higher chloride at or close to saturation with halite and nitrate somewhat below saturation of any of the nitrate phases. We do get some improvement in the nitrate/chloride ratios due to the fact that a certain amount of chloride actually precipitates out. So this kind of tries to couple the time evolution together. You can see that there's periods where the waste package potentially can see high chloride/nitrate ratios, mostly sodium chloride is going to be--but that's associated with very, very small amounts of fluid actually collected on the waste packages. Next slide.

And so to try and wrap it up, this is that slide we showed earlier. Basically here now we're focusing on this period starting with that yellow triangle and on up to say wherever corrosion of the waste package becomes insignificant, but basically considering this part of the trajectory. So, we are spending most of that time in the region where we're not getting a thermodynamic control on the nitrate/chloride ratios. In other words, those brines, if seepage brines do make it through, are going to basically reflect the ambient nitrate/chloride ratios in the ambient system. And a few packages, like the hotter ones, will still see some thermodynamic control, but most of them will not.

This is kind of the field that we end up in during this below-boiling period. Eventually we get a high enough RH, the sulfate minerals become soluble, and sulfate becomes another potential inhibiting anion. Next slide.

And so in conclusion, although the evolution of the seepage water chemistry is complex, there's no question about that, DOE has developed a scientifically defensible, integrated approach that couples the chemistry to the time-temperature-RH history of the drifts. This approach considers both the natural variability of pore water chemistry, capturing the range observed in the repository horizons, but also modifications to the pore and fracture water chemistry as a result of the THC processes. And then finally links that to the evaporative concentration resulting mineral precipitation where seepage actually occurs. And this approach then is implemented through a combination of experimentation and modeling. Next slide. This should be the last, I believe.

Conclusions is that the seepage will not result in calcium chloride brines during the below-boiling period because of the precipitation of calcium mineral phases both during the THC stage in the rock, in other words, and in the drift. In hotter waste packages during the below-boiling period, thermodynamic controls do result in a favorable nitrate/chloride ratio for those hot packages, but most will not really see much of an improvement in those ratios.

Evaporated brine fluxes are on the order of 1 to 100 milliliters per waste package per year as a maximum, again assuming that the capillary barrier was not operating. And, in fact, that leads me again--we've gone down this long road, I want to make this comment one last time. In the expected case where very little seepage on waste packages occurs due to the effectiveness of drip shields and the capillary barrier, the foregoing discussion on seepage chemistry, is irrelevant. And this is, I think, where the drip shield comes back in and potentially is important in shielding the waste package from some of these solutions.

Thank you.

LATANISION: Carl, thank you very much. You're right on time. We have about a 20-minute period for questions. Let me actually begin.

Now, this is Latanision, Board. Can we turn to number 36? Carl, in your conversation you describe the nitrate-to-chloride ratio as being favorable, and I think in your concluding statement, comment is made as a favorable ratio. But is the issue of it being a favorable ratio based on something other than the tests that were done in magnesium/chloride salts? How do we know it's favorable under these conditions?

STEEFEL: Yeah, I should make a few points. One is, I don't know if that was the right choice of word. It was mainly to say that they, at least in this slide, that the nitrate/chloride ratio had improved a bit relative to what's observed in, say, the seepage water which is going to reflect ambient. So, in certain cases because of the precipitation of halite, you get an improvement in that. My understanding that, overall, the issue of nitrate/chloride ratios has been evaluated in the whole series of experiments involving different corrosion tests, but that's going to be what Joe is going to talk about in the next--the next talk.

LATANISION: Latanision, Board.

Are we on? I'm--you know, I agree. I'm prepared to wait for Joe's comments, but this is an issue in my mind. My recollection is that the data that showed the inhibiting effect of nitrates was based on either magnesium or calcium chloride brines. And I don't know that there's data that shows the--maybe I'm incorrect. Greg looks like he wants to make a comment.

GDOWSKI: Greg Gdowski, Lawrence Livermore.

Joe Payer will actually show some corrosion studies that were done in sodium and potassium containing solutions of chloride and nitrate.

LATANISION: Okay, these must be more recent tests; is that correct?

GDOWSKI: Within the last year, yeah.

LATANISION: Okay. Then I'll reserve my question until we get to that point. Thank you.

Any other questions? Dan Bullen.

CERLING: Cerling, Board.

Could we go to slide 11? What I'm wondering about in this figure here, that one of the key issues in the chemical divide concept has to do with anions down--the anion triangle down there in the lower right. And I was just wondering, in the middle non-lith, the purple zone, there seems to be two sorts of chemistries that seem to fall out of that zone; the red dots and the green dots all seem to have a pretty uniform composition. Could you or somebody in the Project explain to me why there are two, sort of, clusters of the purple dots?

STEEFEL: Probably the best one to answer that would be Eric. I don't know--

SONNENTHAL: Eric Sonnenthal, Lawrence Berkeley Lab.

I can probably answer part of that. Maybe Zell Peterman can add more because they collected the data.

The first set of water HD perm, which was in the upper group collected several years ago, we don't really know if there really are two groups of waters. I don't know if we have enough data to say that the middle non- doesn't span into that other regime. So, I'm not sure if--I mean, obviously, it does look like there are two, but there are also differences in spatial. There's spatial differences, which also result in different depths to the surface and different infiltration rates. We know that the sample collected at HD perm is in the eastern part of the repository, right at the edge, where infiltration rates are lower than in the western part under the--in the main part of the repository.

CERLING: I guess. And then I'd actually like you to comment on your backup slides if you could?

STEEFEL: Okay, which--which one is that?

CERLING: Cerling, Board.

Starting with 41, the--your backup slides has some discussions of flow separation which certainly Catholic University has brought up before.

STEEFEL: We took these out of here. Yeah, the issue here was we figured we wouldn't focus on this in the main talk just because of lack of time, so I put them in the backup, but the issue there is that it might have a good--probably should have a good conceptual diagram--but that in the last stages of evaporation because the nitrate minerals are more soluble than the chloride minerals, if you've actually got flow on some surface developed, in principle, you could then leave behind the chloride-rich minerals as the solution continues to move down whatever surface is involved there, and that you'd basically get a separation of your nitrate and chloride in solution.

Then you would basically leave behind chloride minerals which would now at this low RH, such that--you have to have a low RH or you wouldn't evaporate it to that extent --you'd basically have a non-deliquescent chloride mineral with the nitrate moving on. Perhaps later, as the RH climbs up, you'd then get deliquescence of a more chloride-rich solution there. So this, actually, this process is incorporated into the TSPA, that to some extent it is going to occur, some flow separation will occur.

The major argument, I think, against it is simply, again, you have to keep in mind the fluid volumes here that this separation of nitrate and chloride, and I showed that in one of the validation slides, basically occurs at concentration factors well over 1,000. So, you're talking about fairly small amounts of seepage that come in and then dry out. The question is, do you have enough volume there to maintain actually a drip that can now continue to propagate down a surface, given that the surface presumably has some natural roughness, capillarity, whatever, that's going to tend to keep that moisture there.

So, I'm not sure we can make an ironclad statement. In fact, that statement isn't made. It is incorporated in TSPA, but I think there's a--I don't think it is a really significant effect unless seepage was much greater amounts than is presently observed. Again, because you're cutting those seepage fluxes by a factor of 1,000 to 10,000, you don't have the hydrologic driving force to continue to propagate that drift.

LATANISION: Dan Bullen.

BULLEN: Bullen, Board.

Could we put figure 20 on this slide and figure 37 on that slide? One of the--yeah, perfect.

I had a question basically with respect to timing. As I look at figure 37 over here, I see the sort of light blue region where the nitrate-to-chloride ratio is less than .5 possible as being an area that we'd kind of like to avoid. And I'm trying to get a handle on the time frame in which we're going to be in that regime. And so when I go to figure 20 and I take a look at the time steps for the concentrations that you see for potential seepage compositions, I see that there's sort of an instantaneous, as I go out of the boiling regime, you know, kind of one time-step that I'm away from it, but how long in actuality would I be within the region where I don't want to be, which is the nitrate-to-chloride ratio of less than .5?

STEEFEL: I don't know if--I don't know if this is the best one to compare that. Those represent different waste packages with temperature-RH histories. But, basically, you're asking what period of time you'd be in the nitrate/chloride ratio less than .5?

BULLEN: Exactly. Those--

STEEFEL: You wouldn't necessarily be less than .5. It just means you get back out your ambient pore water compositions. It just means there's no thermodynamic control. So, basically, I think the best one to look at is over there in this slide on this side. Basically, in this case, from about 700, a little bit before--well, the boiling period starts--ends at about 750 years, so you'd be there up to--I've forgotten. Actually, we should look at the other slide. Could we go to slide 4 or 5 over here? That's the one that maps to this scenario most closely. I think it's in 5 or 6. Next one. There we go.

That would correspond to a period, basically, all the way up to where you hit 80 percent RH. This is where the sulfate minerals kick in. It doesn't really modify the nitrate chloride. Otherwise, it would be over the entire period, below-boiling period, until waste packages dropped below 90 or whatever best figure there is for where corrosion is insignificant. So, most of the below-boiling period is going to be spent in this period, in this area, where you're basically not getting thermodynamic control on those ratios except for those hotter waste packages. And, again, this is all bundled into TSPA. In other words, TSPA samples this entire distribution.

BULLEN: Bullen, Board.

So, just to follow up, I guess figure 7 is a better representation, but as I look at that, it's what? Maybe 700 or 800 years?

STEEFEL: Right--

BULLEN: And the cooling time that--

STEEFEL: --I believe so. The 750 to about 1500, yeah.

BULLEN: Okay, thank you.

DIODATO: Diodato, Staff. Diodato, Staff. Am I on? Okay.

Yeah, Slide 35 is of interest to me. This is maybe just a small point, but looking at the mean seepage flux number you have there, I mean, the mean is good to have a handle on, but with 10,000 or 11,000 waste packages, it's also nice to have an idea of the range, the variance on that, about that mean because I suspect that sample data set is dominated by zeros for the most part. So, what you'd really like to know is, you know, what are the maximum and how many waste packages are exposed to these higher seepage fluxes? So, I don't know if you have a handle on that or--

STEEFEL: I'm not sure I'm the best person, but actually my understanding of it was that this is not the actual number of packages that are going to see that seepage. There's a much smaller percentage of packages that can actually see that mean seepage flux. And within any one period, it's somewhere, I think, in this period on the order of, say, five percent. Then it goes up to 15, and I don't think it ever gets to 30. So, that's the overall mean seepage flux that any one package might see if the capillary barrier failed.

So, the number that actually see that is considerably less than that. But you're right. Basically, there's a significant distribution there. We just tried to pull that out, you know, mostly for a sort of almost pedagogical purposes to make this kind of argument. But there is a significant range and, in fact, again, that's the kind of thing that would be sampled in the TSPA.

DIODATO: Thank you.

STEEFEL: But if that was incorrect, I think Bo can come up and correct me. But I'm pretty sure that this doesn't mean that all those waste packages are going to see it. You apply then an additional factor related to the capillary barrier which reduces that by a significant amount of something like 5 to 10 percent.

DIODATO: Well, if you redistribute the mass flux to a smaller number of packages, then that particular flux for a particular package should be larger; right?

STEEFEL: Not necessarily because a lot of it just goes past and sheds off the system. So, that's the flux that it would see if--this is my understanding--the flux that it would see if the capillary barrier didn't work, but otherwise somewhere else. You're not just diverting that and getting a larger mass of water on those that seep. You're getting some of your waters diverting around those packages.

DIODATO: Thanks for your explanation.

LATANISION: Do we have any questions from the audience?

SHETTEL: Don Shettel from the State of Nevada.

Can we have slide--Slide 26? This experiment looks like a closed-system experiment. Did you have a stir in the bottom? Is that correct?

STEEFEL: I believe it is because you've got a condenser, at least. Greg is the one who worked mostly on that.

GDOWSKI: Yes, Greg Gdowski, Livermore.

No, it's actually a flow-through system. There's a constant stream of air flowing over the top of the fluid.

SHETTEL: I'm not referring to the gas system. I'm referring to the liquid system.

GDOWSKI: We've run them both ways. We've run tests where we keep the water-level constant. And also we've run a test where we evaporate down the water. So, both--both cases have been run.

SHETTEL: Yes, but what I'm referring to, there are two ways to model in geochemistry. One is a closed system where you allow all the mineral precipitates to remain in equilibrium with the solution. And the other one is where you remove the mineral system so it's essentially a flow-through system.

GDOWSKI: It's a closed system in that sense, yes.

SHETTEL: Okay, is that the most conservative approach? Or would an open system be a more conservative approach?

GDOWSKI: It depends what you're trying to understand. The point of these--

SHETTEL: Well, this--

GDOWSKI: The point of these experiments was to validate the EQ3/6 model and to understand the initial chemical divide theory as it applies to these waters.

SHETTEL: Yeah, but EQ3/6 can be run both ways.

STEEFEL: Well, the issue, I think, was that it's meant to capture also one case where you basically drip a small amount of water on a waste package and then it sits there. So, there is one end-member case. Whether you call it conservative or not, I don't know, but it certainly realistic where you can actually have a closed system developed on the waste package. As I discussed with the flow-separation, there's certain scenarios where it might run off actually.

GDOWSKI: Exactly.

SHETTEL: Well, I'm not calling it conservative--

STEEFEL: Now, one point to be made is that if you're talking about in the rock, actually that's a full reactive transport calculation. Those salts get left behind when the fluid moves and this and that. So, it's really only an issue on the waste package. And, you're right, this is an experiment that's the closest and the--

SHETTEL: Well, some of the salts could precipitate in the rock and before it hits the waste package. And--

STEEFEL: And that's captured in the model as a transport and flow phenomenon.

SHETTEL: Well, you show on one of these diagrams that the salt totally concentrates in the rock before it hits the waste package, I believe. I'm not sure which slide it is.

STEEFEL: I don't know if I'd use that wording, but I'll reword it. The way I said it is there's a small amount of salt precipitated out of a very dilute water as the boiling front recedes. And so, obviously, as that boiling front collapses back down, rewetting occurs. Water is going to start going into those fractures and seeing that amount of salt that's been left there. And so, yes, there is going to be some re-dissolution of the small amount of salts in the original pore waters. But what we were contesting originally was this business of refluxing.

Go ahead, if that was--is that your question?

SHETTEL: No. My question is, I mean, you can evaporate the water in the rock partially or you can evaporate it on the canister. If you evaporate it on the canister, you can have--you don't need such small volumes to hit the canister. The water--since it's a curved surface, it's not very likely that the water stays in one spot. As it evaporates, it's going to run down the curved surface.

STEEFEL: No, but my--this is--my point was is that this--if--are you talking about chloride/nitrate separation?

The issue of chloride/nitrate separation, if that's what you're talking about, only occurs in the very last stages of evaporation. So, even if your original seepage flux is a good quantity of water like this, this container here, you're really only going to get that separation kicking in when you concentrate it a thousand times. Now, you could have the rest of that scenario, it's true, develop, like leaving calcite behind. You could leave calcite and other things behind, but that--

SHETTEL: Okay, so--

STEEFEL: The chloride/nitrate separation is only in, you know, when your brine fluxes are smaller.

SHETTEL: Are you saying that if you separate the chloride and the nitrate that that is a good thing for corrosion?

STEEFEL: No, I didn't say that. I didn't say that. I said that I--it could potentially--

SHETTEL: You said it could occur, yes.

STEEFEL: Yeah, I said it could occur. And then, in fact, it could be potentially deleterious because the nitrate then moves on, leaves a chloride salt behind initially. That's going to dry out because it--if it was--the reason it's left behind is because it precipitated. But then as the

RH climbs back up, that could then deliquesce, and you could get a more chloride-rich solution. That's deleterious, yes. That's--

SHETTEL: Does that separated nitrate solution have any affect on corrosion?

STEEFEL: Joe may have some experiments on that. I don't think so. Chloride, I think, is the major issue that's of concern there.

This, in fact, this experiment is quite similar to--except for the control of the PCO_2--is actually similar to the Rosenberg experiment I was talking to Greg Gdowski--in fact, that issue we brought up before about tachyhydrite, that evaporation is basically occurring in the space of about a day. So, it's a very rapid evaporation. The question--you know, the problem is that when you're talking about magnesium and how that reacts with silica, that does take longer periods of time, generally more than a day. Silicate rates are known to be slower.

But, again, we're back to that question: What's the relevant time period? I would say it's--the time period is where you have a chance to generate in corrosion. In other words, if within two weeks you can react that magnesium with a silica in that local environment, then that's still good enough. That's pulling the magnesium out of the system, out of solution. I guess I should have--

SHETTEL: Well, that depends where things precipitate and how solutions flow whether you pull the magnesium out or not. If you have a flow-through system, it's less likely. Usually what we see in experiments is the silica precipitates out; therefore, there's not much left to pull in magnesium or pull out magnesium.

With regards to tachyhydrite, I believe you said before that might be an unstable-type phase or something.

STEEFEL: That's what I'm arguing, basically.

SHETTEL: Well, I think the very fact that it has a name and is a mineral means that it does have some stability and is found in nature somewhere. So, I don't think you could say that it's exactly an unstable phase.

STEEFEL: Well, I think you could say it. It is--it's an unstable phase. I mean, the same argument about antarcticite. When you're talking about, you know, people discover new minerals, they want to publish them in the journals, but that doesn't mean they have any widespread thermodynamic stability. Certainly, tachyhydrite is not a stable phase.

SHETTEL: Well, there--well, I would disagree with that. It's found in nature, so therefore it must be--have some stability in a certain environment.

STEEFEL: Plus we find it in our experiments as well.

LATANISION: All right, any other questions from the floor?

(No audible response.)

LATANISION: I just want, before you finish, Carl, I want--there was a question that I wanted to ask this morning that I didn't have an opportunity to ask given the timing. And we do have five more minutes in terms of my schedule here. So, I'd like to return to your figure 18 from this morning.

(Pause.)

LATANISION: Yeah. We had a discussion about this at lunch, and then I--I don't think any of the Board members that I spoke with understood the reason why we were not seeing nitrate, but were seeing ammonia in this x-ray analysis. Now, I understand you did analyze the water chemistry and you found nitrates, but could you just restate or comment again on the appearance of ammonia? The ammonium article.

STEEFEL: Yeah, maybe Zell can give the best answer. I did talk to him about this, as well, and there is ammonia dissolved in the actual leachate that showed up. So, it makes sense that some of it reappeared there. But, as I understood it, there was nitrate in the solution, as well. So what happened to it is not clear.

PETERMAN: Yeah, Zell Peterman, USGS.

That is correct, Carl. As you said earlier, we don't understand why we're not getting nitrates here. I suspect the detection limit, you know, is several percent, maybe 5 to 10 percent. If it's less than that, we probably wouldn't see it on this. It's a pretty routine XRD work. We were surprised by the ammonia salts. We hadn't analyzed the leachates for ammonia. When we got these, we went back to the raw samples, leached them again, and did ammonia. And, in fact, they do have ammonia. That sample one, as I recall, has about 60 milligrams per liter ammonia. So, that's kind of interesting. I don't know where it comes from. Maybe it's coming from the underground use of dynamite or something like that.

The others had much smaller amounts by an order of magnitude, a few milligrams per liter. That's about all we know at the present time. This was a pretty simple experiment. The evaporation was sub-boiling evaporation. It was done in a beaker with a heat lamp. We didn't necessarily control the temperature, but it's certainly less than 100. I would guess, you know, 50 to 70 degrees, something like that. So, you know, this is all--all this has all happened in the last two or three weeks. So we've got a ways to go yet.

STEEFEL: Yeah, this is definitely recent data.

LATANISION: Well, let me ask the obvious question. Are you confident that nitrates are present?

PETERMAN: Oh, yes. We--

LATANISION: You're absolutely certain about that?

PETERMAN: Absolutely certain.

LATANISION: I mean, this doesn't--okay.

PETERMAN: 100 percent certain that there are nitrates present at about the level given in Carl's mean value there on that one chart. I forget what it was, 10, 15 milligrams per--or equivalence, milli-equivalence.

LATANISION: Thank you.

PETERMAN: So, we were surprised to see the ammonia salts, but there seems to be ammonia there also.

SONNENTHAL: I can add--

LATANISION: Thank you.

SONNENTHAL: Eric Sonnenthal--

LATANISION: Go ahead, Eric.

SONNENTHAL: --Lawrence Berkeley Lab.

There's some other data we haven't shown yet, and that's on the nitrogen and oxygen isotopes of some leachates from the dust from the tunnel. And they show that some of them are nitrate derived from typical pore waters in the rock. And some are also derived from other sources, possibly in-tunnel things that--and one of them, at least one of them, showed some ammonia-derived nitrate.

LATANISION: Thank you. We're ready to move on and I would like now to call on Professor Joe Payer. He is somewhere in the room, I suspect. He is. It will take us a minute to get the panel readjusted.

(Pause.)

PAYER: All right, thank you, Ron, the Members of the Board.

I have the privilege and opportunity to make this presentation on Corrosion Resistance of Alloy 22. There's some folks over here that are much more involved in the day-to-day testing and a lot of the analyses that's been done here. Up on the panel up here is Dave Shoesmith, who's a professor at Western Ontario? Southwestern Ontario? South--just Western Ontario. He has done a lot of corrosion testing, corrosion modeling, and has also worked closely with the Project on several of these topics we're going to talk about.

Next to him is Pasu Pasupathi. Pasu has about 30 years, almost 30 years, of experience in materials performance and testing and evaluation in nuclear systems, nuclear reactions.

Next to Pasu is Tammy Summers, and Tammy is a manager of the corrosion testing activities at Lawrence Livermore Lab. She's been primarily the--she's one of the principals in the issues of metallurgy and materials aging issues, that type of thing.

Next to Tammy is Raul Rebak. Raul, from Argentina, had some industrial experience down there, came to the states, did a Ph.D. at Ohio State, then worked in industry doing material selection for chemical process plants and other opportunities of that sort, and then joined the Livermore team here in the area of materials testing and evaluations, that type of thing. Raul's been one of the driving forces behind the different methods to measure localized corrosion, comparing and analyzing, and just seeing what all those tests mean.

Greg Gdowski's getting a little bit of a break. He's rejoined the audience out there, but Greg has been very involved in the evolution of environments on waste packages. And then these people are represented by a larger number of folks behind. Next slide.

And this just lists some of that. In the evolution environment area, Greg's been assisted significantly by Susan Carroll and Tom Wolery, who was on the prior panel. In the corrosion testing/analysis area, Tammy and Raul are represented here. Back getting more data, I guess, are Gabriel and--sorry, Gabriel and Lian, and Chris Orme. In the materials testing area, Bechtel SAIC, Pasupathi heads that area up, and he's had significant assistance by Gerry Gordon. Next slide. Well, I guess the next slide is shown on both sides now.

This is an outline of talk we want to spend a little bit of time here talking about, documenting, and reminding you folks of the high corrosion resistance of Alloy 22 in a lot of different environments. The talk about Yucca Mountain, we've heard a lot about it today, but what I'd like to do is put it in perspective of what is it, what are the features and attributes of Yucca Mountain that are significant and important from a corrosion perspective?

We'll talk about the corrosion rate of passive metals, which is a very significant issue. If passive metals remain passive, their corrosion rates are extremely low. The issue then becomes, will they remain passive? And that brings us into talking about some corrosion conditions, corrosion background, and corrosion conditions at key time periods. Carl showed you some of that, the folks from the Center, NRC showed you some of their ways of breaking up the time periods. We'll focus on localized corrosion because that remains the biggest issue, I think, the biggest threat and the biggest issue to determining the life of waste packages. There are going to be the localized corrosion processes.

And we'll finish that up talking about this Period IV that after the cool--after the thermal barrier and while the package is higher than temperatures, the corrosion can still exist. So, that's the road map. I think we'll leave the road map up on that side, and hopefully that will help you follow along where we're at. Next slide.

Alloy 22 is an extremely corrosion resistant material. In any corrosion application, the corrosion that we observe is a combination of the corrosion resistance of the material and the corrosivity of the environment that that material is exposed to. There are environments that will attack any material. So, there's no material we have that's immune to all environmental conditions one could imagine.

The challenge is and the trick is, to win, is to match up a material with proper corrosion resistance in realistic environments. Alloy 22 and titanium for waste packages and drip shields are highly corrosion resistant material in oxidizing environments such as Yucca Mountain.

We've mentioned before, it's been mentioned several times today, the ambient conditions at Yucca Mountain are benign. These are dilute waters. They tend to be mildly alkaline waters. But even the non-alkaline waters in the dilute form are benign and noncorrosive, basically. However, as we've heard from Carl and others, these waters can become highly concentrated when they're exposed to high temperatures and we evaporate, or in the very early stages of deliquescence, and so that's where the focus is.

The issue is to determine a corrosion resistance of Alloy 22 when it's exposed to these realistic environments. Next slide.

Alloy 22 really belongs to a family of corrosion resistance alloys that are of the form nickel-chrome-molybdenum, are the primary alloying constituents. And the nickel-chrome-molybdenum and in Alloy 22, there's some small amounts of tungsten that are added, work in a synergy to provide corrosion protection over a wide range of environments. These alloys depend upon the formation and the tenacity of a very thin oxide film, chromium-oxide type of a film, on the surface for their corrosion resistance. We refer to that as a passive corrosion layer. It forms spontaneously and it's quite tenacious.

There's a lot of large equipment that's been made of Alloy 22. One of its attractions, it's a very tough material. It has significant elongation when you try to deform it. It has significant toughness if it's impacted. It doesn't crack and break like a glass plate would. You can form it. You can weld it. You can fabricate it. All these things are important issues when you're selecting material for an important engineering application. It can be fabricated in several large structures and components. And I'll show you some examples of that in the next slide. Well, no, I won't.

I'll talk to you about some of the corrosion resistance now. These are laboratory tests that are typically done to provide material selection people with a ranking of materials in various aggressive environments. The particular data here is for an environment that's affectionately called "Green Death" because it's extremely aggressive. It's a concentrated sulfuric acid, hydrochloric acid, highly oxidizing by the addition of ferric and chloride ion, and it's a solution that alloy producers will typically run their materials through so they get a relative ranking.

Two things plotted here. The corrosion rate, low is good. And the critical pitting potential, high is good. And what we're showing here is a series of alloys. This is 316-Stainless Steel, Alloy 600, and then down over here are the alloys of the Alloy 22 class of families, and Alloy 22 is this particular one. It's down where the corrosion rates here are not measurable on the scale. We've heard some about Alloy 600 and pressurized water reactors, so just call it out here that there's a significant difference in the corrosion resistance of these two alloys. We're not talking about the same class of resistance at all.

The critical pitting temperature is measured in the laboratory as the temperature below which pitting will not occur. So, if you're below that temperature in this particular environment, pitting corrosion is not an issue. And so high is good here. And Alloy 22, here again, is this alloy. It's at 120 degrees or higher, the solution breaks down at 120 degrees. So, the pitting corrosion potential of Alloy 22 in this environment is greater than 120 degrees.

These are highly acidic, highly oxidizing environments. Next slide.

This is just a couple slides to show that large engineering structures have been made and function well out of Alloy 22. Here's a fellow standing next to a component out of a pulp and paper bleach plant. This was made back in the late '80s of Alloy 22. There's a lot of welds in this structure. There's a lot of forming in this structure. It's been exposed to oxidizing chlorine dioxide, highly oxidizing chloride containing environments, and it's performed very well. The alloy has been used in incinerators and other conditions where you can get condensation and dry, wet and dry, and so forth. Next slide.

We think one of the very relevant pieces of experience, in addition to being a very large structure, this is a flue gas desulfurizer. Flue gas scrubbers are the units that are on fossil-fired power plants. After the coal is burned or the oil is burned, there can be sulfur compounds and nitrogen compounds in the gases. Before those are released to the atmosphere, the power companies, if they're good citizens, run through a scrubber--and most of them are--run through a scrubber and it's a counterflow. The corrosive gases are coming up through the scrubber. Water solutions are coming down. And so you're leaching out or pulling out of the air solution the sulfur containing gases and the nitrogen containing gases.

Coal has a lot of sulfur and chloride in it. The temperatures in this unit are from 80 to 130 Centigrade. Concentrated chlorides and Alloy 22 has provided admirable service in this environment over a large number of years. So, large structures, large high-corrosion resistance, can be obtained with these alloys. Next slide.

Let's revisit Yucca Mountain from a corrosion perspective. Some of these slides are familiar, but I'd just like point out a few things. We're talking about a repository that's under some 300 meters of rock from the surface. Another 300 meters or so down to the water table. The alloys sit up in air at atmospheric pressure. The relative humidity, depending upon the time/temperature, can range from very low values up to saturation. The ambient waters in the mountain are dilute; however, those ambient waters can be modified and become concentrated by the thermal hydrological chemical processes that we've heard about earlier today and yesterday. Next slide.

There's some interesting features here. Compared to almost any other engineering application that I'm aware of, this is one long, slow cycle. There's no start up and shut down. There's no--it's a very slow heat-up, years, and then a very slow cool-down, thousands, tens of hundreds of years, thousands of years.

The waste packages sit up in air. They're supported by pallets. There's no feasible likelihood that they're ever going to be fully immersed in an environment, in a solution of water. That's a big difference. There's no rapid heat--there's low heat fluxes. It is a hot surface, but the heat fluxes are low. There's a slow heating and cooling and relatively modest thermal gradients. You've seen this behavior. This is a typical package plotting temperature and relative humidity and what we're showing is over a time on a log scale. This is 10 years, 100 years, 1,000, 10,000, and so forth out to very long time periods.

When the repository is closed after 50 years, there's a heat-up and then a very long, slow cool-down. On this plot, we're showing both the temperature of the waste package and the temperature of the drift wall. There's a matching drop in relative humidity as a heat-up period, and then it comes back very slowly, as well. There's a limited amount of water moving through the rock, and there's a limited amount of salts and minerals available to the packages. Next slide.

We've talked quite a bit about relevance of corrosion test methods. This is just to reiterate that point. Within the drifts, the waste package drip shield, there are natural convection currents both in this dimension and also along the waste package, so there is air movement in here. The thermogravimetric analysis experiment that's been run at Livermore is run in this sort of matter. It's an open system, air circulation, controlled temperature, controlled relative humidity, and you can follow a process through that, as opposed to a distillation and reflux condenser where the materials are boiled and captured and then can be carried over into another experiment.

A closed system, refluxing, 100 percent relative humidity, we believe this is much more representative and relevant to these conditions. This is interesting to create solutions and some horror pictures of corrosion, but we think it's not very relevant to Yucca Mountain. Next slide.

One of the major challenges in Yucca Mountain is this extremely long time frame. That's what's unprecedented in this particular application, trying to predict behavior of materials over thousands of years and 10,000 years and beyond. And one of the things that's very important though, and hopefully one of the take-away messages for all the folks here today, is that it's important to recognize what the temperature/relative humidity chemistry solutions are at different time periods in that repository. We have a tendency to do a test in a beaker, and if it comes out good or bad, whatever the results are, to mentally adopt that over a 10,000-year period. Well, it's very unlikely that those conditions in that teacup are going to exist over that time period. And so we'll show you some of that.

There's five relevant periods here. Show you the next slide. Well, let me just say that the--this corrosion resistant material, as long as the waste package has no penetrations in it, the radionuclides are completely contained. All right?

A passive metal, the passive corrosion rate of a metal, is a micron per year, a millionth of an inch per year or less than that. Measured corrosion rates are on the order of .01 microns per year. The waste package at Yucca Mountain is 12-quarters thick. We're not talking about a thin-walled structure here. This is a thick plate of material. Two centimeters is 12-quarters thick. At this corrosion rate to go through the thickness of one quarter would take a 160,000 years. At .1 microns per year to go through the thickness of a quarter would be 16,000 years. So, passive metals that remain passive, talking about extremely long lives, is not unrealistic at all. If they'd have made mummies' noses out of Alloy 22, we would find them nice and shiny, okay? But the Egyptians weren't quite that advanced. Next slide.

So, what is the corrosion rate of a passive metal, Alloy 22? Let's show you some laboratory data. Next slide.

A lot of testing done in a lot of large equipment and smaller laboratory equipment doing electric chemical tests, a lot of tests done on many samples, different conditions, over long time periods. The long-term test facility has had thousands of specimens run through it in periods of excess of five years. Next slide.

The general corrosion rate, the passive corrosion rate, has been measured by various techniques; weight loss measurements, electrochemical measurements. The electrochemical measurements are very valuable and useful because you can look at a wide variety of environments. Also, they're useful because they give you results quickly. One of the features is the electrochemical measurements, typically short-term measurements, give you a higher corrosion rate by a factor of 10 or 100 than what a longer term test would be. But they're still quite useful and valuable. Next slide.

Some range of corrosion rates. In a thousand times the dilute waters in the mountain, this particular water had .8 molal chloride, .4 nitrate, at 90 Centigrade, pH 2.8, the short-term corrosion rate was 1.5 microns. After a week, that had dropped down to 0.1, 16,000 years per quarter. In the pH 2 solution, short term, one hour test, 2.1 microns per year. After 30 hours, that dropped down to .2. So, the corrosion rates we measure in these types of environments are extremely low. Next slide.

This is just from the long-term test facility. As point of reference, this is that .01 microns per year.

That's 160,000 years per quarter. And at 60 degrees and 90 degrees in a variety of waters, pH 2.8, pH 10, pH 11 to 13, we see very low corrosion rates after very long times. Next slide.

This just shows some more laboratory data. This is electrochemical data so that the rates are going to be a bit higher than they would be after longer times. Here is in nitrate chloride environments and calcium chloride. And what we find is in a wide range of calcium chloride, .05 ratio of nitrate-to-chloride, .15/.5; the corrosion rate is over the range of 100 to 160. This is some of the new data that you've been told about. The nitrates are, in fact, effective inhibitors. The corrosion rates in general corrosion are very low in this.

This is sodium chloride brines. And, again, the corrosion rates we're measuring here are extremely low, .4 microns per year across here. And it's a function of some of the nitrate-to-chloride ratio. Next slide.

This is some new data to the Board. Again, I believe this is data from foils that were exposed in an autoclave, so we could go to higher temperatures than you would be able to attain at Yucca Mountain with these. They would boil off naturally at Yucca Mountain if you tried to do it. Exposure times of 130 and 157 days and, again, the major point here is the corrosion rates are extremely low. There's no significant general corrosion of Alloy 22 in these saturated brines over the range of 120 to 220 degrees in these tests. Next slide.

So, the conclusions regarding passive corrosion in a wide range of temperatures to 160 and above using pressurized autoclaves to be able to maintain those environments, the corrosion rates are very low. Short-term tests, 2 microns per year and dropping down into this .1 or .01 range, and less even, after longer time periods. Nitrates show some benefit even. The primary benefit we're going to be discussing with you is in localized corrosion, but we find the nitrates are of some assistance in this passive corrosion as well.

All of the different test methods we use, the linear polarization, the coupon studies, the long-term test facility, give the same message. The corrosion rates are very low, and they're very stable over wide ranges of material--of environments. Next slide.

Okay, so the point of that was passive metals remaining passive give very long lives, and Alloy 22 is a passive metal under an awful lot of realistic conditions at the environment. So, why don't we, you know, stop and go home? Why do we spend a couple days here and all the wringing of teeth and gnashing--or wringing of hands, I guess. You wring hands and gnash teeth; right? But the point is this localized corrosion, again. Where does the passive film break down? Where are the boundaries? What environments do you have to go to? The State of Nevada has shown us some environments where, no question, Alloy 22 behaves more like Alka-Seltzer than it does a structural material.

Our argument, our rational is, those are unrealistic and irrelevant environments. You can put Alloy 22 or any other material in some environments that will cause very significant corrosion. The question is will you form those on waste packages, will they persist on waste packages, will they damage the waste packages? The Project has examined localized corrosion of Alloy 22 over a very wide range of aggressive environments, beyond what we believe is the realistic environment, and I'll show you some of that data here now, after we talk about, a little bit about, the corrosion perspective.

We've talked about, and several of the speakers have mentioned, the various factors that protect against corrosion. There's a hierarchy of protection here. The thermal barrier stops dripping onto the waste package. The capillary barrier stops dripping onto the waste packages. The drip shield stops dripping on the waste packages. If the thermal barrier was not functioning, the capillary barrier was not functioning, and in a co-location with that the drip shield was not affected, then a drop of water can hit on the waste package. And now that's what we're going to be studying. But keep in mind here that that can only happen when the drip shield, the capillary barrier, and the seepage occurs co-located. Next slide.

There's no large accumulation of water that's formed in the repository. We've said this before. The fractures go from a low percentage of saturation to a higher percentage. There's a small fraction of the packages even in the absence of any drip shield that would get dripped on. If they get dripped on, the amount of water that's available is very small. This neglects the thermal barrier. During this time period, the thermal barrier would be in place and this is zero. If the thermal barrier wasn't there, with the infiltration rates during the current climate, it would be about two liters per year. Carl explained to you the rational for having to reconcentrate that. It would give about two milliliters per year per waste package.

After the first climate change, we get about 20 milliliters per year of concentrated brine, and then it goes up to 40. Again, just a frame of reference here, 15 milliliters per year is a tablespoon of solution on a waste package. Okay, we're not talking about large volumes of solution at all. Next slide.

There's two relevant time periods. Carl talked about deliquescence in the morning and dripping and seepage in the afternoon. That's why we broke up the talks that way. What happens during deliquescence and condensation? That's when the dust particles on the package, as the temperature drops, the relative humidity comes up, as the temperature rises, relative humidity moves, we can start forming moisture in those dust layers. And we've talked a lot about that. And then the other issue is what happens on dripping and seepage.

The considerations here, and we'll walk you through some of these, are what's the evolution of the environment? The waters on the waste package surface are what's critical here. That's what's going to make or break the corrosion behavior. The evolution of damage, then, underneath that--the evolution of corrosion damage in environments that may cause damage: What's the likelihood? What's the extent, distribution? And, so what? What are the consequences of that corrosion damage? Next slide.

Okay, so you've seen the temperature curves for the waste package and the drift wall. There's a temperature difference between those depending upon the particular package. You've seen the relative humidity. Next slide.

Carl showed you this slide. But yesterday in one of the Center's presentations and the NRC data, they chose to break the periods up. I think they had four periods. But the rational here is pretty similar, and let me tell you how we define the four periods.

The first period is the preclosure, and that goes from the time that waste packages start being put into the mountain to 50 years. So that's the first time period. That lasts 50 years. At 50 years, the repository is closed; the temperature starts to rise. And so we define the second period that goes from a period of on the order of 10 or 15 years. So, that would be from 50 to 65 years. That's the heat-up period.

We then have a thermal barrier in place. The drift wall is greater than the boiling point, and so no seepage will occur and the package is cool. That's the Period III. In this particular scenario, that lasts from like 65 to 750 years. At 750 years, the drift wall is at the boiling point, and so seepage is possible. So, the Period IV here where seepage is possible, dripping on the package could happen, goes from 750. And when the package is at 90 Centigrade, that's what I've defined as the period below which no corrosion of the waste packages will occur.

Why 90? There's some data from the Center that was published that said the critical temperature for crevice corrosion of Alloy 22 was 95°C. Below 95, they did not see localized corrosion. In some more aggressive environments, they saw localized corrosion down to 80. Okay? So, where's the right--where's the right number? I showed you critical pitting temperatures for Alloy 22 that are above 120 Centigrade. And we will argue, we will tell you, that the conditions on the waste package are some place between that crevice critical temperature and the pitting critical temperature.

We do not believe that the critical crevice temperature with laboratory-made type crevices is the--that's an ultra conservative measure of the lower corrosion. We could debate that, okay? But if you pick 90, the things that define these periods are the temperature where the waste package gets to below boiling, 96; when the drift wall is below 96, we can get dripping. In this case, the waste package is 101. There's a five degree difference between the waste package and the drift wall.

The critical temperature here we picked is 90◦C. So, those are three values you need. You need the time when this occurs, and that temperature, and then you need the time/temperature behavior. Next slide.

So what are we--what do we think is going on during these time periods? During this first period, the ventilation is occurring throughout this period. The waste packages are cool. Dust can accumulate on the waste packages and will. That can be dust from the other mining operations and also ingested dust from the insulation. The metal surfaces are dry. There's no corrosion during that first 50 years. Next slide.

During the heat up period, the drift wall has been dried somewhat from the ventilation period. There's a dry zone from the ventilation. Once we get heated up, we've got the thermal barrier in place. The capillary barrier is in place. We've got a highly corrosion-resistant alloy. No dripping or seepage during this time period. The dust that's on the waste package when the walls are sealed defines the amount of dust available to this. There's no new dust sources from external sources. There could be minor amounts of dust generated if the drift--if the drifts collapsed a bit, but that's a small amount of dust material. So, when you close up the waste package, you've basically got a fixed amount of dust on it, and that's what you have. So, no or none, no negligible, corrosion at that point. Next slide.

Period III, thermal barrier is in place. We could get deliquescence. We've gone through the arguments that calcium chloride is a highly unlikely material. If it's there, it's unstable. There's nitrates in the dust. During this period when there can be no dripping on the waste packages, the judgement is no significant corrosion. From 750, in this case, to 1325--excuse me, from the 65 years out to 750 years. Next slide.

In this case now, the drift wall is down to boiling. Dripping and seepage could occur. We still have those other barriers in place, but it's possible that they could impact the waste package. And there's a time period from that 750 years until 1375 years here when the waste package is at 90◦C, and that's what I've chosen as the cutoff for corrosion. If you push that 90◦C out to lower temperatures, you extend this period somewhat in a lower temperature environment. Next slide.

Period V, the waste package surface is now below a temperature at which any significant corrosion will occur. You go back to the passive corrosion rates. Whatever condition the waste package is at that point, you start corroding. And if that was this .1 micron per year, we're talking about many thousands of years to penetrate the thickness of one quarter. Next slide.

Okay. So, summarizing that, in the preclosure period, zero to 50 years, the packages are dry, no corrosion. In the heat up period, no significant corrosion. By no corrosion here, I mean nothing that would be of significance from an engineering standpoint. I don't want to split hairs on do you get a pit in the package or do you get some shallow penetration? There's nothing there that would compromise the waste package in any way. Thermal barrier is in place, no corrosion.

The focus then becomes in this Period IV that requires some more analysis. We're talking about a period of on the order of 600 years for that particular scenario. And then you come out of that period after 1375, in this particular scenario, and there's no corrosion again. So, the focus of the analysis is in this area. Next slide.

LATANISION: Joe, just to ask a question of information here. Are you referring to the EPRI fault tree?

PAYER: No.

LATANISION: What are you referring to?

PAYER: We have our own.

LATANISION: Oh, you do? Okay.

PAYER: And I'll show it to you.

LATANISION: Thank you.

PAYER: We've got two, in fact.

LATANISION: Okay.

PAYER: Next slide.

So, let's talk a little about this localized corrosion process. First, I'm going to talk some about the controlling parameters and what we know about localized corrosion as a process. And then I want to walk you through and talk about some of the experimental measurements. A lot of the experimental data in this is going to be new to the Board from what you've seen in the past. Next slide.

This is just some framework here. When we talk about corrosion, again, this gets back to the idea that corrosion is driven by the environments that could occur and by the susceptibility of the material. If there's no overlap between those corrosive environments and the susceptible material, localized corrosion won't occur, okay? If they overlap, then it's only those environments that overlap with the resistance of the material that are potentially--that could cause localized corrosion. We determine those in laboratory tests, typically. We run accelerated laboratory severe-corrosion tests to determine where these regions are. Once you have a region like that, though, there's additional requirements in order for that to occur.

At the waste package, you've got to get water in contact, of that solution type, with the waste package. It has to stay in contact with the waste package. The corrosion species have to be present to form that electrolyte. The material is susceptible. And those have to persist over a long enough time to cause damage. Next slide.

At a high temperature, the packages are dry. So there is no overlap between there. There's no potential for localized corrosion when the waste packages are dry. When the waste packages--this is lower temperature here, schematically. When the waste packages are below this critical temperature, I picked 90 in the examples I'm showing here, there's no overlap. So, you get below 90, there's no corrosion. You get in the dry area, there's no corrosion.

This is just another way to say that the area of interest for analysis that needs a bit more analysis, more analysis, are these intermediate temperatures from the point where you could have wet packages and it's above the critical temperature. Next slide.

This is just a set of cartoons that say this family of environments could really be broken up into a subset of families, and those sub-families will have different corrosion corrosivities. Ambient waters would be farthest to the right, the least benign--I mean, the most benign. The more aggressive halide environments move it over in this direction. And so as the environments become more aggressive, you move this direction. But by the same token, the metal, the Alloy 22 or the different alloys, can show different classes of behavior. Next slide.

This is just a montage of pictures here. There's a lot known about the chemical, electrochemical, metallurgical processes that go on for localized corrosion. We're not going to go into them in great detail here, but it's a process that's been highly studied and pretty well-known.

This just shows that different metals have different susceptibility to corrosion. In this particular oxidizing-acid environment in laboratory tests, Alloy 825 did not fare very well, high corrosion rate. Alloy 22 and titanium showed no corrosion whatsoever. And you've seen some and you'll see some more of these polarization curves which is a laboratory method that can be used to exam susceptibility and behavior to localized corrosion. And from these curves that, in general, will look like this, there's a couple important features.

The corrosion potential is the natural potential of the metal in that environment. It's polarized up through the passive zone where the film breaks down. This is the pitting or breakdown potential. We then reverse the potential and look for a hysteresis loop. And where it comes back across here is defined as the repassivation potential.

The importance of that is, if the system is operating below this repassivation potential, even if the passive film is broken down by some random event, it will repassivate and reform. The rationale is, if you break the passive film in this region above the repassivation potential, you could see some significant damage before repassivating or you may not see repassivation, at all. And so that's the rationale. We can go in the laboratory and measure these potentials. And that's one of the key features we'll be showing. Next slide.

What controls this passive corrosion corrosivity? The chloride concentration, temperature, the oxidizing potential of the metal surface, the severity of the acidity, and the crevice geometry. How tight is the crevice? What's it made of? Fluoride, temperature, and potential. Higher is bad, more corrosive, more likely to start localized corrosion. Lower pHs, more acidic conditions are bad, and tighter crevices are bad. So, in a nominal sense, we know what controls this process. The question is where are we with this alloy in waste packages? Next slide.

And this is just a cartoon of a metal. In this case, there's a metal-to-metal crevice formed. This is at high magnification where metal squeezes against metal. What we find is, if the environment back in the crevice can become more corrosive, more conducive to corrosion, and there's a lot of factors that affect that. Next slide.

I want to talk a little bit about localized corrosion, the process from initiation, propagation, stifling, and arrest. In order to go, localized corrosion has to start. That's the initiation process. Once it starts, there's a propagation. What's the penetration rate as it starts working it's way through the metal? In many systems we find that the rate is not constant, but, in fact, it slows down with time. And the word, the term, that's used for that is "stifling". The rate gets slower and slower. It stifles. And in some cases, it arrests. Okay? This is both the material and an environment condition. And so it's important for us to look at this localized corrosion. Once it initiates, what's the propagation, stifling, and arrest?

And there's several things we want to look at here. We'll talk a little bit about this Logic/Fault Tree, about getting it going in the first place. And then things that affect the propagation and stifling are things like what's the crevice made of, what's the severity of the crevice, what's this critical crevice chemistry that has to be maintained in order to keep the localized corrosion going, and then what sort of growth rates do we typically see for penetration? That's really the bottom line is what's the damage evolution? Next slide.

A comment on crevice formers. In the laboratory, we formed the tightest crevices that we know how. And the way that's done is with a piece of Teflon, PTFE, the generic name for it, but you take a piece of this polymeric material and you squeeze it tightly against a metal surface. That forms as tight a crevice as we know how. The liquid still gets back in there. If it didn't, we would have no problem.

The liquid is in there. If we try to form that same crevice by squeezing two metals together, because of the surface roughness of the metals, even highly polished metals, we can't form as tight a crevice as we get with these Teflon-type crevices. If you form a rough ceramic, a piece of rock or something, against the surface, you get even a less severe crevice, and that will be reflected in the temperature at which crevice corrosion will occur and also the severity of the environment. And so one of the things that we will argue is that crevices do not exist over the great extent of the waste package surfaces.

There are crevices on every package, but we do not believe dust is an effective crevice former. We're not sure that rock is an effective crevice former. The metal-to-metal crevices that can occur are effective crevices, but they're not as severe as our laboratory tests. So, it's an important point to keep in mind. Next slide.

Critical crevice chemistry, if you get--these are ranked in more corrosion resistant alloys; a ferritic stainless steel, a couple austenitic stainless steels, 625. As the corrosion resistance of the alloy increases, the solution, the acidity and chloride necessary to drive that localized corrosion process, becomes more and more severe. The acidity has to become greater, reflected as lower pH values, and the chloride concentration has to increase.

Alloy 22 is even more corrosion resistant than Alloy 625 here. So, even a more severe solution has to be formed and maintained in that crevice in order to continue localized corrosion. Next slide.

In the growth, what do you we see typically in growth of crevice corrosion? The penetration rate, the rate at which we're going through the thickness of this plate, in the case of the waste package, typically follows a behavior where that penetration rate is timed to an exponential power and a pre-exponential term here. This end value tends to be between .1 and .5. And the important significance of that is shown schematically in the next slide.

The corrosion rate doesn't start and maintain at these very high rates that we can often seen initially. But, in fact, a corrosion rate bends over, and the lower that value of N, the quicker and the lower value at which it starts getting asymptotic here. So, this is just a schematic here where N equals .1, .3, .5, but the main point is we don't maintain these very high initial rates for very long. The corrosion rate decreases. Next slide.

Okay. So, localized corrosion--localized corrosion fault tree here, if we have a corrosion-type of water that will cause corrosion, if that water can form and the metal is susceptible to it, and we measure that in our accelerated severe laboratory tests, if conditions A and B are there, is the corrosion rate positive enough to be above that repassivation potential? If the material is susceptible, the solution can maintain localized corrosion, you still need an oxidizing potential above that repassivation potential or else you won't get localized corrosion. We do it in a laboratory with a black box, with an electrical box, a potentiostat, where we force it up into that region.

If the corrosion potential is positive enough, you'd need a severe enough crevice on the specimen. In the absence of crevice, you would not get localized corrosion. If you've got A, B, C, and D, then you go to--you can initiate localized corrosion. The question is what's the damage evolution? How long will it go? What sort of stifling and arrest might you exhibit?

So, the questions we have here are, the requirements for damage are, do the conditions exist, will corrosion occur, will it persist, and what damage might be the result? And we want to look at those at the five different periods in the repository to determine under which of these localized corrosion might be an issue. Next slide.

Okay, let's talk about some experimental measurements now. Next slide. This, again, is just a picture of a laboratory at Lawrence Livermore where a lot of the testing is done. The specimens are metal specimens. Here's the crevice formers. These things are bolted together. The tight Teflon-seal against the metal is then put into an electrochemical test. Several different tests are used. This shows data for a typical cyclic polarization which has been used extensively by the Project.

There's a Tsujikawa--there's this guy, THE experiment. I have to get Raul to say that at every one of our review sessions here. But that's a more severe test where you break down the passive film and then you step the potential back to see where it arrests. Okay? But essentially the kind of ranking and the kind of behavior you get are very similar in the two results. The THE test is somewhat more severe. Next slide.

Carl mentioned to you the different categories of water, and what I'd like to do now is go back and we'll talk about the localized corrosion behavior in these different types of water. The source of the different waters are the chemical divide theory and other processes that Carl summarized for you. The ambient waters are dilute, multi-species. They're near neutral, but as you go through those dilute waters and they become more concentrated, you can go into carbonate brines, sulfate, and so forth. And so the question here is what sort of corrosion behavior do we observe in that? Next slide.

So, let's talk about the dilute waters and the carbonate waters. And the picture here is pretty boring from a corrosion standpoint. These are not corrosive waters. Alloy 22 is benign in those environments. If you were selecting a material for an industrial application and these were the waters, you would never select Alloy 22. You don't need that high a corrosion resistance. If we were assured that the waters in Yucca Mountain were always in this environment, we wouldn't be talking about Alloy 22 packages. So, these environments are noncorrosive, the dilute waters and the carbonate waters, no localized corrosion. And this just shows one of the polarization curves. You go up. There's no hysteresis. Not very exciting. Next slide. We don't want to be exciting, particularly, but that's the deal.

Okay. What happens, we're going to talk about the family of waters that are--this family of waters, the sulfate brines and the sodium-potassium-chloride-nitrate brines. Major distinction here is can some calcium magnesium be present or so forth? But these are the waters we're talking about. In these waters, the summary is that we find that the waters with modest amounts of nitrate, we see no localized corrosion. Not observed. It's not observed on welded specimens in the long-term test facility and it's also not predicted by our rapid electrochemical forward and reverse tests.

Waters that have low nitrate levels can have the possibility of initiating crevice corrosion when they're polarized into this susceptible zone. One of the things I'll show you on some of the specimens when we do that, when we initiate crevice corrosion artificially by polarizing into that region, that the initial high current penetration rate drops off markedly so we do see stifling under some conditions. Next slide.

LATANISION: Joe, just to get another point of information, are you referring to experiments in which you've got a artificial crevice present or--

PAYER: These are laboratory, the cyclic polarization with the crevice formers on the specimens, the standard tests. And some of these have been backed up with the THE method. So, there's been a collaboration between them. Next slide.

Just an issue on some of the solution chemistry, again. You've seen a lot of this already. Just the straight time/relative humidity, there's inaccessible regions here. So, this whole region can be taken out as possible temperature conditions at Yucca Mountain. Solutions won't exist up there. It's above-boiling. We talked about the eutectics here and that has a big affect on the nitrate chloride. We've talked about the deliquescent points of the various salts, and what that led to is this diagram that Carl showed you that said, okay, for sodium-potassium-nitrate-chloride-type brines, any condition below this line, any temperature/relative humidity are going to be dry salts, no corrosion.

Any condition out here is going to be inaccessible from temperature/relative humidity. The waste packages will be dry. And so the question is, and if we're in this region, the nitrate-to-chloride ratio will be greater than .5 because of this type of behavior. Up here, we can get into the lower nitrate/chloride type brines, and then sulfates come in here.

So there's a rationale for where we exist on time/ temperature--next slide--for these solution chemistries.

Let's talk about the nitrate inhibition of--these are samples that contain welds. The specimen was a plate specimen that had a section of weld through it. And then the crevice formers were formed so that some of the crevice feet were over weld material and some of them were on base plate. So, it was a heterogeneous specimen from that standpoint.

And what comes out of this, this is an eyeball test, but you can see the data in your handouts. The message that comes out of this, and this is data at 80 degrees and 100 degrees, if the nitrate-to-chloride ratio is greater than .5 at 100 Centigrade, we saw no localized corrosion. If the nitrate-to-chloride ratio was greater than .15 at 80◦C, we saw no localized corrosion. Okay? So, that puts a boundary on the nitrate-to-chloride ratio. You need less nitrate at lower temperatures, it's less severe. Next slide.

This just shows that, in a cartoon here, this is the repassivation potentials versus the nitrate-to-chloride at 100 Centigrade. When we get up to this .5 ratio, we get a very positive repassivation potential. That's good. At lower nitrates, it's down around where the corrosion potential could be, and we could have conditions that would promote localized corrosion. At 80 Centigrade, at the .15 value, we've already transposed--moved into this high repassivation potential. The data at 1 molar, 3.5 molar, 6 molar chloride are all basically the same. The chloride in these tests didn't have a significant affect on the behavior.

Long-term corrosion potentials start reaching the Ecrit in these low nitrate-to-chloride. Sulfate can be beneficial under some circumstances. Next slide.

This compares--a bit of a busy slide, but the couple points here--as you go up in chloride concentration, the repassivation potential comes down. And so from a--that's not a good thing. Okay? The more it comes down, the more--the easier it is to get up into that region. So, again, it takes high chloride to do that.

The other point here though is these triangles. These are data from the Center, NRC's laboratory folks in San Antonio. And you'll see that they're consistently higher, their repassivation potentials, than what's been measured by the Livermore folks at DOE labs by both these methods; the cyclic polarization and this Tsujikawa method. Tsujikawa is a bit more severe, but they're about the same. And one of the ways of rationalizing this is the test procedure that Livermore uses forms a tighter, more severe crevice than what's formed by the Center. And so we think there's a logical rational reason for seeing this offset here. It's a bit of a secondary issue, but it does get back to this point that it's important about what makes the crevice. Next slide.

Okay, so now we're going to talk about these waters here, and we're talking now about the high nitrate chloride brines, the more concentrated brines. They're restricted into these types of environments. And what we find is that when there's high nitrates, we see no localized corrosion. This 115 Centigrade, this test here where we do the polarization curve, and we see no evidence of susceptibility to localized corrosion, that's at 115 C in a concentrated fluoride/nitrate environment. That's as concentrated as you can get and still have moisture, okay? So that represents the very first sort of deliquescence in those environments or it represents the very last dry-out drop. And the point is, if the nitrate levels are up, we don't see corrosion, localized corrosion susceptibility. Next slide.

This is a case where we do, in fact, see a hysteresis loop. This is a concentrated chloride nitrate at 100 C. In this case, the corrosion potential is well below the repassivation potential. In a long-term static corrosion test, we would not expect localized corrosion because the corrosion potential is below the repassivation. If we, with a laboratory instrument, polarize up to 100 millivolts to force localized corrosion to start, we measure the current as a function of time and currents going up is not a good thing. Currents going up means the alloy is corroding. And so what we find is when we polarize up into this region with our potentiostats, the current increases, but then with time it drops off significantly in the presence of nitrate. And so in this particular set of environments, nitrate is not only effective at higher levels of preventing localized corrosion from starting, but it's showing a stifling or an inhibiting effect.

This process of stifling and initiation is really an area of active research and active needs and getting better descriptions of just when that occurs. But in this case, certainly in these types of environments, we see evidence in this example of stifling occurring. Next slide.

Gets us to the calcium-magnesium-chloride-nitrate brines. We spent a lot of time talking about that. Ron wonders why, now. We didn't know why when we started into those tests and all, and so it was important to establish the behavior. We've beat on this all day long, so you know the message here. You can choose acceptance of it, but the chemistry is pretty clear. Calcium-magnesium-chloride-nitrate brines are highly unlikely in the repository. If you don't believe that or if you just want to know what would happen if they did exist, these experiments were done. Next slide.

In concentrated calcium chloride brines, the nitrate-to-chloride ratio, again, is effective at high temperatures. This is 160 degree data. These are the highly concentrated, first deliquescent-type brines, calcium chloride, if it were to exist. And at high nitrate values, it's still effective at stopping localized corrosion. At low nitrate, you could have some corrosion occurring if those environments could persist. Next slide.

And this just--this is passive corrosion rates here, but I just want to go back and remind you that nitrate is very effective at temperature ranges between 100 and 160: low, passive corrosion rates. Next slide.

Okay. The focus is on this Period IV that we're calling it. When the waste package--when the drift wall is below the boiling point, it's possible to get dripping seepage from the drift wall if the waste--if the drip shield is not there, capillary barrier is not there, if it hit a waste package, what would happen? Several considerations during that period. The first is, is the capillary barrier or drip shield, are they effective? If they're not effective, then over what area will seepage and distribution occur? What's the composition of waters during that time period, both the composition and concentration of those waters? How does Alloy 22 behave in that? And we go through this, what we'll call, the local corrosion fault tree, and then come down into talking about damage evolution here. Next slide.

We've talked a lot about this medium temperature package. And this represents a realization of a temperature/time curve, the waste package temperature. Here's the matching drift wall temperature. And here's the relative humidity. This is the hotter package, and this is the colder package. For this mid-waste package--medium, moderate waste package, it enters this Period IV after 750 years and the waste package surface temperature is 101 Centigrade. Relative humidity is 65 percent. It leaves that period into a range of no corrosion, defined here by 90 C in 1375. So it's in this region for about 600 years.

The hot package enters that region at 1900. The waste package is at 99 degrees Centigrade, relative humidity 56, and it leaves that period in 3,000 years. So, from the period of 1900 to 3,000 or in this range temperature of waste package continues to decrease over that time period until it exits at 90.

For the cool package, we get into that region at 112 years. The waste package surface is 102 degrees, relative humidity 72, and you would come out of that in 175. So, depending upon the particular waste package that you select and its thermal load and history, you can follow these trajectories. Next slide.

We can take that information, and that's what Carl did, to take that time/temperature history during this critical period. We know the temperature at 750 years, we know the relative humidity, and that picks a point over here. So, at 750, this waste package is at 101 degrees, and the relative humidity is 65. That's that point on this diagram. So, you see what we're doing is we're matching the chemistry constraints to the behavior of the waste package, and then we can follow that trajectory of possible waters throughout that time period. And then we can relate that back to our expected corrosion behavior. So this particular package at 750 starts at this point. And then it continues to cool, moving this direction in temperature. And the relative humidity continues to rise, so it follows this trajectory. And this is the point when it's at 90 Centigrade, that's at 1375. Okay?

So, these are the trajectory of temperature and relative humidity. You can't just pick any temperature, any relative humidity you want. It's fixed. Now the question is what will the salts do, what will the environment do under those conditions? And the damage that can occur is damage over that 600 year period. You don't have 10,000 years for damage. You've got this finite amount of time. Six hundred years is a long time, okay? But it gets much better closer to what corrosion engineers are sort of dealing with all the time. Next slide.

So, here's our crevice corrosion decision tree. I didn't talk to Mick before the conference. I didn't know he was going to make a pitch for decision trees, but we decided it would be useful also. This means no localized corrosion. If there's a thermal barrier in place, no localized corrosion. We've talked to you already about the noncorrosive nature of the dust particles on the surface in the absence of dripping. You don't get enough water, you've got high nitrates, you're not going to form calcium chlorides. Corrosion under those deliquescent conditions and the absence of dripping are not an issue. They're an issue, but they're not a corrosion problem, okay?

So, it's the dripping conditions that we need to consider. If the thermal barrier is working, no corrosion. If the thermal barrier doesn't work or we're beyond that period, the capillary barrier, if it's effective, no corrosion. If the capillary barrier and the thermal barrier break down, and in that same area where seepage occurs, the drip shield is not effective--or is effective, no corrosion. If you defeat or have simultaneous co-located breakdown of those three barriers, you could have seepage onto the waste package. If the seepage on the waste package is a dilute water, a carbonate water, a moderate nitrate water of the sodium/potassium mixtures, they won't support localized corrosion.

So, the only way you can get through this whole line is for breakdown, breakdown, breakdown, seepage occurring at that location and forming a low nitrate/sulfate brine, and then you can have, you can initiate crevice corrosion. Will you? Is the corrosion potential positive enough? Are the crevices severe enough? If they aren't, no corrosion. If they are, you look at evaluation of initiation, propagation, stifling, and arrest, and you determine the evolution of damage. That's the fault tree that we believe makes sense to go through. Next slide.

So, in this critical period, this time period where dripping could occur, seepage is possible, and you're at a high enough temperature that Alloy 22 still is susceptible to corrosion, you follow the corrosion logic fault tree, you use the temperature and relative humidity, the package, to follow trajectory of the types of environments. There are thermodynamic solution constraints on the aqueous phase. The amounts of salt and water and mass balances--you just can't make up and hope you've got as much chloride as you want or need. It has to be an amassed balance, somehow, in the water that's coming into there. The same with the nitrate. The mass balances have to fit. You determine the number of noncorrosive solutions. There are a number of those. There are some solutions that aren't.

When we go through that logic, the conclusion is that massive large amounts of localized corrosion causing penetrations of waste packages is not going to happen. We just can't see that number and those conditions persisting over a wide number of packages, wide amount of damage. Isolated packages may be affected. Okay? We can't totally take that off the table, but we do believe that that's going to be a small number, a small amount. The question then is where and how much? And that's some more analyses, but it's an isolated number. Next slide.

Okay, some conclusions. Next slide. They're sort of matching a bit here. At least I thought they might match. In summary, Alloy 22 is a highly corrosion resistant metal. That's confirmed in laboratory experiments and very harsh environments. It's also verified and validated by industrial experience with large engineering structures that are welded, that are moved around, that are fabricated, put into place, and at multi-species solutions, oxidizing. This sounds like Yucca Mountain, harsh environments.

Yucca Mountain repository conditions, the waste packages sit in air. The ambient solutions are dilute, noncorrosive, but we've got to deal with these concentrating factors. Where will they go? What sort of solutions will they form? Next slide. Okay, we're going to change that one, too, John.

Alloy 22 is a highly corrosion resistant material. And these passive metals, coming back to here, we're talking about a thick structure. It's a plate, 12-quarters thick. That's a lot of metal. A quarter of that at passive corrosion rates will last 16,000 years at .1 microns per year. So, if it's passive and it remains passive, we win hands down, no question, from a corrosion standpoint. The question then is what about localized processes? Next slide.

There's a hierarchy of factors that prevent corrosion. There's a thermal barrier, capillary barrier, drip shield, and this highly corrosion resistant material where no drips form on the package. We've got deliquescent corrosion conditions that pertain and the conclusion is no significant corrosion damage will occur under those environments. Limited amount of water, noncorrosive environments, high nitrates, not a problem. Where dripping can occur, then you've got to evaluate the types of waters that would pertain in there, and then you have to apply that to the relevant times periods over which they can occur. You don't have a choice of just saying they'll be there forever, or they'll be whatever. They're going to be driven by the temperature/relative humidity behavior. Next slide.

We have analyzed this in terms of five relevant time periods, and we focused down to determining that the Period IV when the drift wall is below boiling, the package is above the critical temperature, and when the capillary barrier/drip shield are not operative in that area, then we have to analyze the behavior in that. And when we go through that analysis in our logic/fault tree and so forth, we conclude that there will be a minimum amount of corrosion. Next slide.

No significant corrosion outside of Period IV. Environments during Period IV will show a range of corrosivity. There is a subset of those environments that could support localized corrosion if there were crevices and other conditions met. Alloy 22 is highly corrosion resistant in many of those waters. The localized corrosion is restricted in that time/temperature range. There are several factors that, in fact, could impede the corrosion rate once it's started. So, our overall conclusion is large areas, no significant corrosion, isolated packages may be excepted.

Always/everywhere we believe is unrealistic and just not supportable. Never/anywhere is closer to being what we believe is the case, but we can't say that absolutely. And so, sometimes in isolated spots, we may, in fact, get that.

Okay, thank you very much. I appreciate your time.

LATANISION: Thank you, Joe. We began by saying we wanted to have a very full and objective hearing of the issues that were of concern to the Board, and you've certainly given us a very full package here. In fact, so full that I'm sure it's going to take some time for us to digest all of the information that's present. But I do have a couple of questions, and I think I'll exercise my prerogative as Chair to begin. I see these other hands over here, but I--

UNIDENTIFIED SPEAKER: No, you don't.

LATANISION: No, I don't. I do want to compliment the Project for really very fully responding to the concerns we raised. You folks have obviously done a lot of work over the last six or nine months, or whatever. And I do want to compliment you and the management of the Project for following up.

Yeah, right. I mean, there is always a "but", but this slide is useful in the sense that--maybe it was the previous one, actually. I wanted to focus on the Period IV segment of the histories. Was it a previous conclusion?

PAYER: It was this, maybe. John will find it. John's a magician.

LATANISION: Yeah.

PAYER: Will this one work, Ron, or do you want the--

LATANISION: Well, actually it'll work. And it's in the following sentence. You're basing a lot of your comments on a critical temperature for localized corrosion of 90 degrees.

PAYER: Yeah, in this example, that's what we're using. And I gave you a case for why that is.

LATANISION: Right.

PAYER: But that's--that's right now more Payer's--

LATANISION: No, that--

PAYER: --example case. But, you know, I--there is a rationale for that. The Project has not selected an absolute value.

LATANISION: Okay.

PAYER: And so that is a debatable point, but--

LATANISION: And that is the point I want to raise.

PAYER: Okay.

LATANISION: I mean, we saw some data yesterday from the Project--sorry, from CNWRA that indicated that localized corrosion could occur at temperatures as low as 60 degrees.

PAYER: And the consequences of that--

LATANISION: Right.

PAYER: --were it to be true, would be to move this boundary out to 60. It would extend the time period and it would extend this trajectory along that. And so, again, you'd have to then consider do the environments stay in a corrosive environment or not?

LATANISION: Right.

PAYER: And, you know, the stifling, arrest, all those--all those sorts of things.

LATANISION: Right.

PAYER: But that's--you're right.

LATANISION: The question really has to do with the uncertainty in your choice of a number. And I think in a sense you've already answered that. It is at this point still an evolving discussion, as I understand your response.

PAYER: I think so. I don't know that the Project has decided to use that cutoff particularly.

LATANISION: Yeah.

PAYER: Bob can get more into that issue about how they actually work it into TSPA now. But certainly from a corrosion science and behavior standpoint, it's a real concept.

The other, Ron, as you well know, these environments aren't just a material property. Taking a nitrate-to-chloride ratio at 100 Centigrade, the nitrate-to-chloride ratio we measure that you need is .5. At 80 Centigrade, that's dropped down to .15. And so less nitrates will, you know, will still save you at that condition. And so, the issue is where are you in these realistic type environments?

LATANISION: I'll buy that. Let me ask one other question. I'd like to go to Slide 51.

In the cyclic polarization diagram that you show there, the repassivation potential would normally, if it were a true repassivation potential--I want to understand what I'm looking at exactly--when you have achieved the repassivation potential and you drive the potential in a more reducing direction, you--I would have expected to trace out the passive current density, and yet you show what looks like a cathodic current density there.

PAYER: Well, I think you do. You start here at the corrosion--well, the way these tests are run is you--Raul, tell us exactly how to test a run. I mean, but you get a cathodic branch passive current and then so forth, but--

REBAK: This is Raul from Livermore.

And, yeah, they--we start the cyclic polarizations at the potential below the corrosion potential--

LATANISION: Right.

REBAK: --instantaneous corrosion potential. We go until a--you know, you go up until a breakdown happens, and then by ASTM standard G-61, we reverse that to five million per square centimeter, and goes down--

LATANISION: This is in a crevice geometry; right?

REBAK: This is--all are crevice specimens, yes.

PAYER: --this, yeah.

REBAK: We have a specimen with 24 crevice spots on each one.

LATANISION: Okay. Okay.

REBAK: So, and then you reverse and then whenever that --the reverse current, it cuts the upright current, we call that the--the repassivation potential or the crossover potential. And this is the data that the Project is using for repassivation potential.

PAYER: You're probably used to seeing these people stop plotting it right here.

LATANISION: No, I'm just curious--

PAYER: But if you keep doing that, that keeps running over onto the cathodic side.

REBAK: Exactly. Exactly.

LATANISION: I'm really curious about the fact that you're--if you suppress the potential more to a value that is more reducing--

REBAK: Yes.

LATANISION: --you're actually see a higher cathodic current density, and so--

REBAK: Yes.

LATANISION: --therefore you're plotting a cathodic current density.

REBAK: Yeah, exactly.

LATANISION: That suggests there's a pretty markable shift in the open circuit potential.

REBAK: Yes.

LATANISION: Which surprises me. That's a pretty dramatic shift.

REBAK: Yeah, but this is how this technique is done, and, you know, you always have--when you have that crossover is because you are achieving what people call the second corrosion potential--

LATANISION: No, Raul, to be really clear on this, my experience is that if you find that the anodic current density remains typical of the passive current density, you're likely to track out the remainder of the anodic polarization diagram. If you have a very high--a very high rate cathodic process, then you begin to see that. And it--what I'm seeing here is a really dramatic shift in the open circuit potential, far more than I would have imagined. And given the nitrates or oxidizers, maybe I should have imagined.

PAYER: Dave--

LATANISION: But I'm surprised by that.

PAYER: Dave Shoesmith?

SHOESMITH: Dave Shoesmith, University of Western Ontario.

When you take in the polarization tests, when you take the potential so positive--

LATANISION: Yeah.

SHOESMITH: --you're actually probably hitting the transpassive region, which means you are inducing a whole series of defects in the general passive area at the same time that you are initiating the localized site. And on the recovery, most of the passive area, the corrosion potential has gotten much more positive. And, therefore, you see that effect, stop the repassivation potential, but you also see the very defective passive film that you have induced by going very close to transpassivity. I think that's what we--

LATANISION: I hear what you're saying, and I don't want to prolong this. I'm going to need to think a lot more about this because that surprises me.

David?

DUQUETTE: Just point of clarification. Duquette, Board, a point of clarification on that.

It's the nature of the test, having done several thousands of these for the Navy not too long ago. The problem is that the test is done rapidly enough so that you don't really repassivate the inside of the crevice. And so you've--that's your new corrosion potential, which is way up in that area. And so it's because the--you still have the acidic. If you stop the test for awhile, as we sometimes have done, let it come back to some steady state, you will retrace the passive--

LATANISION: Okay. .

DUQUETTE: --passive current again.

LATANISION: All right. I'll take--

PAYER: We're getting a little detail here--

LATANISION: Yeah, but that's--

PAYER: You generalists in the audience, we electrochemists get really excited about all the (inaudible) on these curves. I mean, they all are like, you know, like our children here. So, we have to--

DUQUETTE: Yeah.

LATANISION: Go ahead.

DUQUETTE: Duquette, Board, again.

Could you go to Slide 48? I want to clarify something in corrosion science here. Okay, when you have N equals 0.5, that's a parabolic equation that's usually controlled by diffusion through some kind of a film.

PAYER: That's one of the ways it can be--

DUQUETTE: You--you--

PAYER: --or diffusion out of a crevice.

DUQUETTE: Fine. But you and I both know that if the situation is acid enough, that you can get an N equal to 1, where your reaction-rate controlled rather than diffusion controlled. And so this assumes that you get a diffusion driven process. I don't think you do, inside a crevice that's very acid, if the crevice becomes very acid. In fact, we've got some experimental data that indicates that's not true. What typically happens is somewhat the reverse of that, although for artificial reasons.

During the initiation period, the corrosion rate starts out slow until the solution inside the crevice reaches steady state and--or reaches a kind of steady state. And then it takes off on a linear basis because there's nothing to cause diffusion in that case. It depends on the shape of the crevice and a whole bunch of other things.

PAYER: If it does that--

DUQUETTE: But corrosion rates don't always go down in crevices. In fact, quite often they go up from the time the crevice corrosion initiates, and they do reach a constant value rather than--reach a constant value and then decreasing.

PAYER: Yeah, well, let me just pass it on to Dave Shoesmith because he's done some of--a lot of this analysis, but--

SHOESMITH: Well, Dave is quite correct in--Dave Shoesmith, University of Western Ontario.

He's quite correct that the current will go up when you first initiate. That's generally related to the fact that you're activating a wider area of the crevice, so it spreads. It will, in fact, the current will go up. But if you hit any kind of IR or diffusion control, which you have to hit eventually in a narrow geometry, the current has to turn over the top. And the only way I've ever seen or know of in the literature where you can get above .5 is if you have an intergranular attack in a sensitized material. I just don't see how you propagate it for long periods at a linear rate or any rate beyond an exponent of N equal to .5.

DUQUETTE: We--

SHOESMITH: I don't know of one in the literature other than intergranular attacked--intergranular sensitized sites that do that.

PAYER: Dave, are you talking about crevices being driven with a potentiostat where you're maintaining all the cathodic current you need?

DUQUETTE: No, actually these are crevices driven by a very strong oxidizer, ozone in that particular case.

PAYER: Okay, fair enough. Fair enough. This a cartoon of that crevice. And I guess the point here is that what we're talking about is diffusion in and out of this crevice as a potential reason for why these bend over. We see the same kind of behavior in some pit growth. But there's other processes that can go here. That can. We're not saying universally, but that can affect arrest, stifling, and so forth. And, again, it's going to be a function of the corrosivity of the environment. But we will say that in a wide number of stainless steel passive metals that this type of behavior is seen and reported. We're not saying it's universal.

DUQUETTE: Duquette, Board, again.

Again, I don't want to discuss all the science here, but the fact of the matter is it depends on the time scale and how rapidly the crevice sets itself up. I mean, obviously David Shoesmith is correct. Eventually there's going to be some IR considerations or some diffusion considerations, but they can take very long periods of time, depending on how long it takes for the crevice to reach some kind of steady state.

PAYER: Fair enough.

DUQUETTE: Now, we're talking about samples that are going to be in place for very long times.

PAYER: Fair enough. The--let me also just reiterate that this whole issue of stifling, arrest, initiation, propagation, what are the rates is an area of active research in the Project, and I anticipate it will continue.

DUQUETTE: Can we go to slide 22, please? Now, these corrosion rates aren't very high, but a great deal of issue has been made about the nitrate-to-chloride ratio. And if I take a look at the data at 140, and I understand that those can't exist at Yucca Mountain, but I just want to point out that the data aren't always as neat as they could be because if I look at the nitrate-to-chloride ratio for the bottom one, which is 6.7, I have a much higher corrosion rate at the same temperature and presumably the same environment than I do with a ratio of .05. And so there, that seems to be a reversal of trend. Even though those rates are low, there is some confusion, I think, in that--I wouldn't say confusion, but, at any rate, the nitrate isn't always an inhibitor.

PAYER: Greg Gdowski, is this environment--that's very close to saturation; right? It's 6.7 nitrate-to-chloride total molality of 9.6 at 120--140.

GDOWSKI: No, I think, it's--

LATANISION: Greg Gdowski?

GDOWSKI: Oh, it's Greg Gdowski.

No, I don't think that's quite the--

PAYER: Okay, fair enough. Oh, well.

(Pause.)

DUQUETTE: Okay. I think I'll pass for right now. I want to think about some more of my questions.

LATANISION: Okay. Dan Bullen.

BULLEN: Bullen, Board.

Can we just go to figure 26? Just a quick question here. As you take a look at the seepage water per year per waste package for the time periods where you take a look at, you know, the 2 liter, 20 liter, 40 liter, that assumes no diversion of water from a hotter to a colder waste package or any lateral diversion along the drift?

PAYER: Bo? The question was does this take account for any lateral diversion from a hotter to a colder package or along a drift? Is that--did I catch that, Dan?

BULLEN: That's correct.

BODVARSSON: Bo Bodvarsson, Lawrence Berkeley Lab.

It does not take that into account. This is just directly seepage.

BULLEN: Thank you. Can we go to figure 67?

Just a quick question. As you track these trajectories with respect to time, are these trajectories identified for, say, a mill-annealed type material, or do you have--would you expect a change for cold-worked or welded or fabricated materials, or do you think everything would be almost exactly the same?

PAYER: The trajectory would be exactly the same. Those wouldn't change, I don't think, the local temperature/relative humidity.

BULLEN: Okay. How about performance--

PAYER: The corrosion--the corrosion behavior that you then get out of this would--that's a second step. I mean, this tells you the temperature/relative humidity, and you've got to go to what the starting composition is to see where that change is. So, that's not accounted for in here, but it wouldn't change this trajectory.

BULLEN: Bullen, Board.

Would you expect it to change performance significantly?

PAYER: It may. There are cases if you, you know, if you age Alloy 22 at high temperatures, the Center's done this on a few occasions where they heated to--well, let me--I'll pass the baton to Tammy here. She can tell you more specifically, but if you artificially age them at high temperatures, you can cause deleterious phases. What we've found in lot of these localized corrosion experiments that contained welds is the welds, if anything, were a bit more corrosion resistant then. But, Tammy, you want to reiterate the--the question was when we're following these trajectories, what would we say about performance of welds or mill-annealed or--

SUMMERS: Summers, Lawrence Livermore.

Following those trajectories, the performance that you saw today, for one, all that data was weld data. So, that is the behavior you would expect for welds. For aging, I think, you know, I could pass the baton. We don't--because Raul knows better the corrosion behavior or the response to aging. But we don't predict any aging. When you look at the Center's data, the 8-70 for--five minutes. That--that is a higher temperature phase. That is not--we don't predict to be stable at the lower temperatures.

BULLEN: Bullen, Board.

Maybe I wasn't as explicit as I thought. I guess the question is would you expect the range for which the corrosion would be a potential problem with respect to chloride/nitrate ratio to change? You mentioned the temperature change--

PAYER: Oh, I see. I see where you're going. I guess what we say for the crevice corrosion, the nitrate/chloride ratio, as Tammy suggested and Raul mentioned, those contained welds. I mean, so there was a weld, there was a heat-affected zone there, and they were covered. Some of the crevice feet were on base metal, some were on weld metal, and some were, you know, just across that. So, based on that set of tests, anyway, there wasn't any high susceptibility of those welds.

Now, if you went through other types of thermal treatment or heat-affected-zone-type treatment, you could affect this cutoff temperature. I could imagine a critical temperature decreasing if you increase the susceptibility of the material somehow. We've not seen that, but, you know, it could happen.

BULLEN: Bullen, Board.

That was actually the question I was trying to get at--

PAYER: Okay, so then that--yeah, that would--and what that would do is you would just track this further. You would stay on this same trajectory. You would just go further.

BULLEN: Okay.

PAYER: Because it's defined. That cutoff point there is defined by 90 C.

BULLEN: Okay.

PAYER: So if you said 50 C, you'd follow that trajectory out to here.

BULLEN: Okay. Bullen, Board.

Last question, and this was an obvious one that I have to ask. Is the certainty of waste package performance greater or lesser if you completely avoid Region IV? And I'll point to figure 75, which is your last conclusion, which I really liked, which says "never/anywhere".

PAYER: Okay, repeat your statement again--

BULLEN: Is--is--

PAYER: I want to make sure before I say yes or no, I want to make sure I hear the wording because--

BULLEN: Okay, I did not say uncertainty. I said is the certainty of waste package performance greater or lesser if you avoid area--or Region IV?

PAYER: I think if you could completely--I guess my quick answer to that would be yes. It would be more certain, if you could avoid Area IV because that's the only area that we see where the dripping can affect the chemistry and so forth. You either have the thermal barrier and--

BULLEN: Thank you.

PAYER: Fair enough.

LATANISION: Norm?

CHRISTENSEN: Joe, I want to go back to a point you made that I just could maybe pick up on, and it's the question of what is a--what is a crevice? And, being completely outside this area, I guess the--is the issue that the EPRI folks brought up yesterday, that is the diffusion of oxygen, for example, the limiting factor that determines a crevice? Is that--try to tie those two things together. Is that the issue? And then maybe to ask your opinion of their approach to looking at, particularly, the issue of oxygen diffusion rates.

PAYER: Answer the last part first. I'm a real fan of that approach. I think the whole area of corrosion science can benefit from more of that. And in my mind, the Yucca Mountain Project could really take a leadership role in the community of--I think it's important to Yucca Mountain. I think it's important to the whole community.

You could refer to this a bit. What sets up the localized corrosion is this area within the crevice becomes active, and it stays passive out in here. In a phenomenological standpoint, what happens inside that crevice--first of all, when you start off, the solution in the crevice is exactly the same as out here, but inside--and so you've got little local anodes and cathodes going on. You've got some dissolution of the passive film. You've got some reduction of oxygen in the environment that's going on. That's happening outside the crevice on the surface and inside the crevice. As a feature of that, you use up the oxygen in the crevice. It's consumed. And it can only be replenished then by a transport process, diffusion into the crevice.

The other thing that happens in the crevice is you're putting metal ions into solution, and you get to a point where the metal-ion concentration will react with hydroxyl ions and precipitate hydroxides. That drives the pH in the crevice down. You're taking hydroxyl ions out. The acidity of the crevice becomes greater.

Also, while you're putting metal ions, cations, positive charged ions, into this solution, you have to maintain electrical balance. So, some negative charge, anions, have to come in there and chloride is one of the most mobile of the anions. And so with time, you start off, the crevice chemistry's the same as the bulk chemistry, but with time, the oxygen goes down; the acidity increases; and the chloride level increases; and all that drives you toward this critical corrosive chemistry that you have to maintain.

Now, if this was permeable to oxygen or other species, then you have to work that into the model to determine how that's going to affect this critical crevice chemistry. That's the sort of 20,000 foot level. David, do you want to add anything more to that from a--

So, the geometry of this crevice affects the transport in and out of it. There are electrochemical--I mean, you can see there's a lot of chemistry and metallurgy and electrochemistry that's going on in this process.

SHOESMITH: David Shoesmith, University of Western Ontario.

No, Joe, that was very good. I just wanted to--I had one point, which is an unknown from the corrosion chemist point of view. The black area that Joe shows is really the area that we're interested in. And what you're really interested in is how vertically deep is it going to go, whereas a lot of crevices like to go horizontally deep. Okay, so horizontally deep then commonly leads to the increase in the currents, and it's the metallurgical features of the material which stop it going vertically deep. And we don't yet know what the relationship is for Alloy 22, whether it's going to spread or penetrate.

The suspicion is because it's such a good material, it has to be spreading. And I think Roger Staehle had some evidence to suggest it was because he keeps going through this pit nucleation and inability to grow process, which other people have seen. But that's still an unknown as to how that's going to propagate.

LATANISION: Richard?

PARIZEK: Parizek, Board.

On Page 32, you're talking about dust, and then implied that there'd be only about a small amount of fixed dust once the doors were closed on emplacement drifts. And dust would be related to construction in nearby drifts that are going on, moving waste packages down, you know, into the emplacement location. There's ventilation dust during the time when things are operating there. Then there's drift degradation, debris also, even though there's a stainless steel sheathing that's supposed to be present as louvers. So, I can imagine, there's debris that can still get in the waste packages. I think of the heater experiment where you see rock debris on the floor and on the waste packages.

PAYER: Correct.

PARIZEK: And so the question about all of that is does the Department plan to do something with this sort of debris as crevices to see if, in fact, you know, dust maybe was a bad example, but rock debris maybe that's not a good crevice compared to what you have in mind, but will you do experiments with this to see if this is a non-problem?

PAYER: Let me cover that. Let me comment on the front part of it. The important point, we think, is that when the drifts are sealed, you then have no longer a source of ventilated or construction dust into the packages. Whatever you have there is there. But that's a finite amount then to determine mass balances and all those other things that can go on. I'm told by the rock mechanics folks, and Bo showed some information on drift degradation, how much drift degradation is likely to occur under different events, that a rock falling from the wall and hitting the drip shield or hitting the floor is not going to create very much small particulate. It might break in half and so forth.

The dust creating feature is during the actual fracture event when you've got these rocks slipping aside each other and you've got a corrosion guy explaining rock fracture to you here. So, you know, but--but the point is, you don't generate a lot of fine small particle rock.

The other point of that is that Zell Peterman reminded us that the composition of that rock drift-to-drift and zone-to-zone is very uniform and it's not very corrosive. And so that's sort of good dust and--if you would. So, there's a finite amount of that.

Coming to your last question is will the Project and will DOE pursue this issue of crevice formation by dust and rock and metal-on-metal? Margaret Chu's corrosion advisor certainly will recommend that. And, you know, no, it is in the plans. It's an important issue, we think, you know, that is worthy of being addressed. We don't think it necessarily has to be addressed prior to license application, but it's one of these things that you would certainly like to know more about.

PARIZEK: Parizek, Board.

My good wife is here with me listening to all of this, and she remains interested in this. Meanwhile, the house is idle and we'll get home and it'll be dusty and she'll clean up the house again of this dust that's been there since we left. And so the question is there's also this air circulation within the drifts that stirs up dust, I guess, that's in various locations that gets re-moved, I guess, re-transported around. Is that part of the--

PAYER: My sense of that is that those air circulation currents--and, again, I'm slightly beyond my fringe of comfort here, so somebody can jump in--but the circulation currents we see, the natural convection and the movement along the drifts, I don't believe are going to be transporting very much dust. I don't see, you know, a little cyclone wind tunnel in there type of a thing. Do you have high nitrate dust in your house?

PARIZEK: I think we have--

PAYER: If you do a lot of--if you do a lot of, you know, wood refinishing or something like that, you don't. You've got problems.

PARIZEK: Well, that's--I'll send some dust. You can test it. Parizek, Board, again.

I guess your fault tree was independent of EPRI's because your yeses were good, and their nos were good. And that's opposite so--

PAYER: Yeah, we have to decide on which is yes, good or bad.

PARIZEK: There was also--I liked your analog or analogy with the coins. It gives a real good sense of how thick something is and rates at which things happen. So, in terms of understanding from the public, it's a useful idea. And I would be most interested in knowing what DOE's confirmation testing plan might include based on today's general, you know, all the presentations that everybody's given. There must be lots of bits and pieces of work that needs to be done to add further understanding or confirm the works that's been done to date and be--I'm sure the Board's going to be anxious to learn more about these details as we go along.

PAYER: And then perhaps Bob can comment on that in the wrap-up period.

PARIZEK: Yeah, and--

PAYER: Or somebody. Not me.

PARIZEK: And, Parizek, Board.

One more point. There seems like when you start a crevice, I got the feeling that once you've got one, they sort of self-propagate themselves. They sort of--they eat their way--

PAYER: Well, they can--

PARIZEK: And the question is, at the different time periods when--at 750 years to 1375 years when you really can't have anything bad happening, the point is if you start a crevice and it didn't finish, can it pick up and take off at a later date? So that there are these periods you think

you're home free, but maybe the damn crevice sort of kicks in
again for reasons you didn't expect and it--

PAYER: It could persist beyond that to some extent, but
not forever. And there are also--there are a lot of
processes. The other point that hasn't really been developed
and elaborated on here is when we do the crevice corrosion
tests in a laboratory and we're doing them in immersed
environments and you've got a large amount of electrolyte out
here. And when we do the accelerated tests in the
environment, we do it with a potentiostat. So all of the
dissolution current in here, all of the anodic current from
the metal dissolution has to be balanced by cathodic current
out here. And when we run that test in the laboratory, we
supply as much of that as necessary to keep the crevice
running.

There are significant cases where the amount of
cathodic reaction out here causes stifling of the corrosion
rate. You can only--and now we're talking about thin layers
of dust or deposits on metal surfaces, that in a very
qualitative sense, I cannot imagine, would be a very
effective cathodic surface to support, you know, localized
corrosion over a long rate. That factor and factors like
that have really not been taken into account and backed up
then with some experiments and solid models.

PARIZEK: Parizek, Board.

Just one more point about the idea of the confirmation testing programs. The things that are still not yet known or well-known, and that was the role, for instance, of the nitrates and how does that actually cause protection? I mean, it seems like if it causes protection, that's good enough for government work. On the other hand, maybe why it does is, you know, part of a fundamental understanding that a program would be striving for.

PAYER: Sure.

PARIZEK: And the whole idea of passive films is to how long are they likely to be around? You're talking about a passive film you'd like to have going into the 10,000 year time frame. And so there may be mechanisms that help break it down for reasons that nobody understands for the same reason that nitrate is good. You know, the idea is the fundamentals here. So we're really interested in where to go with some of these fundamental--

PAYER: Well, I'm--

PARIZEK: --studies. And I would hope it's in the confirmation or the research and science and engineering or something here--

PAYER: There's two arenas in which we can imagine making progress in that area. One is some of the activities in performance confirmation. But I think the things you're touching on now fit much more in what we're defining as

Margaret's science and technology program. And that's very much where a lot of that work will be focused is a more fundamental understanding of these types of electrochemical processes and transport processes. That, to my mind, is a clear definition of things that fit in science and technology. I would suggest getting a lot of universities working very hard on this. And a couple national labs, maybe.

PARIZEK: And, Parizek.

One more point. I feel better today than I did yesterday. Yesterday I thought maybe I felt like posing some of the early questions was dumb. But in the long run, this whole process has resulted in a clearer understanding, a lot of detail, a lot of effort on the part of a lot of people. And it seems to me it's adding a lot of credibility to the program on how metals are likely to behave. So, I appreciate the--

PAYER: Well, we're approaching beer time, and let me-- let me compliment and, you know, the Board for your report. I mean, it caused a lot of excitement, let me tell you that. It got the e-mails lining up and a lot of conversations. But I know having talked to some of you on-line and off-line that one of your greatest intents was to try to stimulate and catalyze addressing some of these issues. And, Ron, you're exactly right. A lot of putting this whole thing together, there's--progress has been made of understanding the processes. We're quite pleased with the way it's come together and where the story is now, to be quite honest. I mean, that's why we can stand up here and make these kinds of conclusions. But you were catalyst in doing that. And, you know, so we thank you from that standpoint.

LATANISION: As Chair of the session, however, Richard, I just want to say we're nowhere close to beer time yet.

PAYER: That's right.

LATANISION: We've got a lot to go. Dave, you have a question. And then I want to make sure that we do--

DUQUETTE: Thure does, too.

LATANISION: And Thure. And then I want to make sure we leave a moment or a few minutes for the audience. So, let's start with Dave.

DUQUETTE: Duquette, Board.

It's just a point of clarification. Slide #58, please. That's the data that shows the chloride concentration and so on and so forth.

PAYER: Correct.

DUQUETTE: What environment were the DOE tests run in? Is--I mean, we know that the CWNRA did those in magnesium chloride, I believe.

PAYER: Yes.

DUQUETTE: And what was the environment that was used for these tests as part of--

PAYER: Tammy, Raul, do we--

DUQUETTE: --by the Department?

REBAK: This is Rebak, at Livermore.

Both the Center and our data is for sodium chloride solutions.

DUQUETTE: For sodium chloride?

REBAK: Yes, pure sodium chloride.

PAYER: Straight sodium chloride of this molality. And the temperatures, this is the Ecrit 95 for the Center, and 90 for the, I guess, the--for the Livermore data, is that--

REBAK: Yes. All Livermore data, 90 degrees C.

DUQUETTE: Okay, thank you. It was just a point of clarification.

LATANISION: Thure?

CERLING: Cerling, Board.

I'm glad that 43 is still up because one of the things that it chose is a difference in chemistry, as you pointed out, between the bulk solution and the solution in the crevice. And so what my question has to do with, what happens to the chloride-to-nitrate ratio in the crevice as it propagates and how does that compare to the bulk solution composition?

PAYER: The chloride-to-nitrate ratio--or the nitrate-to-chloride, whichever side you want to do that, but more nitrate is good--helps to define the conditions in which you'll initiate this crevice corrosion. I don't know that we have information on the effect of nitrate chloride once you initiate it in the propagation mode. Do any of you folks know of any data specific or general in this?

REBAK: This is Rebak, Livermore.

Also, we didn't do any data, but there is some Center data that I presented a couple of years ago at the MRS meeting in Boston and where they showed that sometimes they initiate the crevice, and then they add the nitrate, and nitrate actually suppresses the growth of the crevice. That's been reported before.

SHOESMITH: Yeah, Dave Shoesmith, University of Western Ontario.

Just to back that up, although the observations on Alloy 22 might be sparse, there are observations on other materials, oxi-anions when they get inside the crevice can stifle it.

CERLING: But they have to somehow get inside the crevice?

SHOESMITH: Yeah, if you were to look at the transport number for chloride and nitrate, they're not that much different. Chloride as preferential, but only to a small degree. So, if this is an ionic migration process into the crevice, there's no reason why you should discriminate in a major sense between chloride and nitrate. It will, but not to a major sense.

CERLING: And then just a question as very much a non-corrosion kind of person, you made the comment about the critical temperature being 90 degrees or whatever the temperature is. And so my question is if a crevice has already been initiated and it goes through that temperature, will it continue to propagate or does that basically kill the crevice growth?

PAYER: People do experiments where they intentionally start a crevice, as we did there, and then lower the temperature and see where it will arrest. So, there is an arrest point. I'm not sure exactly where it is for these sets of conditions we put here.

CERLING: Thank you.

LATANISION: Let me ask whether there are any questions. Roger Staehle?

STAEHLE: I have a few comments. You can turn off the slides. I think we've all had enough slides. I thought I'd talk with just plain words and say a few things.

Historically, the process of prediction is basically based on several important ideas. One is defining the local environments. Until you define the environment, you can't define the corrosion process. The second is that you need to define the material. What is the structure and properties of the material? And third is you need to define the modes of corrosion and the dependencies of the modes on temperature and stress and so on.

So, in 2001, there was a very nice meeting at the Case Western Reserve that I think Joe hosted and brought together many of the actors in this program. And I pointed out several things there at that time that, first of all, there had been to date no accelerated tests. By 2001, there were no serious, if any, accelerated tests on the materials of construction to the question of the heated surface. And third, there were no serious surface definitions of what's on the surface. And so I was concerned that without those kinds of acknowledgements that you couldn't get very far.

And so the Nevada Program that was set up started to answer some of these questions, and not to say that Livermore didn't also start to work on them, too, but Nevada Program developed an accelerated testing program. They developed a program to look at surface environments. And they developed a program to define the mode of corrosion of C-22. And, so far, we've made a lot of progress. I won't say that we're doing all the things we know how to do, but we are doing some things.

For example, we have developed an accelerated test approach. Not everybody, of course, agrees with it, although it's not too different from what Livermore seems to develop.

We've developed a method for analyzing surface chemistries. We have started to define the rates of corrosion as a function of primary variables. And, again, there's a lot more we know how to do that we haven't done. And so, having identified those major issues and prediction, the Nevada Program has set about to answer some of those questions.

Now, it seems to me that the questions that have to be answered, that is the engineering problem, essentially are the fact that we have a waste package; it's in a rock full of chemicals, and rocks full of chemicals; it's got a heated surface; it has an oxygen environment; and it's got water. And those are the main components of this enclave that we have to deal with. And the issues here have to do with how this is all distributed, but that's the system that we're working in. We have to acknowledge that system.

Now, one of the things we know right now next is that C-22 can perforate. It is not always passive. And it can perforate very, very rapidly in environments that can be reasonably argued to be relevant depending on how you define the surface. Third is that there are multiple paths. There is not one path to an answer here. There are multiple paths. And I don't see the acknowledgement that we're dealing, not with a single path, with multiple paths.

Next, we're dealing with a matter of complexity. This is not a simple set of four domains. It's not a simple set of a passive metal, but it's a complex system. And complex systems need sets of approaches, not single approaches. And I'm concerned that I don't see that kind of an idea. Complex systems need bounding approaches, not just one approach, but boundaries. And I don't see an effort to define those kinds of boundaries with respect to corrosion.

And, finally, in the engineering situation, there's no inspection. In a nuclear plant, you would inspect the nuclear plant once every two years, and you kind of know what's going on. But here, you don't inspect the once every 2 years or 10 years or 100 years or 10,000 years. You just don't inspect it. So, whatever you do in the beginning is what you're going to have to live with, which is a totally different engineering problem than anything I think we've dealt with in the past.

Now, I have some very specific comments on much of what's been said. I have a set of comments and objections to what has been said by many of the speakers that I won't bore you all with, but I don't find as much satisfaction in what's been presented as some of you do, and maybe you don't either. But, for example, I'm concerned about the idea that the passive film is pervasive. It's not pervasive. The passive film is a special case and a 10 to the minus 7, 10 to the minus 8, and per square centimeter, is not the general case.

I'm concerned about the concept of nitrates and sulfates and carbonate inhibitors. Those terms of inhibition are just wrong. They are specific for certain cases, but not for all cases. And in this case, I don't think they're all that applicable in the first place.

Now, I don't think we can also make progress by calling this a corrosion resistant alloy. It's an alloy. It's got certain properties and certain environments. But to call it a corrosion resistant alloy, I just think is a big mistake. I think as an alloy that will work under some circumstances, and only those circumstances. I think this concept of realism and what--let's do something realistic, I think we have to reexamine the concept of realism. And realism is what to somebody fits his predisposition, but not necessarily realism. So, we have a set of realisms here, not all of which are congruent. And I hear a lot of the terms like "I believe" and "I argue", but I don't hear so much facts, but I hear a lot of, it sounds like, a religious revival actually.

Now, I think the way we're going to make progress here is by making a serious effort to define the surface composition. The most important idea here in prediction is to define what is on the surface over time. And I think to say we define what's on the surface over time is not a single answer. It's a set of answers. We need five different environments or ten environments or three environments, but it's not a single environment. We need a set of credible environments that we can identify that we need to investigate. And I'd like to see someone lay out that set rather than saying, "well, this is the answer," or "this is the environment." I'd like to see a set of environments. I think we're--well we've made progress, but--and then I'd like to see us define the modes of corrosion of C-22.

The idea that C-22 is totally a passive material all the time is just ridiculous. There are many ways that C-22 can corrode. We need to develop the dependency of the corrosion of C-22 on primary variables that cover this range including ferric ions, incidently.

I would like to see us accept the idea of complexity. I think this whole thing has gotten into a sort of a quantitative mantra here that is entirely inappropriate for the complexity of the system. And I'd like to see us, in a sense, quantify the concept of complexity. I would like to see us develop an approach of bounding situations rather than getting precise situations. And these complex systems, you cannot deal by having a central value. You've got to have a bounding approach.

Now, I'm a little bit concerned also about some of the language I hear here. First thing I ever did in serious corrosion engineering was look at the cracking in Dresden reactor pipes in 1967, and I observed stress corrosion cracking at room temperature in stainless steel. And the gray heads at the time said, "Roger, don't worry about it because it's a bad heat." Well, the next time it failed was still a bad heat. But after awhile, they--on the way to failing all the major piping and DWRs. And I hear some of that same pattern here that, "well, you just had one set of a certain kind of corrosion here, but it won't happen. It's not general." Well, I've heard that before. I know what that sounds like. And I think we have to be careful we don't get ourselves trapped in catch phrases and in slogans.

And, finally, after we've made the arguments I made yesterday in a half an hour that probably should of been a couple hours, somebody told me, well, the reason I'm making those is because I was being paid. Well, you know, a fox smells his own hole first. And I'm concerned about the intensity of the feeling here about what's going on rather than the objectivity. And I think that we need to think seriously about the objective approach to developing sets of ideas, bounding conditions, and recognizing complexities.

LATANISION: Thank you. If there are no other comments, I guess there are. Okay, let's take them.

McCARTIN: Tim McCartin, NRC.

I would like to respond in part to the comment about inspection and it was mentioned earlier today. But the regulation does require a performance confirmation period.

We would expect that would be at least 50 years. The regulation does specifically require performance confirmation for the waste package. Although there's not an inspection for 10,000 years, I think people would laugh at us if the NRC had a requirement to inspect for 10,000 years. There is a performance confirmation program that is a, I would say, a corollary to the inspection program for an operating nuclear facility. And that, you know, we look on that as 50 years of testing and challenging the assumptions on the waste package.

Thank you.

LATANISION: Yes. Okay.

AHN: Tae Ahn, NRC.

A couple of clarification questions to Joe. In your Slide 67, has--

LATANISION: Pull the microphone down towards you.

AHN: Slide 46, there is a diagram--no, his case, 67. Yeah, the right side curve there is a diagram showing inaccessible region. He defined during his talk, but I still do not understand what the definition of inaccessible region. That's one question.

The other question is, Joe, in your Slide 13, it's basically TGA testing. Could you tell us some idea about the air flow rate you used in the TGA testings?

PAYER: Okay, let me--let me ask Greg to double up on both of those. One was the definition of the inaccessible region. Why is it inaccessible? And then the second one is flow rates in the TGA experiments.

GDOWSKI: Hi, Greg Gdowski, Lawrence Livermore National Lab.

I'll do the TGA one first. The air flow rates were such that we could exchange the complete volume of the system within one minute. So, I think the volume of--I can't actually remember what the volume of the cell is, but the air flow rate was such that it would be completely exchanged within a minute.

By the inaccessible region, that's where the water vapor pressure would have to exceed one atmosphere in order for you to obtain those relative humidities in that region.

PAYER: So water would boil off at that. You couldn't maintain an aqueous environment in an ambient atmosphere under those conditions.

GDOWSKI: The repository, you're above the local ambient pressure there, so you can't. The water pressure cannot exceed that.

LATANISION: All right. Thank you.

I am going to ask Bob to make some comments, but I'm going to--as indicated on the schedule, but I'm going to delay that until we take a break. And I--you hear a sense of relief here on the sideline. But I want to make clear a couple of comments before I do that.

Number one, I'm very sensitive to the fact that yesterday we apparently lost someone who wanted to speak during our public comment period. And we're on the schedule to have public comments at 5:20 and we're going to do that so that no one will be out of sync.

Secondly, I do want to give you an extended break because the idea is for each of the constituencies who are represented in our deliberations, and by that I mean not just the Project, but also the State of Nevada, the NRC, Catholic University--well, that's the State of Nevada, right? And EPRI, of course. Sorry. Thank you, Mark. I want to give you all a chance to collect your thoughts and to--and perhaps in Roger's statement he's responded to this already, but I want to give each of those constituencies an opportunity when we come back to provide some wrap-up comments.

UNIDENTIFIED SPEAKER: Mr. Chairman?

LATANISION: Yes?

UNIDENTIFIED SPEAKER: About five minutes?

LATANISION: About five minutes each, exactly right. No more than five minutes. But I do want to make sure we have an opportunity to hear from everyone in a summary sense. And, Bob, you will lead off given that we're deferring your point in the program right now. So, let's now take a break. It's 5:00 o'clock. We must be back in the room at 5:20, and the first item on business will be public comments.

(Whereupon, a brief recess was taken.)

LATANISION: Let's come to order.

We have three people who have signed up on the public comment register. And so I will invite Judy Treichel to come to the podium?

TREICHEL: Judy Treichel, Nevada Nuclear Waste Task Force.

Thank you very much. After all these many, many years, the Technical Review Board is still the only entity that provides this sort of opportunity for public interest groups or for people who don't have any professional part of the program, particularly not financially. And so as a person who works for an outfit that operates on 120-thousandths of the DOE budget, I totally appreciate this opportunity.

The first thing I would like to say is that there has been reference made to the drift-scale test in a lot of the presentations that were given. And the drift-scale test is just sitting out there. It's about six and a half years old, as far as operation goes. It heated up for four years. It's been cooling down for about two and a half. I get the weekly temperature readings off of the thing, and the temperature's not dropping very fast anymore. It's down below boiling, but it's still pretty warm if you were going to just walk in. And it appears from those sheets that come every week that nobody has walked in.

So, it would seem to me to make sense that the Department of Energy should consult with the State, certainly, and I don't know who else, but before the door opens, it would seem like there should maybe be a plan because this may be the only way or the only opportunity that there is to collect stuff like dust that accumulated and actually got heated up for a period of time. And there's been criticisms of the drift-scale tests.

I've been out to the site countless times and with a wide variety of groups. And when people are real dazzled, they believe that this is a replica of an operating repository. And when they're not so dazzled, it's just sort of something that's there to prove the model. But it's only in one spot, and the spot may not be like any or all of the repository, and so you'd only have one kind of dust, and it might not be worth anything, but I really don't think you should miss the chance.

And the statement was made that there was no seepage or there was no water moving because of that test, and that's not true either because I've been out there and felt the rain that was created there. And there finally was a poster that was called, "Why is Water Dripping on my Head?" So, there definitely was something moving around and it was wet.

In January of 1997, I was asked by the Board to give a presentation, and the thing was called, "The Total System Performance Assessment and Transparency. How can the public know what the TSPA is and what it means?" And I guess that was the first meeting that Dan and Norm were at, so I'm sure you remember every word of this, but I thought it was interesting and I want to disagree with Margaret about the value of TSPA.

At the time that I gave this presentation and the reason it was asked was that there was a war going on because the Department of Energy had suggested that they change the guidelines. And the major change at that time was to take out the disqualifying and qualifying conditions. Those were things that the public really understood. We knew what those were and we could recite them and we knew that those had to be met. It was going to be replaced with something called a "Total System Performance Assessment," that seemed real mushy, and in some ways it still does. And one of the--I'm certainly not going to read this, but there's a couple of things that seem pretty interesting after over seven years.

The public was led to believe both in face-to-face meetings with the Project and through many media reports that Yucca Mountain could and would be disqualified as a repository site if any one of the qualifying conditions could not be met. Never was it mentioned during that time of frequently held public meetings and updates that disqualifiers could be fixed or mitigated or averaged against other factors.

For it to be of any value--this is TSPA--it must be used to understand what is not known rather than as a basis for confidence in predictions of repository performance. The TSPA has to be a tool and not a product. And at that time we worried a lot that uncertainties could be disguised as manageable weaknesses resulting in layers of assumptions that ultimately create an illusion of confidence and accuracy. And one of the very last sentences is that, "At this period of time in January of 1997, a major remaining unknown due to lack of data is, what will be an acceptable thermal load?" And here we are all this time later and we're still arguing about the thermal load.

But there was a lot of problem with TSPA, and it's still being argued back and forth. And as a spokesperson for the public, we all know that when something goes wrong, it's not the total system that fails. It's one specific thing or, as DOE would call it, "an initiating event". And at TMI and at Chernobyl and at the Challenger, and at any of the disasters we can think of, there was just an initiating event. It wasn't the failure of the whole system. So, I think that should be kept in mind.

In also looking at this, I think it's interesting that the problem that I pointed out in that presentation about the thermal load has been taken as a lemon and made into lemonade by the Department because as of today there's now a thermal barrier instead of problems about whether or not you can have a hot repository. I also think that the showing of the C-22, or Alloy 22, as a product in industry is a little strange because I wouldn't think that any of the companies that were using that material that Joe Payer showed in those pictures would be thinking of using it for 10,000 years and probably would figure that it would have to be replaced at some time. And if it lasted for even 100 years, people would be very surprised that it had. So, with anything we're familiar with, there's always a disconnect when you're talking about a repository because it's not like anything else at all.

And, finally, I find it very troubling that the Department has taken what they call a "Prevention Plan," and any time there's something that's a problem, it never really becomes--it certainly doesn't become a showstopper or a disqualifier, but it never even becomes a serious hurdle. It always just has to be put in the category of things that can't happen. So, if there's problems with the saturated zone or unsaturated zone, then you get this can that can't fail for 10,000, 11,0000, 40,000, however many years. And then those things don't matter.

And if there's something wrong with something inside the surface facility, if there's a possibility that something could go wrong because something got dropped, then you make it impossible for it to drop. And in some ways that's very good. But in other ways, it masks the possibility that something is going to go wrong and that a human factor or any kind of thing that can possibly go wrong will, and it had been ruled out.

So, thank you for the opportunity. I have this letter if you want another copy or if your filing system is great, you may still have it.

LATANISION: Thank you, Judy.

Let me next call on Mike Lee. According to his affiliation, Mike is with the ACNW of the National Research Council. Is he in the audience? Yes, sorry. There is another NRC, of course. But I guess Mike has left. All right.

How about Rod McCullen?

MCCULLEN: Thank you. Rod McCullen, Nuclear Energy Institute.

Yesterday, out on the table I picked up this brochure. I think it's the first time I've seen this on what the Nuclear Waste Technical Review Board is all about, and it's a pretty good brochure. And I noticed in here there's a bullet list of three things the Board does in conducting its ongoing review. The Board does the following. The second bullet says takes a "systems" view of repository performance in waste management.

And I was really struggling yesterday with the contrast between this statement and the mission of the Board, and the statement that was asserted in a letter and emphasized in the Board's presentations yesterday telling DOE, "don't use Total Systems Performance Assessment to dismiss our concerns." And, fortunately, I think over the last two days looking at everything that's happened in totality, I started to see how that--how that dilemma gets answered. And I want to try to wrap some of that up because I think it's instructive.

Most particularly important was the presentation by Dr. Chu this morning and some of the discussion that ensued afterwards. First, it was healthy to hear that the Board doesn't, in fact, completely disbelieve in TSPA. I think that was good. Also, I think that for Dr. Chu to explain the fundamentals of the approach that we use, the risk informed approach that's prescribed by regulation and resulted from the recommendations of a lot of science over a lot of years, particularly one fundamental that ties in very well with, I think, everything we heard today is this notion of a risk triplet. The idea of you ask the question what can go wrong, how likely is it, and what are the consequences?

And the thing I've seen in the Board's letter, and the Board's report, and the Board's presentation, and the information by the State of Nevada, and, until today, the stuff that DOE was responding with, addressed only the first element of that risk triplet. That is, that question of what can go wrong? I think Dr. Latanision asked Dr. Chu the question of, "well, if I'm an engineer, I want to avoid things." And I am an engineer, and I think when you come to avoiding things, you have to look at all three elements of that risk triplet.

And I think that what we've seen in these last couple days, particularly with the decision tree analysis by EPRI and by DOE, and it's interesting that they both independently came to very similar conclusions, explains a systems perspective addressing all elements of that. What can happen? At each step of the tree, what can happen? How likely is it? What are the consequences? What can happen next? How likely is that? For the first time, and I agree with Judy and I think this was clear in Dr. Chu's talk, TSPA is just a tool. But for the first time, I think today we saw an explanation of that that is a lot less of a black box in terms of that decision tree analysis today and yesterday.

There's also a second element to taking a system's approach to evaluating repository safety that I think is important, and it keeps coming out every time Dan Bullen gets to ask questions. And that is the notion that he's asked the same question over and over again, the question of why not just stay in Environment IV, why not just stay in the cold environment? And the answer to that is actually very simple. It's really not an answer you can give just in the context of a corrosion meeting though because it touches on something broader. And the answer is you don't design a repository based on a single input parameter.

In fact, what you do is you design a repository, and I think what DOE is showing that they've done very well, design a repository that can mitigate all those things that are most risk significant. You don't look at a single parameter without respect to its risk significance. And I think what we've heard in these presentations is this notion of localized corrosion simply isn't very risk significant.

So, there's this need to look at all the elements of risk over all the elements of the repository, and come out with an integrated solution. I think DOE's ability to do this has been dramatically bolstered by the Board asking these tough questions. I think this is a particularly compelling example of the value of this type of critical review, that because for the first time today we are, and yesterday, are seeing really a very solid explanation of this. I think in the case of localized corrosion, I think the Board's questions have been answered. I think they've been answered thoroughly, completely, authoritatively, and definitively.

I just hope, you know, of course, the Board needs to come to its own conclusions as to whether or not they agree with that statement, but I would just hope that in addressing that, that you remain true to your own advertising.

Thank you.

LATANISION: The conscience of the meeting. Thank you. Thank you very much.

Last call for Mike Lee. I take it that Mike is not in the audience, and so we will now go to the wrap-up statement segment of the program. And, Bob, I will ask you to go first.

ANDREWS: Thank you.

It's a pleasure to be back this afternoon. And my objective in the next 15, 20 minutes is to summarize what you've heard from the Department, from Bo, from Carl, from Joe, and the relevance of that information to addressing the Board's questions that were, you know, appropriately raised in the October and November letters and reports, respectively. And to end that way, summarize the conclusions not only with respect to the Board's questions, but with respect to the broader question of the range of environments, thermal, hydrologic, chemical environments, on the package and the range of degradation modes of the waste package in those environments. You can go on to the next slide, John.

As I said this morning, our goal was to answer the questions the Board raised in that letter and to be forthright with our--the response with new information, to point out where that new information directly addressed the Board's questions. I think they were appropriate questions. They were based on the information available last May. I think as I said this morning, the analyses and model reports upon which this information resides last May were not even in draft form. I don't want to say the information was baking, but how you integrate the information to answer questions that are relevant to the behavior of the package, in particular, in the environments, both thermal and chemical environments that are likely, was probably not fully developed last May. Therefore, the Board's questions, which I think were appropriate questions.

A lot more has been done. I think you appreciate the additional work done not only by Joe and Carl and Bo to synthesize it and present it to you, but the researchers and analysts at the labs, the survey within BSC that provided the fundamental information that allowed Bo and Carl and Joe to present that information to you. And it was under Margaret's direction and forceful leadership that got us, you know, to integrate appropriately. And there were the additional information explicitly developed to explicitly answer some of the questions that the Board raised, and I think you saw some of that, in particular, in Joe's talk. So, if I can go onto the next slide, John, I think Slide 4.

So we focused on the data. We focused on the models. We focused on a range of lines of evidence, multiple lines of evidence, not just using our data, but using data available from others; data available from NRC, data available from the literature, analog information to tell a story, if you will, of all aspects, not just, you know, a particular test to answer a particular question and to use those data and multiple lines of evidence to define the range of environments both during the thermal period and after the thermal period.

You know, we purposely broke Carl's talks, as we said this morning, into two parts because the Board's questions really were in two parts. And I think the chemical environments are in two parts. One part is the salts in the dust and how they deliquesced. Another part is the liquid water and how it evolves when it can contact the drip shield and the waste package. So, it was two separate and distinct discussions.

We discussed a little bit, not at full length today, the uncertainty in some of the models and analyses and data that are used and how we're propagating that uncertainty. I believe we have an opportunity to discuss that with the Board in much greater detail from the full system's conflict component, not just the in-drift environment and corrosion aspects as presented today.

It's important to point out that that system's representation will include that uncertainty, does include that uncertainty and variability, and it also includes the low-probability events and features, which were not the focus of today. We tried to focus on the Board's questions which are kind of likely behavior, likely thermal hydrologic response, likely chemical behavior, and likely degradation characteristics of Alloy 22. All of those effects, though, and uncertainty have to be incorporated in the TSPA for the evaluation of the site per Part 63 requirements.

We purposely did not show you any TSPA results. We did not want to dilute the discussion away from the critical nature of the Board's questions, which were really after the fundamental technical bases underlying the models, underlying the analyses, and underlying the response of the thermal hydrologic system. And we did not want to, if you will, dilute that with a discussion of does it make a difference from a TSPA perspective? But you saw some other people put it into a TSPA perspective. Both EPRI and NRC presented results of, well, what if we're wrong and what are the implications from their models using their analyses, even though they are only insight-producing models for either of those two institutions, to give you a sense of does it make a difference from a TSPA perspective?

Let me go on to the next slide. And I probably can--well, let me stay on this for a little bit. This walks through the individual processes, more or less, as we presented them. And they're all processes that can affect the environments, and all those environments can affect the degradation characteristics of Alloy 22 in the repository. We talked a little bit about drift degradation and, if you will, nominal performance. We did not talk, although Bo had in a backup slide, drift degradation in a destructive event, in a low-probability seismic event.

We did focus a significant amount of attention on the thermal hydrologic processes because, as you saw from Carl's briefings, understanding that temperature/RH representative profile and the distribution of possible temperature and RH profiles does affect the type of chemistry that can evolve whether it be an aqueous chemistry or whether it be a dust chemistry. So, it's important to have a fundamental understanding of the temperature/humidity response and the range of temperature/humidity responses. It is different in different places in the repository. It is different for different rock types. And it is different for different waste package types. And so that range of thermal hydrologic responses are important to consider.

Clearly, that thermal hydrologic response is a function of the design. I think we all acknowledge that how you load the repository, how you ventilate, how long you ventilate, what maximum load, heat load, you put into a particular waste package, how closely you space the packages, all of those variables do affect thermal hydrologic response. Even given those variables, even fixing those variables, which of course the current design has done as presented to the Board in January, gives us a wide range of thermal hydrologic responses that we've presented to you. We showed three representative ones, but then we showed the, if you will, the gray area where all of the other thermal hydrological responses are.

Changing one of those design parameters, if you will, would clearly change the number of packages that would see a particular type of thermal hydrological response. Some might be cooler. Some might be warmer. Today some members of the Board recommended going hotter. I guess it was individual opinions as expressed by the Board. And I think that Dr. Duquette said, individual opinions are allowed by Board members and it's only the formal positions that count. But I thought that was kind of an interesting view of some of the Board members. And as pointed out, Dr. Bullen asked his favorite question of what about colder? So, there's a range of possibilities, but there's a range of responses that are being projected in the thermal hydrologic analyses.

The deliquescence of salt and thermal seepage affect both parts of the initiation of localized corrosion depending on whether you have liquid water present or not. Next slide, John.

I think we talked about aqueous solution chemistry. And then to, finally, the last bullets there that Joe talked about with respect to the initiation of localized corrosion and the general corrosion rate with the passive layer or without a passive layer and the effects of crevice corrosion were all discussed at great length with the additional data that we have under a range of environments, repository relevant environments, to address those. So, in the next few slides, what I'm going to do is just kind of summarize the conclusions that Bo reached, that Carl reached, and that Joe reached. I have generally a representative slide over to the left to talk about what the subject matter is and what Bo talked about, but I want to focus on the bullets that are on the right.

I have to admit to, I took the liberty to a three-points insert slides that were presented yesterday; one from EPRI and two from NRC, one from Roberto Pabalan, and one from Darrell Dunn because of the relevance of their conclusions to the same issue we were talking about today. So when we get to those, I'll just reinsert those three slides from those presenters from yesterday.

Okay. With respect to the thermal hydrological response--and I'm not going to go point by point with respect to the Board said and we said. I just want to say these are what we presented to you. First off, that, yes, the thermal conductivity is important. Uncertainty in the thermal conductivity is important. And a range of thermal conductivities for the lower lith and middle non- and for the upper lith and lower non-, I guess, were incorporated into thermal hydrological responses. They were incorporated in the seepage models, incorporated in the thermal hydrology models.

So, there's a wide range of thermal conductivities that have been evaluated and they've been developed based on the in-situ tests that we performed, and include the effects of variable lithophysic porosity and various scale-type effects in developing a reasonable thermal conductivity for the basis of the models. All of the models with thermal hydrology, the thermal seepage models, particularly the seepage part of the thermal, were based on in-situ direct observations. I think Bo went through great detail of almost test-by-test. There's other tests that are the bases, but I think he presented the representative tests for the bases for those models.

The calculations that we have of temperature and humidity--let me switch to the second on the screen, the other screen, John--is a wide range. That gray band that you see is the full range from low thermal conductivity, low percolation rate, if you will, to high thermal conductivity, high percolation rate, giving the maximum temperature, minimum temperatures, respectively, and everything in between. So, nominal percolation rates and high, medium, and low thermal conductivity. And there's a range of percolation rates across the repository block that are factored into the thermal hydrologic responses. Not one five millimeter per year percolation rate, but that percolation rate, in fact, very spatially across the repository block. And that's factored into that full distribution, as well.

So, we have a family of thermal hydrologic responses. And if you like plotting them as a function of time, then you can plot them as a function of time, if you like, and it's more representative to plotting them as TRH curves that vary with time. Then we showed them both ways, essentially. It actually becomes more relevant from a chemistry perspective to plot them as we did on the right-hand side rather than to look at them as a function of time, but understand that that temperature/humidity duplet is really changing with time for each package. For each package, some are a little cooler and some of them are a little warmer. And we believe that that reasonable range as encompassed in the left-hand slide there has been incorporated in our projections of the response that are then fed into the projection of how the rest of the repository system responds.

So, I go on to the next slide with respect to seepage and thermal seepage. Probability of seepage during the thermal period is low, but not zero. You'll see even on the right-hand slide there--or, I'm sorry, the left-hand slide, but the right-hand picture of the left-hand slide, even at a few 100 years, there's a possibility for some seepage to occur over some packages because the temperatures are below the boiling point for a certain fraction of the packages. This happens to be for the mean infiltration, mean thermal conductivity case. There would even be more packages in that initial seepage possibility realm if I looked at the high thermal conductivity/high seepage case.

So, there is a possibility of initiating some seepage during what has been called the thermal pulse period on some packages that may happen to be at the edge of the system. But it's a low probability. And, generally, during that period, there is no seepage if the temperatures in the drift wall are above boiling, as Bo pointed out in the bases Bo presented.

Once we get past that time period which, you know, is in the first 100 to 1,000 years-ish, so a few percent to up to 10 percent of the regulatory time period, we are into the seepage representation, which has factored into it the change in percolation flux associated with changes in climate. So, the fraction of seepage goes up and the fraction of waste packages that potentially see seepage goes up. And Bo presented the bases for that.

All of these result in a range of possible seepage rates and amounts that vary across the repository block. And those ranges, you see an example of that range in the lower left-hand corner, and the uncertainty in that range, which is based on the upper left-hand corner, have been incorporated in the assessment of post-closure performance. Let's go onto the next slide.

Okay. We then went on to Carl. As I said before, we broke it up into two parts; during the thermal pulse and, if you will, after the thermal pulse. And as Carl pointed out, we have no observations anywhere in the mountain or around the mountain of any calcium or magnesium chloride salts. That's not to say that calcium or magnesium chloride salts don't exist anywhere. They clearly exist some places. And, in fact, there have been some observances of it in the High Sierras, as Carl said. But you've got to get that salt somehow to Yucca Mountain, which means it can never have seen a humidity above 20-ish percent, the deliquescent relative humidity for calcium/chloride type salts as Carl had. Or the likelihood of that salt somehow getting to Yucca Mountain never having seen a 20 percent humidity, you know, given day-to-night fluctuations, is essentially zero.

So, the possibility of getting a calcium/magnesium chloride salt sitting on a waste package, we think, is essentially zero. Even if it did occur, as Carl pointed out, it quickly is unstable and would quickly become stable with chemical reactions with other constituents, with CO_2, etcetera, and H_2O vapor. Even if those salts were present, as Carl pointed out, they would quickly volatilize. Essentially, Carl's--the first three bullets here which Carl walked through are the same as--let me go--I'm not sure where we put it, John. We put it on this slide? No. The EPRI's--no. EPRI's all three--three things.

Anyway, the first three slides that we have on there essentially are the nos or highly unlikelies that EPRI identified. So we don't think the calcium/magnesium chloride exists or is stable, which are nos on their diagram. Well, maybe it's, I guess, the first two nos. And that given the open system that it would evolve, would volatilize, especially at the high temperatures, and the acid gas would be dispersed very quickly into the repository block. So, let me go onto the next slide, if we can get out of the EPRI conclusions. Okay, thanks.

Now, we're talking about the seepage part and the chemistry evolution in the rock and the evolution of that chemistry in the drift when that water seeps into the drift. The chemistry evolution, I think as Carl pointed out this afternoon, was based on direct in-situ observations and models and supported also by smaller scale laboratory tests. The evolution of that pore water, even though it is complex, when you simplify it to look at a few key constituents within that water, for example, fluorides or nitrate/chloride ratios, can fairly readily be explained. And the bases for that evolution from the rock into the drift, and it's evaporation in the drift, and changes in concentration within the drift, I think Carl gave a very excellent presentation trying to integrate how those chemistries evolve.

Just stepping back a little bit, we realized that last May, sometimes the abstraction process in TSPA can appear complicated and we may over-complicate it somehow in how we present it because there was quite a bit of discussion, as I recall, last May of binning of waters of different types and how those bins evolve and things like that. We clearly didn't talk about bins in this meeting. We talked about how the chemistry evolves, not how the abstraction of different water types is grouped together into like compositions. So, it's probably easier to explain when you look at a water composition and how that water composition evolves rather than the whole family of potential conditions that the binning process that we talked about last May leads to.

I think, as Carl pointed out, the initial aqueous chemistries with the exception of one which is the W-0 type water, the HD perm type water as presented, all evolve into a sulfate or bicarbonate type brine; varying degrees of nitrate/chloride ratio, as Carl presented, and talked then about the importance of that nitrate/chloride ratio and total chloride concentration when we got to the discussion of corrosion.

It's important to point out, Bullet 4, that those brines, even if they exist in the rock, which they do, and come into the drift, which they might with some probability, there is still a drip shield there. So the degradation characteristics of the drip shield, which we did not talk to you about today, but the degradation characteristics of the drip shield then come into play, which affect the probability of that chemical constituent getting into direct contact with the Alloy 22.

This range of chemical conditions in the aqueous phase and the evaporation of that chemistry within the drift has been incorporated within the TSPA. If I can go on to the next slide. Okay, I think I also have one from--well, let's just skip over that. I thought I had one from Roberto

Pabalan. I do? Okay. With his conclusion slides from yesterday.

I think it's important to point out that it was interesting, I felt, yesterday that EPRI sort of focused on the deliquescent part of the Board's question, and NRC sort of focused on the seepage and brine evolution part of the Board's question. I think they both recognize, as we do, that they are two different sets of conditions and you have to evaluate them separately because they are different chemistries.

So, I think what Roberto concluded yesterday was that he had a time period, he called it Environment II, Joe Payer called it Environment IV, where there is a potential for a chemistry existing that can lead to low nitrate/ chloride ratios and which with those potentials have a potential for, depending on the temperature, conditions of initiating localized corrosion if they contact the waste package and they concentrate. So, you have to have all those ifs lined up, which we'll get to in a second, but they identified same conclusion, I think, that Joe had and Carl had, that there is a window of environments where the nitrate/chloride ratio can be low enough and the chloride concentration can be high enough that the potential for initiating localized corrosion occurs if that contacts the waste package.

Let me go onto the next slide. So, it's consistency in the conclusions there, I believe, even though we're obviously coming at it from very different approaches and even different data sets in some cases.

This was a discussion of how much water are we talking about when it comes into the drift. It starts out as 20 to 40 liters per year. Those are kind of the mean rates for a mean seepage when you have the monsoonal climates and the glacial transition climates. So, we're already focused on this time period out beyond 600 years. At time periods less than 600 years, the values are about a tenth of that, roughly. But that water then has to concentrate and evaporate, so then you're down to, as both Carl and Joe said, you're down to milliliters, to tens or 100 milliliters, of water per year per package to get that degree of concentration to increase the chloride concentration, and the nitrate chlorides are more or less going hand-in-hand as they evaporate.

This range of volumes, both in the seepage side and range of composition concentrations in the aqueous side, are included in the TSPA model. So, this uncertainty and variability of these aspects are included directly within the assessment of performance.

Going to the next slide which get into the corrosion part, Joe--I can't follow up Joe, to be honest with you, so I might just skip over these, I think. I think it's important to say that we have significantly additional data that Joe presented. Let me show, yeah, let's keep that, the logic diagram that we have up there that show all the things that have to have lined up with answers of, in this case, no, before localized corrosion can be initiated. And that's even given the discussion that we had associated with the conservativeness of the critical potentials that are measured, given the crevices that both we and the NRC used for the testing methodology for determining that critical potential.

So, we have to go through all of these nos to evaluate the initiation of localized corrosion. Joe then talked about, given it's initiated, will it propagate? And there was--there is some uncertainty associated with that. Although there was some assessments done and there's literature available that show the, if you will, the slowing of the propagation of localized corrosion, given it's initiated, even in Alloy 22, and Joe presented some of our own data that suggests that with the reduction of the current, keeping a constant fixed potential. That particular process has not been included in the TSPA.

So, within the TSPA, if a crevice is initiated by a certain nitrate/chloride ratio with a certain temperature range for a certain chloride concentration, and given the presence of the crevice, that crevice will propagate through the waste package. So, the fact that, as I think as Joe pointed out, additional research, additional analyses, additional study of the effect of crevice propagation in Alloy 22 in repository relevant conditions is still required to, if you will, take full benefit for that particular process within the post-closure performance assessment.

As Joe pointed out, the conditions to support localized corrosion, given that logic tree, are only possible during Period IV, and only if the nitrate/chloride ratio is less than some critical value where there's some uncertainty in that critical value, if the temperature's greater than some critical value where there's some uncertainty in what that value is, if the corrosion potential is high enough, I mean, there's some uncertainty on what the corrosion potential is, and if severe crevices are present, and there's uncertainty in that. So, the uncertainty in all of these aspects, i.e., where is this boundary? I think Dr. Latanision asked is 90 degrees a fixed, hard, fast, you know, in-the-sand kind of point where below which there is no localized corrosion? And Joe correctly answered no. There's uncertainty in where that point is. I mean, that uncertainty is a function of the environment. It's a function of the chemical environment in particular. So, that uncertainty is being factored into the system assessment.

I believe I stuck in here a slide from when Darrell Dunn. He kind of had the similar conclusions as Joe's. Passive corrosion rates should depend on temperature and similar conditions, we agree with that. Accelerating uniform corrosion has been observed. Localized corrosion depends on the chloride concentration and the concentration of inhibitors, the temperature, and the metallurgic condition. And that bullet is certainly true.

Those factors, the metallurgic conditions, the weld conditions, if you will, the inhibitors, the chloride concentration and temperature are all factored into the probability of initiating localized corrosion based on the data that you've seen from NRC and that you've seen from Joe. And, as I say, Joe presented a subset of our data, but I think a representative subset of the data used to make the conclusions associated with that third bullet. And I think we also presented the data that that confirmed from our perspective what NRC observed from their own tests, from the Center's tests, with respect to the effects of nitrate and carbonate and sulfate.

I don't believe the effects of sulfate we're taking credit for. We're right now within the probability of initiating localized corrosion, but the effects of nitrate are being considered. Let me go on to the next slide.

Okay. Joe presented not all of the tests that were indicated here. And not all of these, more than 200 open circuit potential tests and more than 300 cyclic polarization tests, have been completed in the last nine months, but I would have to guess and Tammy and Greg would have to correct me that some two-thirds or three-quarters of them were completed since the last May's Board meeting. So you are seeing a lot more data, linear polarization data, cyclic polarization data, and there are more data behind the data that Joe was presenting today.

Those data were focused in--I think the question was excellent this morning--they focused in on the sodium chloride, sodium nitrate, calcium nitrate type conditions that the in-drift chemical environment that we believe was most likely to see. And it also focused in on trying to detail out where the effects of nitrate, the effects of sulfate, where there are beneficial effects, were no longer present or observed with respect to the critical potential. So, we purposely focused the testing program over the last six, eight, nine months on exactly those environments, on exactly those nitrate conditions, over exactly the temperature range that we felt was more relevant.

In addition, we have collected the data, not cyclic polarization data, but some of the other--the linear polarization tests, at even a higher temperature. You saw the one data set there that went up to 220 degrees

Centigrade. Those data are still being collected at Livermore. Those high temperature data are still being collected at Livermore as are additional cyclic polarization tests over even additional chemical environments.

So, we see a snapshot in time today just as we saw a snapshot in time a year from now, but with a lot more information behind that snapshot and a lot more integration of that information with respect to its relevance to the degradation of the waste package container. I had some--so if I can just go to the summary slide, John.

I think the last report in November and the letter in October correctly reflected the Board's understanding that they had based on the information the Project and others had presented to them last May, but there's additional information. Some of it directly focused at answering the Board's questions. And I think we benefitted from the hard questions that the Board asked to get some of the specific tests to address specific questions completed.

We focused the testing program, which we had plans on doing anyway, on the specific environment and uncertainty in that environment that's more likely to evolve in the drift. And that additional information and additional syntheses of information is what we saw today. We also, as I have down here at the bottom, we did some additional analyses directly focused at the particular questions that the Board asked. In particular, the results that you saw from Bo with respect to the thermal seepage tests, if you will, that NRC performed, we felt important to be able to explain that test using our models, rather than having someone else interpret it for us. And the relevance of that test using our models, which we did.

We felt it important to explain and try to understand the environments that the Catholic University tests were being performed under, and could we evolve? And I think Carl had in one of his backup slides, he didn't go into it, could we evolve thermodynamically a condition that looked like pHs of .5, or zero, or minus .5, such as in the Catholic University experiment?   So we were, if you will, testing our own models and our own understanding using the observations of others.

There's additional analyses the survey has performed, and Zell talked a little bit about it and Carl presented it in his briefing on the dusts, you know, to try to understand the soluble fraction in the dust, to look at other places of dust in the arid Southwest and what those dusts look like and how representative they are to our conditions in order to answer the Board's questions. Next slide, John.

Okay, so now I'm in conclusions. So, I believe the three presenters and the healthy dialog during the discussion and presentations addressed the questions and concerns the Board raised last November and October. We did that with additional information and additional syntheses of information. We used all lines of evidence we could get our hands on.

I think the Board has recommended to the Project numerable times to not just rely on one aspect, not rely on just a model or something like that, but--or sensitivity analysis, but cover the full gamut of possible lines of evidence to address a particular issue. And we did that. We did that with additional tests. We did in-situ observations, laboratory tests, models with additional sensitivity analyses, and with comparison to alternative models, and, as appropriate, some natural analogs that all three presenters presented to you; Bo with respect to geothermal systems, Joe with respect to the use of Alloy 22 and other industrial applications, etcetera.

We conclude from all of this, not surprisingly, the same conclusion we had this morning, that the conditions necessary for widespread localized corrosion will not occur during the thermal pulse for all the reasons that we laid out throughout the day, mostly focused on the deliquescent and brine seepage evolutions. The chemistry and temperature are such that the possibility of initiating localized corrosion during the thermal pulse is extremely unlikely. After the thermal pulse, we have a period, this Period IV in Joe's talk and Period II in the NRC's talk, where the possibility of initiating localized corrosion, given a number of if statements occur, could occur.

Finally, on the last slide, the uncertainty in all of the above, which of course there is, has been or is being included within the total system performance assessment. The effect of that uncertainty will be evaluated as part of the performance assessment, if you will, the post-processing of the results and the evaluation of what the results are. And we'd be happy to present that to the Board in September, and I think we have planned a two-day or some number of day meeting on that particular subject.

And when we do that, I think it would be worthwhile just to come up several times here to talk not only about performance assessment, but also talk about performance confirmation. You know, another key element of the license application is to talk about the requirements. I think Tim McCartin mentioned it earlier, half an hour ago, the requirement of performance confirmation. It is a requirement, and it will be in the license application, and the specific tests that are required to meet that requirement will be identified. And I think probably September time frame would be a good time.

The revision of that is in review right now within the Department. And there's some dialog back and forth between the Department and ourselves on getting that final performance confirmation plan which uses the bases for Section 4 of the Safety Analysis Report completed within the next couple of months.

So, with that, I'm going to stop. I took a little longer than my 15 minutes.

LATANISION: No, thank you, Bob. That was very helpful.

I think we should take a few minutes for the Board to ask questions if you have any that you'd like to address at this point. Mark?

ABKOWITZ: Abkowitz, Board.

LATANISION: Hold on.

ABKOWITZ: Abkowitz, Board. Abkowitz, almost electrocuted member of the Board.

I wanted to start, Bob, by making a correction to one comment that you made. To my knowledge, no Board member has suggested that they would be comfortable with a hotter design than what you have proposed. The context of that was a question that I asked you which was given how emphatically you believe that corrosion is a non-starter, and that that was a line of defense that no--perhaps was not being eroded into, then why not go to hotter design. I was more questioning your confidence--

ANDREWS: Okay.

ABKOWITZ: --not any particular position that I had.

ANDREWS: Okay, thanks for the clarification.

ABKOWITZ: I did want to run through a sort of a different type of decision or logic tree, and would like to ask you to react if you would. And it goes somewhere along these lines. It starts off with the question which is a riskier environment from a safety performance standpoint, a cold design or a hot design? And I've yet to hear an argument for why a cold design presents a higher risk.

I think I've heard information over the last couple of days to suggest that we are further along the learning curve in terms of whether or not a hotter design presents a particular risk with regard to corrosion. I think I've also heard that this is a very complex process with many different scenarios and much conceptual development that's underway. So, we may be narrowing the risks that's presented by that, but I don't know whether we've totally resolved it. So, from a decision tree standpoint, the question of which design is less risky, cold or hot, one would go into the direction of cold is less risky.

So, then you go to cost benefit analysis and you ask the question, you know, is there a benefit/cost ratio here that suggests that we have some kind of tradeoff between risks and costs? And, obviously, that's an important question from any standpoint because we're always into that tradeoff. And from what I've been told from previous work that DOE has done and shared, that the overall economic cost of each option are relatively the same; effectively, a wash is what I've been told.

And so then we come to the conclusion that if one design is less risky than the other and they both cost the same, the conclusion one has to reach is that agency ego is dominating common sense. And, therefore, the only question that's left on the table, given the stage that we're in right now, is whether the period of time that's necessary to make the design decisions to go to a lower risk, higher benefit/ cost alternative is worth the tradeoff versus the thousands of years of improved safety performance. And that's the question on the table. So, I would just like to ask you to react to that decision tree.

ANDREWS: Okay. I appreciate the question. Let me talk about the risk part of it, which is something I know about and I'll leave somebody else to talk about the cost part of it and the cost benefit analysis that the Project, the program, the country has to go through with that.

On the risk side, there's not a clear indication, you know, right now sitting here with our current models, current understanding, current range of potential thermal hydrologic responses, thermal seepage responses, chemical responses, drip shield responses, that it's a slam dunk that colder would be better, if you will, from a dose risk perspective. One could make arguments depending on how you went to that cooler design, in fact, the risks went up. They would go up with respect to things like the probability of a igneous intrusion dike intercepting the repository block, just from a flat out geometric area perspective, if that's how you went to a cooler design. There's clearly other ways to go to a cooler design.

But I think we saw both in the way Joe presented this range of potential, and I want to emphasize potential, susceptibility for localized corrosion, and the same way NRC identified this potential range of potential susceptibility for localized corrosion, there is uncertainty in where that point is. It's not a 90 degrees, you know, flat out, we're 100 percent confident that everything below 90◦C would have no potential for localized corrosion. In fact, the Center presented some data for, I believe, it was low nitrate/ chloride ratios at significantly lower temperatures, you know, 60 degrees, 70 degrees Centigrade, where the possibility at least appeared possible to initiate localized corrosion at a lower temperature.

So, simply, I think in the Board's letter or report it said the information available to us to date says below 95◦C looks pretty good. I'm paraphrasing a little bit. I think the information available now would say, well, at 95◦C is probably not the cut line, but it's somewhere with some uncertainty below that. And, during that time period, there is going to be the possibility of thermal seepage because you are clearly, the packages below 90 or 95 or 80, there is a possibility for seepage to occur and the chemistry that had evolved in the rock to get into the drift.

I think it was important, one of the things Carl pointed out with respect to the chemistry evolution, is the chemistries are, more or less, after a few thousand years. They're kind of evolving back to the ambient chemistry. That ambient chemistry has some range on it, but some of that range includes low nitrate/chloride ratios. Those low nitrate/chloride ratios, even in the ending condition, were the temperature to be 70°, 80°C to contact the package, could initiate localized corrosion.

So, having said that, is it better from a post-closure risk perspective, and I'll just focus on post-closure risk, from a post-closure risk perspective to have some design that was cooler or some design that, like we have, that's a little bit warmer? It's not at all clear that one would be any better than the other. Then you have to factor in, of course, the pre-closure risks. And maintaining an open facility for longer, maintaining ventilation systems for longer, making a larger repository have pre-closure risks associated with them, actual safety risks; worker safety and other risks associated with that. So, you can't divorce the decision making process from the pre-closure risks either.

We've kind of focused on one aspect here which is the next 10,000 years rather the next 50 years during construction and operation. So, having said that, from a risk perspective even though we haven't done the quantitative analyses, I have to tell you, there's no "a priority" precut answer that one would be demonstrably better given all the other uncertainties than the other.

Now, on the cost part of it, probably someone in the Department should talk the costs. I believe one was a little bit more costly, to be honest with you. The lower operating mode was more costly depending on how you developed the costs. And if you ventilated for 300 years versus ventilating for 50 years, there's clearly some costs associated with that. If you increased the area by a factor of two, clearly there's some costs associated with that.

Joe, do you want to--Joe Ziegler?

ZIEGLER: Yeah, this is Joe Ziegler, DOE.

I think that probably the most definitive thing on that was what was in the EIS where we looked at a range of thermal operating modes. And my recollection is, is there was a greater cost associated with the lower temperature operating modes, and it was basically due to what Bob said, is we extend the operation of the repository many, many years, then the costs are going to go up. You extend the amount of excavation that you do, the costs are going to go up. I can't remember the exact numbers. I think it was in the range of 15 percent or so from one extreme on the operating mode to the other one as far as temperature goes.

And I think the EIS also has got a fairly well documented quantification of the environmental impacts including worker safety, occupational safety, additional radon release from additional excavation which would affect both workers and the public, and that's documented pretty well in the final EIS.

LATANISION: Bob, thank you very much.

I think at this point, we'll turn to the closing comment period or wrap-up statement period from the point of view of the other organizations that are here. I understand that all of the EPRI folks have left, and based on a short conversation with John Kessler, I understand that they felt--he felt that their comments had been adequately described yesterday. So, apparently we will not have any need for a closing statement or wrap-up statement on behalf of EPRI.

I'll call next on Tim McCartin, from NRC.

McCARTIN: I'll be very brief. We thank the Board for the opportunity to participate in the meeting, and I think we take the same approach as EPRI. I think we explained our understanding yesterday and have nothing further to add.

LATANISION: Okay. Thank you.

Maury Morgenstein, on behalf of the State of Nevada?

MORGENSTEIN: (Inaudible.)

LATANISION: Sure. Sure, by all means.

MORGENSTEIN: But they're quick ones. I'd like to start out in recognizing the fact that the Program really has come a long way, especially since several years ago or maybe around the year 2000, I don't remember exactly when, when the Program was sitting in J-13 in the saturated zone in the laboratory looking at reactions that we deem today clearly non-representative of the issues that are at hand. So, we've come a long way, but I don't believe that we are at the end of the road, and I don't think that we should think because we now have some basic understanding of what might be happening that we really do have a full breadth of knowledge necessary, for example, for licensing.

Following that, the issue with regards to whether we, the State, would prefer, if we had to have a repository, a repository that was deemed cold or cooler, or one that was deemed, as it appears today, warmer, we would unequivocally go to the cooler repository for a host of reasons all of which can be summarized by several issues of which can be summarized by one statement. And that is a cooler repository is one that we could get down and understand.

It is manageable from the point of understanding the near-field reactions. It is clearly unmanageable in the state of a hot repository. Now, whether this results in a repository configuration that would cause additional costs or there are other attributes that we need to factor in, somebody has to do a cost benefit analysis. Do we want one that we can rely on that's safer or do we want one that has less radon exposure to the workers? But what are--what are our considerations?

With respect to a hot repository, we believe that the Program has gone somewhat astray on several items. If we, for example, went to Joe Payer's excellent representation of the different stages and we went to stage, I think, the hot--the thermal stage, and that's probably stage three, if I remember correctly, if I'm wrong tell me what number it is. But if we went to the thermal stage and we took a look at several issues, maybe my point will be made a little bit clearer. Essentially, I have four comments on the thermal stage.

One, we saw that the deliquescent salts that were studied or looked for, which are essentially calcium-chloride/magnesium-chloride, were not deemed to be reasonably expected and in the salt packages that we see in the dust. There was no mention of nitrates. No mention of an attempt to look at, say, magnesium nitrate. Yet, we are all clear that magnesium nitrate salt deliquesces and that if you put C-22 in it, C-22 reacts and corrodes.

I mean, we have presented this information so that we know that it's a bad player. Not all nitrates are, in fact, inhibitors. They may be inhibitors under conditions of local corrosion, but under conditions of general corrosion, they appear not to be, at least in some concentrated circumstances, and that's really what we're looking at. We're looking at deliquescent salts on a metal surface. Yet, we ignored the major player. So, we're not ready to give up the thermal period from the point of view of dust. We don't think that the analyses have been adequate at present.

The other major issue that really troubles us is the concept of seepage. In almost all cases, we're looking at the mean seepage, and our understanding of seepage events is that they are going to be episodic. When we go in the field in the summertime or even in the--well, dominantly in the summer, we get rained on. And when we see floods at Yucca Mountain, I've been out there and Forty-Mile Wash was completely flooded, and actually there was a rowboat that went down Forty-Mile Wash. Don't ask me where it came from, but it was very weird to see this, and this was back in the 80's.

That is an extreme event. We do have extreme events almost on an annual basis. It is those extreme events that produce water in the repository. It's not the average annual, mean annual, rainfall or infiltration. If we concentrate on the extreme events and then concentrate on the vapor barrier, which is purported to exist around the opening of the tunnels, we can overcome the vapor barrier during the thermal period under two conditions: one, sufficient head in a fracture system that carries water, certainly not one that doesn't carry water, so it will be not all over the repository; two, if there's a high enough salt content in the vadose water coming through.

So there are two clear conditions in which we can go through the vapor barrier and produce water in the near-field. If we can produce water in the near-field, and that water contains solute, and it evaporates on hot metal surfaces, whether it be the drip shield or canisters or both, during the thermal period, we can have serious reactions in the repository much more so than we would under cooler repository conditions. We consider this extremely serious, yet we do not see the Program focusing in this area.

With respect to the--and, essentially, my final comment, with respect to the other end of the temperature scale, we don't see any cutoff point. There may be one and probably there's likely one with respect to localized corrosion. We do not view localized corrosion as the only significant means of corrosion in the repository. We have reason to believe based on tachyhydrite reactions and formation reactions that we will, at least, see temperatures from 65 degrees to 140 as being very significant. Tachyhydrite is only one of a multitude of salts that we have looked at now that show strong reactions; strong reactions under conditions of 1X pore water, unconcentrated, dripping on hot metal surfaces. No preconceived Soxhlet experiments. Simple hot plate.

You have to have some humidity. It's very important. Without which if it's totally dry, no reactions. So that there are some conditions necessary. These conditions seem reasonable. They're not outlandish. They fit within the confines of a cold repository as well as a hot. Under the conditions of a hot repository, if you form deleterious salts, which you're likely to do, those will stay active probably through the full period of confinement.

Thank you very much.

LATANISION: Let me ask if the Board has any comment or question to follow up?

(No audible response.)

LATANISION: Anyone from the audience want to respond?

(No audible response.)

LATANISION: In that case, Mr. Chairman of the Executive Committee, I pass the baton to you for a wrap-up on behalf of the Board.

DUQUETTE: This will take about 10 seconds, but I do want to thank everyone who participated in the meeting. I especially appreciate comments from the public in this particular case.

As everyone in the room knows, the Board's mission is to evaluate the technical and scientific validity of DOE activities. So, we especially look forward to hearing from DOE on the new data and research on this subject. We did give DOE some latitude to put together today's agenda, and it's clear that a lot of hard work in developing new data and developing new concepts and in making the presentations went into their effort.

We especially appreciate the Nuclear Regulatory Commission, EPRI, and the State for participating in the meeting, although you're not required to, of course; only the Department of Energy is when we hold these meetings. So, we really appreciate your efforts and your presentations.

The discussions over the last couple of days gave the Board a lot to think about. The meeting started out, of course, to just address the thermal pulse. We've gone way beyond that, and we appreciate that. I think we've gotten a lot of good and new information from it.

Bob Andrews did say that the information presented today answers the questions raised by the Board. I'm not sure that that's entirely correct at this point. As is our practice, we will review everything we've heard, the new data that comes with it, and we will, of course, report in a letter to Margaret when this meeting is over with.

And so with that, I think the meeting is concluded. I want to, once again, thank you all for your participation, and it's cocktail time.

(Whereupon, at 6:38 p.m. the meeting was concluded.)