UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER 2003 BOARD MEETING

January 28, 2003

Crowne Plaza Hotel
4255 South Paradise Road
Las Vegas, Nevada 89109

NWTRB BOARD MEMBERS PRESENT

Dr. Mark Abkowitz
Dr. Daniel B. Bullen
Dr. Thure Cerling
Dr. Norman Christensen
Dr. Michael Corradini, Chair, NWTRB
Dr. Paul P. Craig
Dr. David Duquette, Afternoon Session Chair
Dr. Ronald Latanision
Dr. Priscilla P. Nelson
Dr. Richard R. Parizek

SENIOR PROFESSIONAL STAFF

Dr. Carl Di Bella
Dr. Daniel Fehringer
Dr. Daniel Metlay
Dr. Leon Reiter
Dr. David Diodato
Dr. John Pye

NWTRB STAFF

Dr. William D. Barnard, Executive Director
Joyce Dory, Director of Administration
Karyn Severson, Director, External Affairs
Linda Hiatt, Management Analyst
Linda Coultry, Management Assistant
Davonya Barnes, Staff Assistant

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I N D E X
(Cont.)

PROCEEDINGS

8:00 a.m.

CORRADINI: Good morning. My name is Mike Corradini. I'm Chair of the Nuclear Waste Technical Review Board, and it's a pleasure to welcome you to the Board's first meeting of 2003.

Let me first give you a brief background on the Board itself. Our Board was created in the 1987 amendments to the Nuclear Waste Policy Act. Congress established the Board as an independent federal agency to evaluate the technical and scientific validity of the activities of the Department of Energy as related to the disposal of commercial spent nuclear fuel, and defense high-level radioactive waste. The Board is required to report its findings and recommendations twice each year to the Congress and to the Secretary of Energy.

The Board is, by law and design, a multi-disciplinary group composed of eleven members with expertise covering a wide range of disciplines. Members of the Board are appointed by the President from a list of nominees submitted by the National Academy of Sciences.

Before I introduce the members of the Board, I regret having to announce the recent resignation of one of our members. I'd like to take a moment to reflect and share on the outstanding contributions made by Debra Knopman to the work of this Board during her six-year tenure.

Debra's sense of responsibility to and enthusiasm for the mission of the Board commanded the admiration and respect of all the members and the loyalty of the staff. She repeatedly demonstrated keen technical insights and an ability to focus on the issues of greatest importance to the Board's review work. Equally important was her willingness to listen, and her patience and her ability to get at the myriad of details involved in understanding the critical issues. She never ceased to impress the Board members and staff in her ability to evaluate the details precisely and in the proper context.

As a consequence, her judgments were greatly valued and will be sorely missed. We wish her the very best in her future endeavors, of which we know there will be many, and want to convey our most sincere and heartfelt thanks for a job well done.

Now, let me introduce you to the current members of the Board. As I introduce them, I'd like to ask them to stand briefly or acknowledge that they're here, and be identified. Let me again remind you that we all serve in a part-time capacity. In my case, I am Chairman of the Department of Engineering Physics at the University of Wisconsin, Madison. My areas of expertise relate to nuclear safety as well as industrial safety, with emphasis on subjects such as multi-phase flow, heat transfer and mass transfer.

Mark Abkowitz is Professor of Civil Engineering and Management Technology at Vanderbilt University in Nashville. He is Director of the Vanderbilt Center for Environmental Management Studies. His expertise is in the area of transportation, risk management, and risk assessment.

Dan Bullen is an Associate Professor of Mechanical Engineering at Iowa State University. His areas of expertise include performance assessment, modeling, and materials science. Dan Chairs both our Panel on Performance Assessment and the Panel on the Repository.

Thure Cerling is a Distinguished Professor of Geology and Geophysics and a Distinguished Professor of Biology at the University of Utah in Salt Lake City. He is a geochemist with particular expertise in applying geochemistry to a wide range of geologic, climatological, and anthropological studies.

Norm Christensen is a Professor of Ecology and former Dean of the nicholas School of Environment at Duke University. His areas of expertise include biology, ecology, and ecosystem management. Norm Chairs the Board's Panel on the Waste Management System.

Paul Craig is Professor Emeritus of Engineering at the University of California at Davis, and he's a member of the University's graduate group in ecology. His areas of expertise include energy policy issues associated with global environmental change.

David Duquette is Department Head and Professor of Materials Engineering as Rensselaer Polytechnic Institute in Troy, New York. His expertise is in physical, chemical and mechanical properties of metals and alloys, with special emphasis on environmental interactions.

Ron Latanision is a Professor of Materials Science, Professor of Nuclear Engineering and Director of the H.H. Ulig Corrosions Laboratory at MIT. His areas of expertise include materials processing, corrosion of metals, and other materials in different aqueous environments. Ron is also a Co-founder and Chairman of the MIT Council on Primary and Secondary Education.

Priscilla Nelson is the Director of the Division of Civil and Mechanical Systems for the Directorate for Engineering at the National Science Foundation. Her areas of expertise include rock engineering and underground construction.

And, Richard Parizek is a Professor of Geology and Geoenvironmental Engineering at Penn State. He is also President of Richard Parizek and Associates, Consulting Hydrogeologists and Environmental Geologists. His areas of expertise include hydrogeology and environmental geology.

Now, let me turn to our meeting's agenda. As is customary, we will being with an overview presentation by Dr. Margaret Chu, the Director of the Office of Radioactive Waste Management, who will update us on the developments throughout her program.

Next, Jeff Williams will give us an overview of the operations of the entire waste management system, from waste acceptance to transportation, to waste emplacement at the repository. This also will be the subject of a meeting next month of the Board's Waste Management Systems Panel, which will explore the same subject in much greater detail.

Following Mr. Williams, the Board will hear about the status of the Yucca Mountain Project from John Arthur, the newly appointed Deputy Director for Repository Development.

The morning will conclude with an update on science and engineering activities by Mark Peters.

The afternoon session, to be chaired by Dave Duquette, will include presentations on Nevada-sponsored corrosion studies, materials testing at Lawrence Livermore National Laboratory, waste package manufacturing, the influence of paleosols on fluid flow and solute transport, and planned analyses of the capabilities of the barriers that make up our Yucca Mountain repository.

I must say a few more words about public comment and the ground rules for our meeting. Many of you know this. I just want to repeat it. We have scheduled our public comment period at the end of the meeting in the late afternoon. Those wanting to comment should sign the public comment register at the check-in table. That's located in the back where people are coming in now, where Ms. Linda Hiatt and Linda Coultry are seated. They're waving their hands. They'll be happy to assist you.

If someone wants to comment and absolutely cannot stay until the comment period at the end of the meeting, please let us know and we will try to accommodate you at the close of the morning session.

Let me point out, and I'll remind you again later, that depending on the number of people who sign up for comment, we may have to limit the length of time you have to make your comments during the comment period.

As always, we welcome written comments to the Board for the record. Those of you who prefer not to make oral comments or ask questions during the meeting may choose the written option at any time. We especially encourage written comments if they are more extensive and our meeting time would not allow them to be spoken orally.

Finally, I have to offer one usual disclaimer for the record so that everybody is clear on the conduct of our meeting and the significance of what you're hearing. Our meetings are spontaneous by design. Those of you who have attended our meetings before know that the Board members do not hesitate to speak their minds. In fact, when they do so, they are speaking on behalf of themselves and not on behalf of the Board. When we are articulating a Board position, we'll be sure to let you know that. And you can find final Board positions in our written letters and reports, which can be accessed through the Board's website.

So, let's begin by our first speaker, Dr. Margaret Chu. She was confirmed on March 6, 2002 as Director of the Department of Energy's Office of Civilian Radioactive Waste Management. She has over 20 years of experience at Sandia National Laboratory that ranges from research and development to program management. Her expertise includes nuclear waste management, nuclear reactors, energy policy, nuclear materials management, nuclear non-proliferation issues, environmental remediation, and technology development.

Dr. Chu's experience with radioactive waste management includes service as the Director of the Nuclear Waste Management Program Center, and management positions to the Waste Isolation Pilot Program, a deep geologic repository in New Mexico. Dr. Chu will give us an overview of recent developments within the Office of OCRWM.

Margaret?

CHU: Thank you, Mike, for the introduction. Thank you for giving me the opportunity to give everybody a quick update of the OCRWM program.

Since our last meeting, one of the more major things that happened was in late October, I did a reorganization of my office. The main thing I did was I created a two deputy organization. We used to have one deputy. I've got a two deputy organization, one deputy at headquarters, another deputy in Las Vegas.

In addition to elevating the office in Las Vegas to the deputy level, I also renamed the Yucca Mountain Site Characterization Office in Las Vegas, that's the old name, I renamed it to Office of Repository Development to reflect the fact that the program has turned the corner, and we're in a new phase as a result of the site designation by the President.

I'm very, very pleased to have John Arthur joining our team as the Deputy in Las Vegas to lead our new Office of Repository Development. John later will introduce himself to you and make some remarks.

At the headquarters level, in addition to program management function, we now have three divisions that reflect headquarters program function. First is transportation. The second is strategy development. And the third is the science and technology, an international program.

And on the M&O contractor side, we're welcoming a new person joining us, John Mitchell, who came from Y-12. He will be starting early February. In fact, I think next week. Both John Arthur and John Mitchell have a tremendous amount of experience in managing large and complex programs. I believe they will provide the right kind of leadership that we need for our new phase. So, personally, I'm very, very happy with them.

Now, I want to say a few words about license application preparation. Our schedule is such that we're still planning to submit the license application in December of 2004. The key activities in this area include the post-closure TSPA, the pre-closure safety analysis, repository design, and of course addressing the key technical issues with NRC.

In addition, we need to certify all the electronic documents in what we call the license and support network system, this is part of the NRC requirements, six months before we submit the license application. So, that means by June of 2004, we need to have the LSA, the LSA system, certified. And we have made very good progress in this LSA system area, so I think we will be able to meet that schedule as well.

So, overall, I believe in the license application preparation area, we are on schedule, on track. But, for me, the most important thing of license application preparation is to ensure the application is of the highest quality.

Let me talk a little bit about the transportation program. Like I said, our schedule for license application is December of 2004, and then it will take between three to four years for NRC to review, and to hold a hearing on our program. And, so, the decision will come, you know, three or four years later on whether we will get a construction authorization. And our goal is to open the repository and start receiving 400 metric tons of waste in 2010, the end of 2010.

Now, since the wastes are located in 131 sites in 39 states across the nation, the development of a transportation system by 2010 is one of the most critical elements of a successful program.

So, in the next seven years, we need to have the whole transportation infrastructure developed, transportation fleet acquired. We will have the required shipping casks available, certified, and then we will have the maintenance facilities and services available, all the supporting equipment ready, the operations logistics figured out, waste acceptance completed, and emergency response readiness completed. This is not a small job.

Given the future uncertainties in a complex program like ours, we want to plan this whole transportation program in a way that our path forward will provide us with the greatest flexibility and plenty of contingencies. You know, in the next "X" time, I won't say when, we're not quite sure yet, in our planning, you will see that, flexibility and contingencies, because I believe that's the only way we'll make sure we will get there, given all the uncertainties.

Regardless what the details might be, you know, the outcome of our planning, there are a few guiding principles we'll be using in our planning process. Number one, public safety and public confidence are the most important consideration in our transportation program.

And, secondly, we will work closely and continuously with other federal agencies, state and local governments, and the other stakeholders during our whole planning process, because it has to happen that way for it to be successful.

And then, thirdly, we want to use private industry to the extent possible in our transportation program. And we want to leverage the experience and knowledge of all the transportation people, not only in the U.S., also internationally, to help us build a good system.

And the fourth one, the fourth guiding principle, we will be looking for opportunities in technologies that will enhance the safe and efficient operation of the transportation of waste. This will also be one item that our

Science and Technology Program will be focusing on. And later on, Jeff Williams is going to give you a little bit more detail on our transportation program.

Now, I'll say a few words about our newly created Science and Technology Program. As we reported to you in our last meeting, a Science Technology Task Force was formed in the last fiscal year, '02, to jump start a few ideas. And then since the beginning of this fiscal year, '03, we are very fortunate to have Tom Keyes and Dr. Bob Budnitz joining that program and helping us to initiate the Science and Technology Program. And they have also tapped into a few subject matter experts to help them in a variety of technical ideas. So far, we have looked at a whole suite of ideas, and we're in the process of developing a few potential projects.

Because of the budget situation in '03, we have this plan, the way we're planning is sort of like a phased approach and dependent on how the budget is finalized, and then we'll decide how we're actually going to start up. And I'm hoping in our May meeting, we'll give you much more detail on what are the things we actually will be starting.

And while we're doing the Science and Technology, our goal is to select a project that spans the whole spectrum of our objectives, from increasing confidence, you know, understanding our repository system, to innovative high payoff kind of ideas from short-term wins, to a long-term project. So, we're hoping to cover the whole spectrum.

And my goal is to use the Science and Technology Program, and hopefully to institutionalize this program, so that our repository program can constantly take advantage of the scientific advances in the world for many years to come. So we won't stay stagnant in the program.

And, finally, I want to thank the Board, because you play a very critical role for our program. You point us to the right direction, and you give us honest feedback, and I want to thank you for that. And that's all. Thank you.

Do I have any QA time?

CORRADINI: Yes.

CHU: Any questions?

CORRADINI: Paul?

CRAIG: Margaret, thank you very much, and I'm very, very pleased that you got Tom Keyes on board here. He did a good job on the Board of Radioactive Waste Management.

CHU: I'm pleased, too.

CRAIG: And he's a physicist, so this is really healthy. Welcome aboard.

My question has to do with how you're going to handle new developments in science in the LA process, as we understand that you've frozen input to the TSPA process already, and yet science goes marching on, and of course the role of new science is critical to what the Board is up to.

So that is the area where we expect to interact with you a lot.

What will be the process for dealing with new developments?

CHU: You know, I have sort of like a vague vision that might work. I think whatever we do, if there are new insights that are relevant to the existing LA models, a technical basis, we want to feed it to them. And then in the minimum, either new insights that can be used in the preparation of LA without disrupting, you know, without creating any major things, then they ought to be fed in. And, if something comes in after the license application time frame, something at all, there will be three to four years of review time. If there's new information, it can be thrown in during that period as part of the review cycle.

And then, of course, we are hoping to interface with the project from this point on. So, the performance confirmation part, hopefully we can have insights, too. So, there's different things that may come in, some probably informally, some more formally, and then dependent on what they are, what the new information will be, we just have to keep that communication very close.

So, I don't know if that's a satisfying answer to you, because it's dependent on the topics. It's hard to give an example at this point. But, the bottom line is we will have very good communication between the science program and the license application.

CORRADINI: Can, then Mark.

BULLEN: Bullen, Board.

Actually, Margaret, thank you very much for a nice overview. But, I've got a couple of quick questions about the points that you focused on. First is a follow-on to Paul's question with respect to the Science and Technology Program. You mentioned that you would like to institutionalize it. Could you share with us, you know, based on the budgetary constraints that you have now, and maybe your crystal ball of looking into the future, how would you propose to institutionalize it so that the science and technology would be ongoing?

CHU: This year, I think the money is going to be relatively small. And then '04, for '04, we put in a good request, and we'll see how it comes out. And then, to me, I think it's critical that in the next 18 months, we have to show the potential value added to the whole program. I personally feel that's the key to institutionalizing, rather than keep begging for money and then say trust me, you know, we're going to do this and that. So, those are the initial things that we start, and I think it's going to be critical for our future.

BULLEN: Well, I guess I was looking for maybe something a little more concrete, like is there a potential as the Director that you could tax the other programs to make sure that the institutionalization occurs every year so there's a fraction of a percent that goes to the S&T Program? Or is that something that you don't want to institute right now?

CHU: Not yet.

BULLEN: Thank you. But my other follow-on question is actually going back to license application preparation, and you're talking about the highest quality application, and we agree as a Board that that's probably a great thing to have. But, how is there resolution of the key technical issues going? We had numerous KTIs that have been, you know, identified as needing to be resolved prior to LA. Can you give us a little update on how KTI resolution is coming?

CHU: You know, I think you know that we have a schedule, a master schedule. From a schedule perspective, we are on schedule. Okay? And what I have encouraged my staff to do, and make sure when we address that, really address the key points. Now, the process is a little slow for my taste. There's a lot of like interactions back and forth.

What I would like is to make sure these interactions with NRC are more very focused, and there will be decisions made sooner so we really know what are the remaining issues, so we can start working on it.

So, I think we are working toward a more focused approached in these KTIs to make sure really we address the key points. And I don't want to let the process run over the product. Okay? So, this is the direction we're going to be moving.

BULLEN: Bullen, Board. One last quick question.

With respect to the fact that the budget has been a continuing resolution, has that impacted your ability to address those KTIs, or are the KTIs at the forefront?

CHU: It's absolutely in the forefront, unless they give me zero dollars.

CORRADINI: Okay. Mark, and then Dick.

ABKOWITZ: Abkowitz, Board.

Margaret, I had a couple of follow-up questions for you on the transportation activity. The first one is that you laid out some issues that culminate in 2010, and you I think rightfully characterized the transportation issue as being extremely complex. Is there going to be a formal transportation plan included as part of the license application?

CHU: Transportation plan is not part of the license application. Okay? It's not part of the requirements. But, we are working, I don't know if you are aware, the Secretary of Energy had made a commitment in his testimony to issue a transportation plan by the end of '03. So, we are working toward that. And we've been kind of debating ourselves what level of detail can we provide by the end of '03. So, some level may be a little bit more detailed; others may be more at a strategic level, depending on where we are. So, yes, we will be working on a plan, but it's probably more at a strategic level than a detailed level.

ABKOWITZ: Okay. My follow-up question is I appreciate your comments about wanting to be inclusive with the process as it's developing, and you made mention of the stakeholders that you'd like to bring into this process. My experience is that you're dealing with a large number of different stakeholders in terms of their perspective, and also their geographical location relative to the issues. I'm concerned about how you're going to even come up with a strategic approach by December, and being fully inclusive in this process. Do you have a plan to have customer focus groups? Is there going to be a lot of transparency to this as you go forward? Because public confidence will be a critical issue in all this.

CHU: Right. Actually, our program has had many years of working relations with a lot of focus groups that relate to transportation all these years. So, there is existing cooperative agreements already, working groups throughout. What we are trying to do now is gear those relations up in the very near future as our basis. And then actually as part of the transportation program, we're going to have what we call our institutional plan. That's really the whole thing.

And then I hope Jeff later on, Jeff, maybe you can talk a little bit more about that. Yes, it is very important to us. Yes, it is hard.

CORRADINI: Dick, and then Ron.

PARIZEK: Parizek, Board.

On the listing of topics to study, I'm sure if you ask for input from all sorts of researchers, you'd get a whole shopping list of things to do. Is it task force role to sort through and figure out what projects might be funded in the initial period when you have limited money, and later on, even as you have more money, in order to decide which projects are likely to give you the most value added into this whole process? Or do you have some external people also involved in the review?

CHU: Right.

PARIZEK: Because, in other words, you could have in-house biases in terms of the interesting projects some people would like to pursue, but may not always represent this broad spectrum of needs you have.

CHU: I'll tell you, because of the funding situation, and initially we'll have a small group of people working on it, our plan has been this year, Tom Keyes and Bob Budnitz, what they have done is tap into basically the existing community, okay, knowledgeable about what's going on, and then try to extract what are the things people have been saying, how come RW hasn't looked into this, how come they haven't addressed this. Okay? And use that as the basis of our initial thing. And as FY '04 comes, our plan is to formalize the proposal solicitation process somewhat.

See, the thing is I'm very sensitive. I don't want to create a huge bureaucratic process. So, we need to keep it balanced between tapping into people, but we're not familiar with those folks, and they have tremendously good ideas, and we want to make sure we don't miss it.

So, now it's very informal. Okay? And we tap into an existing pool of folks for knowledge, but the next phase, we can expand it so that there will be a more formal solicitation process so we make sure we don't miss the good ideas.

PARIZEK: But it will still be the task force members, I guess, what, six members?

CHU: Right. They're going to be with us. And then, you know, those six members, and then maybe later on, we want to expand, depending on, for example, we don't have anybody really knowledgeable on transportation issues, for example, right now, we may want to expand that so we bring in people who have that knowledge. So, it's a pretty fluid kind of a review task force for us.

PARIZEK: Thank you.

CORRADINI: Ron?

LATANISION: Actually, I think you've basically answered my question in response to Mark's regarding the issue of public confidence, and developing that. So, I'll pass on that.

But, I do want to add my pleasure in learning that Joe Payer will be joining Bob Budnitz and the crew.

CHU: Right. He is on the review group for us.

LATANISION: Right. As you know, I'm sure he was chairman of a panel on which I had great pleasure in serving related to the waste package issue a year and a half ago, a great addition.

CHU: Thank you.

CORRADINI: Priscilla?

NELSON: Nelson, Board.

This may be a question of semantics, but as I was reading the response that you prepared to our letter, I was struck by use of the word "technology." And in the sense of exactly what it was and when it was used, was science included? Or was science separate understanding, and technology separate solutions, new developments? So, the sense that I have here is is there a difference between the two?

CHU: Not to me. In my own mind, it is the same sort of stuff.

NELSON: Okay. Because in the specific context, many of the actions that are talked about in here are really talked about in terms of technology, in your response. And it was my perception that science was, well, we need a fundamental understanding, but what we're really after is the technology, and that was the flavor that I took from the response. And the blending of the understanding, which necessarily must integrate across the various parts of the project and the incorporation and finding of new technologies is a major area for the future of what you want to have happen. But I can see if it's separate, things happening that may counter.

I was just talking with some of the other Board members this morning about the discussions regarding evolution of the drift environment, and what is the chemistry of the water, what's going on with corrosion, and then separately talking about some of the seismic stability issues, and having backfill still as a possibility, but not really considered for the corrosion aspect. So, it becomes a case of if technology gets developed to solve problems as they arise, sometimes the broader science view that might identify interferences may not be highlighted.

CHU: I very much appreciate that comment. Actually, we're doing, Tom Keyes and Bob Budnitz, this is something we have talked about is not losing sight and then just start going down one area, because what we're really trying to do is the whole thing. So, to me, it's really science and technology to have to view it both ways. Sometimes you understand the scientific things more, and then you come up with the technology solution. Or when you try to bring in technology solutions, you realize there are additional scientific issues pop up. You really need to look at it holistically, and then look at the whole thing. But I very much appreciate your comment.

NELSON: Thanks. That's good.

CORRADINI: Other questions?

(No response.)

CORRADINI: Thank you, Margaret.

CHU: Thank you.

CORRADINI: Our second speaker is Jeff Williams. He has been with the federal government for over 21 years, and with the Department of Energy in the Office of Civilian Radioactive Waste Management for over 16. He has worked on and managed several aspects of the waste management program, including Environmental Assessments and Site characterization Plans for the potential repository sites.

He has also worked on and managed system studies and conceptual designs for a monitored retrievable storage facility, multi-purpose canister feasibility studies and conceptual designs, integrations of the DOE waste into the OCRWM system, total system life-cycle cost, fee adequacy reports, and international activities.

Today, Mr. Williams will summarize for us the proposed operations of a waste management system, from the waste acceptance to transportation, to final emplacement within a Yucca Mountain Repository.

Jeff?

WILLIAMS: Thank you. Thanks for the introduction.

This is actually a presentation of the overall system, and I think I have 20 minutes, and it looks like we're a few minutes ahead of schedule. I really wasn't planning to talk about the transportation plan per se, since there's a panel session at the end of February to talk about that in more detail. But, this is a eye level view, sort of an elementary level presentation on how the overall system can operate. I probably have a bit more slides than I can cover in the short period of time, but I'll go through them quickly.

From an overall standpoint, this is a slide just showing the architecture. Our goal is to accept waste, which I don't think that the Board has heard much about over the years in terms of our relations with the utilities. So, I'll spend a little bit of time on that.

The transport waste, repackage it at the repository and emplace it. This is just a list of many of the major parts of the system. I think you probably know the first repository is scheduled to hold 70,000 metric tons of waste, 63 of which will be commercial spent fuel, 7,000 defense waste. This presentation really focuses more on the commercial aspect of it.

Okay, what I thought I would do, first of all, sort of look at what the situation will be like in 2010, what we'll be facing. And I thought I'd start with the utility side. Our projections are that there will probably be about 72 commercial sites with 104 operating reactors. Today, there's 103. Brown Sperry has said that they plan to restart in about 2007. There's seven reactors that their license will expire prior to 2010. Five of them have either applied for a license extension, or announced their intent to do that.

In the year 2010, those reactors will have generated about 64,000 tons of spent fuel, generating at a rate of about 2,000 tons a year. At that time, of that 64,000 tons, 53,000 will be in spent fuel pools, whereas, 11,000 tons of that will be already packaged up into dry storage at the utility sites. This will be the older, colder fuel. There will be 44 sites that have dry storage facilities in 29 different states.

This slide shows a little bit about dry storage technology. I remember talking with you about this. It probably wasn't you, but the Board, in the '93 and '94 time frame when it was starting. It's important because 11,000 tons of the inventory will be in dry storage at utilities at the time.

So, basically, what's been developed at the utilities are different types of storage technologies. Early on, this is a surrey plant in Virginia, Virginia Power. It's actually called Dominion Power now. There are bolted metal casks made for storage. Their intent was that they would be transportable. However, they don't have transport licenses now, and I think the technology has changed a bit, so that they probably won't be transportable. They're what's called single purpose storage casks.

Subsequent to that, became some canister technologies where a canister of multi-elements was placed into a concrete container. And those technologies are welded shut and they're in storage at utilities.

Subsequent to that began the development of dual purpose technologies, both welded closed technologies, as well as bolted closed technologies. The bolted ones could easily be transported and removed, whereas, the welded ones, to be repackaged, would need to be cut open.

One thing that's significant about these is that they're all heavy. They're big, and they would require transport by rail. So, from an overall waste management system, you see what you're looking at if you're going to take these.

The next slide basically shows our target waste acceptance rates starting in 2010 at 400 tons, going up to 3,000 tons within four years. I'd like to make it clear that these are, as the bottom of the bullet says, these rates are scheduled, are targets only, and they do not create any binding legal obligation on the Department.

The plans would be to then receive this waste over a 24 year period. This is just the commercial waste. The DOE waste would come in at the same sort of levels, with about 10 per cent more each year, which over 24 years, we go up to 63,000 tons, or so, of commercial spent fuel with about 7,000 tons of DOE waste.

Okay, now if we turn to the utilities and we look at the contract that we have with the utilities, basically the manner in which we accept this fuel is really guided by that contract, and the contracts were signed in 1983, and they're currently under litigation right now.

So, one thing that's important about the contracts is DOE doesn't have the ability to select what fuel we want, because the contract lays out the rules by which these things are done. The contract established what's called the oldest fuel first rule. In other words, the first fuel that was discharged from the reactor earns an allocation for that utility for it to go into the queue for waste acceptance.

We issued, well, we did it up until about 1995, something called an acceptance priority ranking and an annual capacity report. And those are reports that are available. The last one was developed in 1995. The acceptance priority ranking report basically shows the order in which plants earned their right in line. So, if you look in the report, it will say Dresden 1 discharged 30 tons in such and such a day, and it's got the first right. And that Dresden 1 then, that right actually goes to the utility that owns Dresden 1, which happens to be Excellon.

Excellon has 20 plants right now. So, Excellon has maybe 30 tons out of the first 400. Actually, I think it's 52.2 in 1998. And although that right was earned by Dresden with that spent pool batch, Excellon could give us whatever fuel they would like to out of what they have at those 20 plants. So, the point is is that basically, we don't have complete flexibility over what we accept from the utilities.

The next slide talks about acceptance criteria, and this is geared basically towards the standard contract and the commercial spent fuel. Basically, we have our obligation to receive all the commercial spent fuel regardless of what type it is, or what condition. There's no other facility that's out there that's planning to pick up failed fuel. There's no failed fuel repository. We're it. And, so, our obligation extends to all the fuel.

The contract does say, however, that utilities are required to classify fuel as either standard, non-standard, or failed, and anything other than standard fuel is subject to delayed acceptance. Now, like I said, our obligation extends to all fuel.

Now, the multi-element canisters that I talked about that are at storage at utility sites, a canister, for example, has 21 assemblies. They may be welded shut. Those aren't covered by the contract right now. When the contract was signed in 1982, those technologies didn't exist. If you read the contract, it talks about PWR, BWR spent fuel. It talks about the sizes and shapes of them, but it has nothing in there whatsoever about canisters or dry storage technology.

Okay, the next slide talks about how we schedule the pickups. And, basically, utilities, purchasers will submit what they call a delivery commitment schedule identifying the location and range of fuel to be picked up 63 months before delivery. The utilities were submitting these in 1993, '94, '95, and we actually approved some of these delivery commitment schedules, and we've approved delivery commitment schedules for 2,850 tons of DOE spent fuel. So, we have basically an agreement with the utilities for what fuel will be picked up for that first 2,850 tons.

And, again, this is something that's also under litigation, and depending on how the litigation turns out will guide us in how we pick up fuel. This first 2,800 tons is basically for the first few years of waste management operations.

Once the fuel is picked up, next we go to the transportation cask fleet. In our EIS we evaluated two different scenarios. One, shipping mostly by truck, and, two, shipping mostly by rail. This is consistent with Margaret's statement about being flexible and trying to understand what happens under either scenario.

I think the most important thing about this is under the mostly rail scenario, there will be about 170 shipments a year. This is a projection that could change somewhat, depending on whether it's 80 per cent rail, 85 per cent rail, whether the spent fuel casks are fully loaded, partially loaded, whether they hold 28 assemblies or whether they hold 17. So, this is a rough estimate.

Under the mostly truck scenario, you can see there will be over 2,000 shipments per year. The estimate that was done in the EIS had 2,200 shipments per year. The bottom line there shows the size of the cask fleet that would be required for either scenario, and the biggest difference being the number of truck casks that would be required under a mostly trucking scenario.

Under the mostly trucking scenario, we have always assumed that the Navy fuel will be shipped via rail. It's packaged up in multi-purpose canisters.

One other point there on the bottom of the slide, the FEIS states that preference is for rail for both Nevada and nationally.

Okay, next I'd just like to talk a little bit about what kind of technology already exists today. Earlier on, I talked about the dry storage at the utilities, and most of the, or several of those technologies for dry storage have also now been certified for transportation. So, these are basically a list of the transportation casks that are certified by NRC, or at least have been submitted to NRC for certification.

You can see the first, there's close to 30 casks that have already been certified and built and loaded with fuel. So, those are sitting at reactor sites loaded with fuel. They're certified for transportation and storage.

Once again, these are very large. They would require rail or heavy haul shipment. And a heavy haul shipment of one of these is not a simple task at all. It would require an extremely long truck. I didn't bring any pictures of it, but it's not an easy task. So, they're primarily rail. They are dual purpose casks. And I don't want to go into all the different characteristics about them, but they're designed to hold the fuel that's being discharged from the reactors and needing to be placed into storage.

Okay, the next slide shows the existence of truck casks. And, basically, we have much fewer truck casks, and the reason why there's an emphasis on rail casks is because it's been driven by the needs of the utilities to do dry storage, and it's much more economical to build a large cask with 20, 25 assemblies as opposed to a small truck type of cask.

However, NAC, Nuclear Assurance Corporation, does have a certified transportation cask that holds one PWR assembly, or two BWR assemblies. Also, there's eight of them that have been built, and it's being used today for different research reactor type fuel shipments, as well as foreign research reactor fuel shipments. I think a shipment is planned from Brookhaven this year, which would use a NAC cask.

Then there's two other ones, the General Atomics 4, which is for four PWR assemblies, and the General Atomics 9, which are outgrowths of a program that DOE funded beginning in about 1988 to develop a truck cask. We stopped funding that in 1996, but General Atomics on their own did go license their PWR truck cask. So, it's a high efficiency truck cask. It holds four more assemblies than the NAC cask. It's right on the border of being a legal weight truck, in other words, being able to transport on the roads without overweight permits. I've joked about we'd need a jockey to drive it. So if I was going to drive that truck, it might be overweight.

And the GA 9 doesn't have a certification yet, that's a BWR cask, neither of which have been built. We did pay for the building of a scale model test of the GA 4 cask, and it was tested in the mid Nineties.

Okay, this next slide basically shows out of those existing casks, you're looking at the spent fuel that's in storage at the reactors in the year 2010, and what those casks, what will we cover out of that spent fuel out of those fuels. And we plotted PWR and BWR, and it's basically the existing casks that are certified out there will carry about 67 per cent of the BWR fuel, and about 55 per cent of the PWR fuel.

It's expected, however, that the industry is going to continue to modify those casks on their own to increase the capability, because they need that as hotter and hotter fuel comes out of the pool, they have need to store hotter and hotter fuel, so they will plan to, we believe through discussions with them, they plan to continue to upgrade their designs, their certifications, to be able to handle basically the full range of fuel.

Okay, this next slide is basically just summarizing the last few slides that I told you about. I think Margaret even mentioned, and the law talks about, DOE is going to use what says the industry to the maximum extent practicable. What we plan to do is use existing casks, existing certified casks. In other words, we don't have a massive effort, like we did in the Eighties, to go design our own transportation casks.

As I just said, the existing casks may need to be enhanced to transport higher burn-up and higher enriched fuel. We expect that to take place through the industry. Industry's emphasis has been on the large rail casks, and that's primarily been because of the economics for storage.

For a mostly truck scenario, additional technology development is required. We've got the GA 4 and the GA 9 and the NAC cask. However, they haven't been built yet, and we believe that there is additional technology and room for some more emphasis in that area. One other area is the DOE spent fuel and the high level waste. Casks for that will need to be developed.

This slide is basically just the NRC cask performance requirements. Regulations require that the casks meet these performance requirements, puncture to drop test of 10 meters, followed by a puncture test onto a spike of four inches, followed by a fire, 1,475 degrees for 20 minutes, and an eight feet underwater immersion. There's additional cask tests.

The demonstrations can be done either by analysis or tests. Regulatory accident tests cover about 99 per cent of the accident conditions. NRC has been proposing to do full scale cask tests, and actually RW requested funds in '03 to support those tests. So, I think you all know, and Margaret said that we've been on a continuing resolution all this year, so we haven't provided any funds.

The next slide is our requirements documents, basically showing some of the things I've already said. Private industry will be used to the fullest extent practicable. This is the highest level requirement document that we maintain for our program. Basically, we say that operations need to have the flexibility at the repository to receive by rail, heavy haul or legal weight truck, and they need to have the flexibility to receive any of these different casks that may be developed, single purpose casks, casks that are either multi-purpose canisters, dual purpose canisters, transportable storage casks, and the specialty casks, such as South Texas Long Fuel transportation casks, or anything else that may be developed.

Basically, the requirements say that we need to be ready for anything, and these are sort of the things that we've identified.

One last thing on the transportation of single-purpose storage casks, I mentioned early on that many utilities have put their fuel in dry storage at the utilities in technologies that aren't certified for transportation, and they basically have a couple choices. One is they can open those containers back up and repackage into a certified transportation cask. They could do that. Actually the DOE, in concert with EPRI, developed a dry transfer system for doing that that's been reviewed by NRC.

There's also the possibility that they may seek a one time transportation exemption to be able to transport their storage technologies to a repository.

The next slide, over the years, there's been a lot of interest in multi-purpose containers, in other words, containers that can be stored, transported and disposed of. As a matter of fact, the DOE funded that program from 1992 through 1996, and then stopped funding of the program, not because we didn't support it, we do support the development of multi-purpose canisters, and as a matter of fact, the Navy is moving forward with that. We would expect I think in a draft RFP for transportation services that came out in 1998, we said that we supported the development of multi-purpose canisters by the private industry. And should any of them be successful, we would share the savings on our system with them. The details of that have never been spelled out.

Routing for OCRWM Shipments. Once we pick it up, then we need to ship it. Our plans are to begin selecting routes approximately three to five years before shipments begin. Our interactions with regional planning groups have basically--that's basically what they are looking for, is three to five years ahead of time.

We did identify preliminary routes in the EIS, and those were evaluated. And as Margaret said, we're committed--committing to working with the states, committed to working with the states and tribes, and we'll consult with them in the selection of the final routes and our planning. We haven't laid out the details of how that's going to be done yet.

As far as route selection, we need to follow the rules for highway routing selection. Basically, the carriers select the routes to reduce transit time in accordance with DOT's regulations, mainly following the interstate highways, bypasses. Also, a state or tribe may designate an alternative route, consistent with DOT regulations.

As far as rail routing is concerned, there's no federal rail routing regulations. Current DOE practices for other DOE shipments has been to minimize the time, minimize the distance, minimize the number of carriers, interchange points, maximize the use of best available track, and in the EIS, we used the computer code called INTERLINE to identify those potential corridors.

Okay, once we transport it, we finally get to the repository, and what do we do with that? Before transportation takes place, we need to know what's going to happen with that fuel. Do we need to blend that for a colder, hot/cold thermal management strategy at the repository? Does it need to be surface aged prior to storage? Anyway, that's important to know ahead of time.

When the casks are received, they'll be received, swabbed, and they'll be unloaded. If the fuel is going to disposal, if it's in a bare fuel transportation cask, it can be loaded directly into a waste package. If it's in a canister, welded canister, it may need to be cut open and loaded into a waste package, or that canister may be loaded directly into a waste package if it's a disposable canister, such as the Navy's.

If it's going to storage, bare fuel could be placed into storage casks such as what they're using at the utilities today. Canistered fuel could potentially be transferred to the same type of storage cask that's used at the utilities.

This next slide is the surface layout, and this is the latest one that's in the conceptual design report. Once again, the February panel is going to go into quite a bit more detail on all these things, the transportation plan, as well as, I understand, a presentation on the surface operational aspects, as well as underground.

This one here shows basically the repository--I don't know if there's a pointer--this shows the current plan for phasing, where things within, this is the fence line right here, this is the first phase. And what's included in the first phase is a transport receipt building. If you can see on your slide, I think it's called a TRB, like the Technical Review Board, transport receipt building, and that's where the transportation cask is received. The impact limiters are taken off. It's swabbed down, and so forth.

Over to the right of that is the disposal canister preparation building. And then behind the two major buildings right here is what we call a dry transfer--I'm sorry--this is the major building for transferring spent fuel in Phase 1. The dry transfer building has a welding capability for the waste packages. It only a capability of receiving and packaging 500 to 1,000 tons per year. So, the second phase, which is outside of that first fence, needs to come on line by year three to maintain our acceptance rates.

The capability of that first building in Phase 1 is about 500 to 1,000 tons per year, depending upon how it comes in. If it comes in in truck casks, we have a much lower capability to process waste, because when you're opening a truck cask with only four assemblies or two or three assemblies, you still have to take off the impact limiters. You still have to go through a number of the same steps as you would a rail cask that holds many assemblies. So, we can process more fuel with a rail cask than we can with a truck cask. So, that's why the capacity, the through-put capacity varies.

This one down here in Phase 2 is a bigger dry transfer building. It has more capability, more lines for welding. And this one down here actually is called the remediation building, and it has a pool in it, and the pool could be used for hot fuel aging, for example. And as I said before, you'll get a lot more detail on this in February.

As we go underground, the potential underground, I think you've seen this phased approach before where the first four panels are sufficient to hold 70,000 tons at a two meter spacing, with panel five having approximately 25 per cent more capacity. It's a modular approach where we can allow adaptive staging to apply lessons learned from one phase to the next.

The first phase will use the ESF to construct that panel one, and the panel one construction takes about 27 months, which is quite a bit of an improvement from the SR design to be able to get ready for emplacement.

The next slide shows underground, once we go underground, the emplacement drift transfer dock. This is the waste package transporter. You can see it's now on wheels. I think earlier designs, it was not on wheels. But the waste package and its pallet will be in this waste package transporter, along with the transfer dock that the waste package sits on, and as it's transported through the repository down to the drift, this is at one end of the drift where it's docked up with the drift, the waste package will be inside the waste transporter. And when it's docked up with the drift, the emplacement gantry right here will pick up the waste package and the pallet, and will move it down the rail lines and emplace it in the waste package. One other thing about this slide, it shows the steel sets in the drift and the rock bolts.

Okay, the next slide gets to the configuration of the waste packages. I think you've heard a lot about waste package corrosion, and so forth. It's a two layered stainless steel C-22 waste package. But what this one shows is it shows a PWR waste package, a BWR waste package, and then another one, a codisposal waste package with cans of high-level waste glass right there. Actually, the way this one is laid out, I'm not sure whether it would be packaged that way with one DOE spent nuclear fuel assembly also in the middle of high-level waste glass. That detail is still under review. But it just shows how those could possibly be mixed.

That's actually the end of the slides. Then there's a summary slide there that I don't think I--I don't have the summary slide with me, but basically, some of the points I made, waste acceptance planning is difficult because we can't pick exactly what we want. Industry has done a lot of development. We plan to use private industry. We've stated our preference for mostly rail. And we plan to select routes three to five years before shipment begins, and at the repository, we have the capability to blend or age spent fuel.

And that's sort of an overview. I didn't address some of the things Margaret asked on the transportation planning, because that's not what I thought I was asked to do at this presentation, and we'll address it at the end of February.

CORRADINI: Thank you very much. Questions? Dave, and Dick.

DUQUETTE: Duquette, Board.

If rail is preferred, will there be a major effort to have to build spur lines or trunk lines to carry the rail out? And the second question is tied to that, is do you envision dedicated trains, or will this be mixed with other commercial activities on railroads?

WILLIAMS: The first one is yes, there would be, to get these large heavy loads there, it has to either be by heavy haul or by rail. We've stated our preference of rail. We haven't made a firm decision on rail. The EIS evaluated five different rail routes to Yucca Mountain, ranging from about

100 miles long, to a little over 300 miles long, ranging in cost from, I don't know, $300 million to a billion dollars to build those things. So, that would be a major decision that would need to be made, is what corridor would we select.

CORRADINI: Just for clarification, so this is at the end station?

WILLIAMS: At the end station, right. And, actually, at the other end of the side, there's many of the utilities may not have rail capability, where you would need a heavy haul or barge to a rail at the utility site. And Nevada is where I'm talking about building a rail, where you have no plans for building rails at utility sites.

DUQUETTE: If they're needed at utility sites, who would be responsible for building them?

WILLIAMS: DOE is responsible for doing the shipment. We take title to the spent fuel once it's loaded at the utilities. Okay? And in accordance with the contract, I've skipped over a few things, we've asked the utilities over the years how they would prefer us to ship, and about 90 per cent of them said rail casks.

Now, some of those don't have rail capability, and it will have to be heavy haul, it will have to be barged. There's barge slips at some of them. And that's something that we would have to work with the local community on. I know when we went through the EIS process, there were a lot of comments about barging, especially in the east, a lot of negative comments, and that's something we would need to work with the utility. I think once they saw heavy haul, there might be some negative perceptions of that.

The dedicated train question, I'd say the question is still up in the air. There's strong opinions on both sides. The Department of Transportation has been doing a report on the value of dedicated trains for a long time. I think we'll wait to hear what that says.

CORRADINI: Dick and then Mark.

PARIZEK: Parizek, Board.

Regarding the question of failed fuel, perhaps you could help me understand what failed fuel is. From a public perception point of view, it would seem like maybe it's more hazardous to deal with that. You say you might take it later, which means would industry be inclined to fix it so it's no longer failed in order to get it out of their plant?

WILLIAMS: Well, I think the normal industry practice may be to can it. Okay? Can it in a small can that's the same size as the spent fuel assembly for transportation.

Basically, failed fuel is something where the cladding has failed, and you can tell that it's failed. The problem is is in a spent fuel assembly, there may be fuel rods inside that assembly that we don't know that they've failed. So, you know, if it's obviously failed, then yeah, it's subject to delayed acceptance. However, we are required to take it. We're going to have to figure out a way to take it. Can it, you have to get special certification from NRC for transporting it, and for storing it if you know that it's damaged.

PARIZEK: If you receive waste, and you say you swab it, and so on, but you find it's not like you want it, do you send it back, or you're stuck with it?

WILLIAMS: No, we can't send it back. We have to have the ability to deal with it. And that building in the second phase called the remediation building has a spent fuel pool when the capability to deal with what we call off normal events will be designed into that building.

PARIZEK: One other question about interim storage. You didn't mention that. Are you planning on storage on the site in view of the waste, or handling rates that you can with? I mean, you could surely go to interim storage. Are you planning that now?

WILLIAMS: I'd say we have plans for a limited amount. And depending upon how things work out, as a matter of fact, that drawing I think shows I think the capacity is about 1,000 tons. The surface layout drawing, I believe that says surface storage. That has a capability of about 1,000 tons. If we were to need more than that because of some thermal strategy or a change in how we, or let's say there was a government decision that we needed to pick up waste much faster than our target rates, something happened out there and there was a national emergency, then we would have to develop the capability. And we've identified other places where storage could take place. But there's 1,000 tons.

CORRADINI: Mark?

ABKOWITZ: Abkowitz, Board.

Thank you very much, Jeff, for your overview. It helped me understand a few things a little bit better.

I really want to make a couple of comments, and you're free to comment back if you'd like. There are some issues that I think are very important as this goes forward, because this is an entire system, there's a lot of different activities involved in it, and the interactions between those activities are very important. So, I hope that as the process moves forward, DOE will take a holistic systematic look at the entire process.

There are a few things that I'm concerned about that I hope will be included in that. Number one, the worst case scenarios were developed before 9/11 and, consequently, some of the issues that we are now aware of as to what could happen and the potential consequences associated with them require rethinking some of that. And, so, I hope there will be a security element to this process.

Secondly, I see that the presentation is really focused right now on what I would call logistics and not operations. At some juncture here, we have to go beyond do we have the capacity to move this stuff from "X" to "Y" and get into issues of how that's going to happen in terms of just a few things on that list would be maintenance, carrier selection, emergency preparedness, communication, and we could go on.

And then, finally, I hope that the process will be cognizant of the fact that there will be a confluence of these shipments as they start to move from their various origins to their destination. In doing so, you're going to have larger volumes of these shipments congregating as it moves towards Nevada. So, we need to be aware of the fact that it's not a linear process per se.

And, also, from a public confidence standpoint, as those shipments congregate, you're talking about passing them through communities that really didn't have any benefit directly from the energy that was produced from the process. And, so, public confidence in the safety and security becomes that much more important.

Thank you.

WILLIAMS: Yes, I think I couldn't agree with you more. Everything you said I think is things that we're thinking about. You know, we haven't developed emergency plans, security plans, yet and things like that, but they are definitely on the top of our mind, and I think that we would be pleased to work with you as we develop this. This is, I think sort of a new beginning where, you know, we have the opportunity to get a lot of input from people. So, I don't have any arguments with you at all.

CORRADINI: Dan, then Priscilla.

BULLEN: Just a couple of quick questions. Could you go to Slide 12, please? This basically shows the burn-up problem that you have with the transport, and you're looking at the fact that if you have a very aggressive transportation schedule of 24 years or so to get everything to the site, you may run into some problems.

Maybe it's an obvious answer that you just used, the rate of packages and more shipments, but you seem to be counting on the development of enhanced technology by the private sector. Are you doing anything to aid in that development?

WILLIAMS: Well, right now, we're not. I mean, that's one thing that we've been thinking about doing, and actually in our '03 budget, we talked about high efficiency, high burn-up, rail casks, the need for development of that. However, like I said, after we've been talking to the industry, basically, their view is that they're going to need this prior to 2010, and different people have been talking to us about the technologies they're thinking about.

So, I think this is a question that needs to be answer. You know, I tell you six months ago, we were saying that, yeah, we're going to need to develop that and fill in that gap, but the industry has been telling us that they're going to do it for us before 2010.

BULLEN: Bullen, Board.

I'm very pleased that you're talking to the industry, which leads me into my next question. You mentioned the multi-element canisters are not covered in the waste acceptance criteria, having been developed since the criteria were developed.

Except for the fact that the utilities are now looking at the economy of scale, I mean, it's cheaper to put it into a bigger waste package, and recently, we've looked at designs that are even bigger than the ones that have been approved, all the way up to 69 boiling water reactor assemblies, and maybe 36 pressurized water reactor assemblies, those are really big containers. And, so, the question that I have is is there any effort by the DOE to speak to the utilities and to maybe design an interface that would say, you know, we can only bury 21 PWRs and 44 BWRs, why don't you take title to the fuel, put them into those types of transportable containers, and not have to reopen them?

Now, the other thing is also that, you know, there's only a small fraction--well, actually, it's a vast majority that hasn't even been made yet. So, these are the kinds of things that you might want to be conversing about?

WILLIAMS: All I can say, Dan, is we did this for four years. We had extensive industry interaction, and we spent a lot of money doing it. We developed a design, we hired a contractor to do a design, to develop something that was storable, transportable, and disposable. And, basically, Congress quit funding the program, and the industry at the time said that it would be best to do it themselves. So, it was terminated in 1996, and we still see the benefits of it. I don't know what more I can say.

NELSON: Nelson, Board.

My question deals with to what extent do you interact with other federal agencies? And, in particular, I've been to quite a number of DOT, the agencies of DOT, planning for next generation, whatever that means, new technologies, smart systems, new modes, and multi-modes. I wonder to what extent the project is interacting in tracking those potential changes and investment that could really change what's available, instead of dealing with regulations as they exist now? Some cases will be even pushing DOT to do things that would be helpful.

And in analogous thinking about FEMA and emergency response, nobody really knows, I don't think, what's going on with Homeland Security Department and what it's going to be in a year. But I know that there's been some very significant rethinking of our responses and mitigation investments inside of FEMA since 9/11, and that's a moving target as well. To what extent does the project expect to interact with those two agencies, for example?

WILLIAMS: Well, I think we expect that we are going to have to interact closely with them. And in our strategic plan that we've been writing, we've been talking about that. In terms of making it happen, I guess I'm going to, well, poor mouth for a minute, basically in 1996, our transportation program was shut down, and we have had basically no money for transportation, and our staff has been three or four people. And this year, we were planning to ramp it up and start things back up, and we haven't yet because we're on a continuing resolution.

But, with the people that we have that are writing together plans, and so forth, we have talked about doing that. And we do maintain contact through interactions like the transportation external coordinating group, which has members from that different community. We had quite a bit of discussion with the DOT while we were going through the site recommendation. We've been talking to the American Association of Railroads about their advanced rail cars, and so forth.

But in terms of us going out and issuing contracts for the development of those sort of things, it hasn't really taken hold yet. But, it's a good point.

NELSON: Nelson, Board.

I just encourage that to be an early start.

WILLIAMS: I agree with you 100 per cent.

CORRADINI: We're going to have to move on. One last question. Thure?

CERLING: Thure Cerling, Board.

It's clear that there's some changes that will have to be made in the infrastructure, both at the shipping end and at the receiving end, and I was just wondering do you envision any major changes in infrastructure that have to be made in between, using existing rail lines and roads, and that sort of thing?

WILLIAMS: Well, I mean, that's something that's going to need to be looked at. In the Eighties, we did what we call the FICA study, I can't remember what it's--Facility Interface Capability Assessment, where we looked at what the situation was around the utilities, and so forth. That's out of date now, and there's some places that you may need bridges upgraded, or you may need rail tracks and the facilities upgraded. But, like I said, we haven't started to do that yet.

At the repository end, we have the ability now to design the capability to do that. So, I think most of the infrastructure upgrades would probably be closer to the utilities, and it's something we haven't taken on yet. But we see the need to do that. We see the need to update those 1980 studies that we did.

CORRADINI: Jeff, thank you very much. We'll move on.

Our next speaker is John Arthur. John was appointed on October 8th of 2002 as Deputy Director for Repository Development. This newly established position in Las Vegas is responsible for licensing and development of the Yucca Mountain site. Previously, Mr. Arthur was manager of the DOE's National Nuclear Security Administration, the Albuquerque Operations Office, which provides oversight of the two national labs, and the nuclear weapons production complex.

Mr. Arthur's management responsibilities also included the transportation of nuclear materials, safeguards and security, nuclear facility construction and environmental management services. Over the past 24 years, Mr. Arthur has served in several senior management positions within the DOE, including Manager of the Waste Isolation Pilot Plant, Manager of the Uranium Mill Tailings Remedial Action Project, and Assistant manager for Environmental Operations and Services at the Albuquerque Operations Office.

Mr. Arthur will summarize for us the status of the

Yucca Mountain project.

ARTHUR: Thank you, Dr. Corradini, and I do look forward to meeting individually and working with the Board as we forge ahead on Yucca Mountain.

I also might say I had the opportunity to first meet Margaret I guess it was about 14 years ago, I'm aging ourselves, on the WIPP program. So, I'm sure pleased to be back in this program to work with her again.

I am very pleased to be here at this time of repository development. My main expertise, as Mike stated, is in the repository development, regulatory compliance, environmental management, and most recently the national security areas. And I can guarantee you with just two months experience on this program, it's going to test every skill I ever had in the Department of Energy and private sector.

As Margaret mentioned earlier, our overall goal at this time and challenge is to change the priorities and operating culture from one of site characterization and site development into licensing, characterizing the additional work, operating a spent nuclear fuel and high-level waste repository. And as Margaret also stated, the first and foremost priority I and our staff have right now is developing a quality license application by December of '04.

As we mentioned, DOE will be the licensee to the Nuclear Regulatory Commission responsible for the program, but we execute this through a major performance contract with Bechtel and SAIC aligned with our other partners in the national laboratories and the U.S. Geological Survey and others.

First, I want to talk about a few things. Focused science and engineering development activities will continue, and as I'll talk, some of those will provide inputs into license. I'll talk a little bit about what flexibilities we have and don't have at this time, but also really focus after licensing to have the Science and Technology Program that Bob Budnitz is leading really provide inputs for the future to make sure we optimize designs, logistics and other things for the future.

I want to talk first of all about management philosophy. First of all, having been an NRC licensee before, both in the Department of Energy and industry, I understand that it's equally important to obtaining and getting the license out in December of '04, we have to have an operating culture and operate like a licensee, which includes a number of activities in the operating environment.

First and foremost is to show and demonstrate our capability to manage the repository program, which includes things like training, qualification of our DOE and contractor individuals, defending the application, as well as knowledge of processes and defensibility of the license.

Quality assurance in every way is the foundation of our licensing process. It will be built into our products and the cornerstone of our licensing documents. And by that, I mean not just employee training, but validation of the various models, all the datasets, and other key areas that are required to support licensing and construction.

Our formality of interactions with NRC is going to continue to increase as we go towards licensing. You mentioned in one of the questions earlier about KTIs. We had a management meeting last week and our areas to continue to focus and work those off and the priority, ways to support the licensing, because there's a lot of issues that will be resolved as we work through those.

Also, I might state that accountability is increasing in this program, not only on things like closure of corrective actions, they're very important. Some have had right management focus, some haven't. So, we're putting more accountability, so on a monthly basis, we can look at our metrics, not just on an organizational basis, but down to individuals as appropriate to keep the focus on working the necessary actions to support licensing.

In the near future in our program, we'll be issuing a strategic plan, and it will have many goals. We are in the process of cascading those goals into manager performance appraisals, and also contractor incentives.

We're also in the process of revising our contract management and project discipline. Just a couple points there. We are, John Mitchell and myself, in February, we'll be going into a series of what I call monthly operating reviews. There will be metrics and performance base. In time, I'll share those, as appropriate, with you all at some of the meetings to show what percent complete we are on our not just overall license application, but also the design aspects of that, all the validation of models and other areas. So, we want to focus monthly on that, as well as some of our site operations and other critical activities.

Also, I might state that as we go through our design, we are trying to benchmark best practices, not just nationally and internationally, it's not just to have it developed here. If there's something new we can apply that in an integrated system is going to work, we're going to look at a way to do it, not just to meet the tech-specs and the license, but performance as cost effectively as possible.

Also, in our organization, I am in the process here in the Las Vegas this week of completing our Office of Repository Development. We have to get our federal team aligned to the positions, also the proper responsibilities to carry out the future. And as I told my federal folks, I know John Mitchell and Bechtel realize that as we go through this program in the next five years, and even ten years, we'll go through different phases where both our federal expertise and contractors need to be flexible as we go through licensing, licensing defense, final design, and construction that will require us to have a lot of dynamics on how we manage our resources. So, that equally is on all of our management screen.

A couple other areas. Communication is a key, and I want to just talk two things. One internally the program, you hear the term a lot "walk the talk." A lot of expectations on how we implement under the NRC license, a safety conscious work environment. We've set expectations of our leadership and employees. John Mitchell, myself and all of our leaders, and I know Margaret is supporting us from Washington, are getting out to meet with the individuals to hear how things are going, to make sure that our expectations are being achieved.

I also have done some things like just e-mails to all employees, individual meetings, and other areas. You just can't focus on it enough. That's important to me, to keep the work force in a quality fashion, as well as to get the license completed.

External communications, not just the Board, but also NRS and others, we'll continue to step that up and have meaningful exchanges. I also might add that in the last week or so, we have revamped our OCRWM website, and right now, we're in the process of consolidating multiple websites. In time, I'd like to put on there our strategic plan that comes out, and also some of our operating metrics, so the public and others can see how well we're performing against the established goals, both the good news, and also where we're having some variances and issues.

A couple last points. I want to, in the next few weeks, get back out to our site again, take a look at the current site infrastructure and make sure that things are up to date, aligned and maintained so we can transition in time as we go through licensing, and also to make sure things like our as built drillings are up to snuff that will be required for a license application for construction.

As Margaret mentioned, we are committed to continuing focused science, various studies to support licensing, and then also the long-term repository performance.

Now, just a few specifics on the NWTRB. I have, in the short time, tried to review some of the most recent reports, recommendations, and our responses back to you, and I do appreciate the importance of the reviews, and also look forward to continuing to try to minimize the various uncertainty in our performance calculations, as well as trying to increase the defensibility-in-depth of our license application, all the performance assessment and calculations supporting that.

In the short time also, I have not had, by no means am I an expertise, I'm still drinking through a big fire hose, but I've had some topical briefings on the order of an hour on some of the issues such as Chlorine-36, the source of moisture in the cross drift, and some of the corrosion studies.

While the science program is separate and distinct from our focused repository development, there will be inputs to support the license. I might just add that the actual license that is being prepared--or excuse me--the design that's being prepared to support the license, we should be completing that in January of '04, so just about a year from now, we'll complete that. So, when we talk, and I heard some of the comments earlier, and I fully agree, some of the key aspects, such as security, some of the operational and logistical areas, we will have some time to do some necessary reviews on that prior to license issuance.

And in that area, I'd like to bring in some of the expertise that we've had, not just throughout some of the areas of support, Homeland Defense, but also national security so we have the right level of approach on not just the repository, but also supporting the transportation management.

So, with that, I will summarize. I'm very pleased to be in this program at this important time, and I look forward to working with the Board, and will be glad to entertain what questions you might have. Thank you.

CORRADINI: Questions? Mark.

ABKOWITZ: Abkowitz, Board.

John, first of all, welcome. I look forward to working with you.

I was curious with your background in WIPP and now moving into the position you're in now, if you could comment on some of the lessons that you learned from WIPP that you think are transferrable, and some of the unique characteristics you're facing that are unlike your experience at WIPP.

ARTHUR: A lot of things are similar. I know one of the thing, and I told Margaret and other people when I joined the program, said be real careful not to bring WIPP experiences in because they were different programs. But a lot of similarities in some of the performance, even though we have different regulatory bases on that, there are a lot of similarities.

I think a couple things when we look at a long-term vision for this repository, if you look back on WIPP, we went through multiple phases. And one of the areas, a tactical error we made early on was, you know, we changed through areas just to get some waste in the underground, to really achieving the ultimate vision, which was to have enduring operations. So, I want to make sure, and I think Jeff touched on this, that while we have transportation, a repository program, and many other aspects, we need to do a lot of systems engineering and other evaluations to make sure it operates as a system.

And WIPP, in the early days, we probably didn't do that as well as we could. And, again, the goal is for enduring operations, not to get the material in and then come to a stop. I mean, we want to make sure we have flexibility in our operations.

I think the other areas, and there's actually a very good book out, I don't know if the members have had a chance, it's by Chuck McCutchen, actually, that worked for the Albuquerque Journal, and he actually wrote on WIPP for a long period of time, and he actually summarized lessons learned over WIPP, things they could have done better. One of the areas was to continue to work with all the key stakeholders early on.

If you look at this program, we're going to go through a lot of changes over the next five or ten years, as I mentioned, licensing, construction, into repository operations, and to make sure that we're working aggressively not only with the state, but the counties, to have relationships for the future, to make sure that we forge ahead. In WIPP, some areas we delayed too long, and we ended up having problems later.

And I think the other area that we've learned that can be applied to this program is in transportation. I mean, I've seen, and I'm by no means an expert on all of them, but from WIPP, also what we're doing in our national security with nuclear weapons and other transportation, we need to make sure we bring in all the expertise to help us build systems for the future, not just to have the right logistics, but the right degree of security for this program.

I have high confidence we can operate it very safely and securely, but we need to make sure we build that in early in our planning. I could probably go on for hours, but I would encourage you to look at that book. It's very good. It's a good summary. I had our managers read it on the program.

CORRADINI: Priscilla, and then Dan.

NELSON: Nelson, Board.

You may not have formed an idea of this, but one of these areas that the Board has been wrangling with, and I know the project has as well, is how to create and communicate the action of the natural system and the engineered system. And I think it's one area where WIPP and Yucca Mountain are quite different as they're configured now. And I'm not sure that there's been complete success satisfactorily on how to talk about that, and I'm wondering if you have any thoughts there. How high a priority would you say that is for the project to find a way to express, measure, communicate these contributions?

ARTHUR: I can give you just based on limited experience, and in time, I'll try to cover it more, but we are trying to, and even in the areas of the natural system, as well as engineering areas, is take a look first of all at performance assessment. Again, somewhat similar, but some differing approaches in how we do a PA on the program, but to bring in expertise, you know, from WIPP to assist us in some of our reviews as we do the performance assessment here.

And I do think it's important to communicate that. We just need to have the right tools, and I do want to have some further discussions with our people as we proceed. So, it's just based on a limited time, but it is important. I agree with you, Priscilla.

NELSON: Nelson, Board.

Is this a priority for the project? And, if so, in what kind of a time framework would it be a high priority to actually figure out how to tell the story?

ARTHUR: I'll have to get some specifics. As far as committing to a schedule in the short time, I just really can't, but I will look into it and either have one of our people today or myself get back to you. I wish I could get my arms around all this in this time, but it's important, and I'll get back to you.

CORRADINI: Dan?

BULLEN: Bullen, Board.

You mentioned a strategic plan that's going to be issued near term. Could you expound a little bit about that, tell us maybe the time frame for the plan and plans for revision of it? I mean, it's got to be a living document if you're going to have a long-range goal. Could you give us a little bit of background on that, please?

ARTHUR: I can talk, and Margaret can nod if I'm on the right track or not. But when I first got involved in the program, I guess it was back before I actually came out here, we had a leadership retreat in October, and I was very pleased, I mean, there was the initial architecture for strategic plan and I think with Margaret coming on, myself and some new leaders in the program, we wanted to actually take some ownership of that before it came out. So, what we've done over the last month or so is get comments from our team members to make sure, because we do have high-level goals in there, and it includes, you know, emphasizing the importance of science and technology, systems engineering, and other areas.

We're in the process of doing our final reviews, and I would anticipate it would be sometime in early

February, or actually middle February we'll have that out. And we'll post it on the website when that comes out also.

BULLEN: Bullen, Board.

I would like to see that. One last follow-up question with respect to your site infrastructure and your visits to the site. I guess the question is do you have any water out there, and do you expect to have water with respect to the State's permit?

ARTHUR: Well, we do have water. And right after the holidays, we're very pleased, but we were able to actually get potable water for our workers, so things, for the time being, are proceeding okay.

BULLEN: Bullen, Board.

Do you have water to continue the experiments that you need?

ARTHUR: We do have temporarily some, but we do have some issues we're still working on, and I'll leave it at that.

BULLEN: Thank you.

ARTHUR: I do know the importance to have that.

CORRADINI: Other questions?

(No response.)

CORRADINI: Thank you very much.

ARTHUR: Thank you very much.

CORRADINI: I think we're on break. We'll be back at 10 o'clock. Thank you.

(Whereupon, a recess was taken.)

CORRADINI: Our final speaker of the morning is Mark Peters. Mark is responsible for science and engineering testing within the Performance Assessment Organization of Bechtel/SAIC. Formerly, he was responsible for the technical integration of science, construction and design organizations and scientific technical leads engaged in the field testing at Yucca Mountain. Earlier, Dr. Peters was technical lead for thermal analysis.

Mark?

PETERS: Thank you.

CORRADINI: He will talk to us about science and engineering update at the project.

PETERS: Thank you very much. Thank you for having me back to speak to the Board. It's always an honor to speak to the Board. I only have ten minutes; right? I realize there's a lot of slides. Well, I've got back-up, in my defense, but there's still a lot to go through.

Science and engineering update. Let me be clear on what this is. It's similar to what I've provided to the Board in the past. It's really an update of the testing analysis program. I won't discuss design activities per se, but I'll focus on the technical program, and it will be a walk-through the status of the program. I tried to structure it by walking through the natural system and engineered system. This is the work of many people. I simply am summarizing that. I'll try to credit people as I go through.

Overview, again I've already said this, I'm going to start with status of the unsaturated zone program, starting with the ESF, exploratory studies facility, moving through the drift scale test, an update on Chlorine-36 validation that I know is of much interest to the Board, summarize some USGS work in the area of fracture minerals, inclusions, then move into the cross drift, staying in the underground, still unsaturated zone, and talk about some of the testing, particularly focused on flow and seepage in the repository horizon, stop and then move below the repository horizon to the Busted Butte tests we're looking at, flow and transport through the Calico Hills units.

Finally, the saturated zone. Here, our work is done very closely in cooperation with the Nye County Drilling program. I'll move then into an overview of what's going on in the volcanism area, and then jump over to what I called engineered barrier system. Clearly, some of this work feeds both natural system and engineered system models. Here, I'll move back into the field underground, and talk about thermal properties work that we're doing in the underground as well in the laboratory, and then mechanical properties investigations that is again a combined field/laboratory program.

Then, I'll discuss briefly a couple of investigations that integrate in with the environment in the drift. I will point heavily to what you're going to hear a lot more about this afternoon when I talk about this area. And then, finally, a very brief overview of what's going on in the waste form testing and analysis area, and then wrap up.

The first slide, just to get you oriented, a layout of the exploratory studies facility, the north portal here, the south portal here. I think everybody is very familiar with the five mile loop that is the ESF. This shows the locations of the alcoves and niches in the exploratory studies facility, as well as the red here is the cross drift. I will also talk about results from some of the testing in that area later in the talk.

For the purpose of the ESF piece, I'll focus primarily on Alcove 5, where we've completed the drift scale test, and then also Chlorine-36 validation, where we've looked at attempting to validate observations of apparent bomb pulse Chlorine-36 at two locations in the ESF, the first being the Sundance Fault area down here by Alcove 6, and the second being the Drillhole Wash Fault as exposed up here near the turnoff of the cross drift.

Starting with the drift scale test, I've added a few more slides than I typically have for the drift scale test to bring you up to date on a little bit more of what we've done in this test. This is a diagram of the drift scale test showing schematically the boreholes. You have an observation drift, a connecting drift, and then an approximately 50 meter long heated tunnel where we've got nine large mock waste canisters inside the tunnel, as well as 25 wing heaters on each side. These are heaters installed in the rock.

The boreholes shown in blue and brown are drilled above and below the drift, primarily looking at moisture redistribution as a function of heating, and now cooling, and boreholes drilled within the drift itself are primarily for temperature control, temperature measurements, as well as mechanical displacement measurements.

We turned off the heaters, as I think the Board is aware, a little over a year ago. So, we're a year into the cooling phase. This is a diagram showing that the power has, in fact, been turned off, turned to zero. We went into a natural cooling phase last January 14th, I believe it was.

The drift wall temperature, this is a representative thermal couple at the drift wall. The actual temperature now, if you were to go out there, is actually just below boiling. So, this is slightly out of date in terms of up to today. I think the current temperature again is, at the drift wall representative thermal couple is about 97 cesium.

These bullets summarize what I'm going to go through in the following plots that summarizes some of the observations that we've made, as well as how it compares to predictions. The first point, I'm going to show an example of temperatures in the test block, and how we're getting cooling in the drift wall, and then do the conduction. We continue to see small rises in temperature away from the drift wall, and they're converging and eventually the whole system will start to cool uniformly.

We did see evidence of heat pipes or convective effects at the boiling point in a lot of our boreholes. We've now seen a disappearance of those heat pipe signatures as we've cooled.

A little bit of geochemistry. CO2 concentrations in the gas phase. Gas phase continue to change, and that's consistent with what we're predicting from our models. We're also modelling fracture saturations within the fractures, and we use air permeability as a means of attempting to estimate fracture saturation. And I'll show a plot on that as well. And that also alludes to the final bullet there where we have to worry about being able to back out mechanical versus hydrological effects and changes in fracture saturation.

An example of how the temperatures continue to rise in the test block. This is one borehole, a down looking borehole within the heated drift, about halfway down the drift. The different lines are actually time histories of a given temperature sensor as a function of depth in the borehole. As you can see here, near the surface of the drift, near the drift wall, versus deep in the rock, as much as 15 to 20 meters into the rock, you see the temperatures gradually continue to rise.

This set of slides compares two time slices, one at the end of the heating phase, and the other six months after cooling, so, this would have been last summer, showing data for three boreholes drilled in the heated drift, one up, and then one at a 45 degree angle, so, one up from the drift, one at a 45 degree angle, and one horizontal off the drift, showing data and predictions. This shows the evidence of the heat pipe effects, the convective effects. This particular, the red here is actually along a wing heater. That's why you see such high temperatures. These other two are away from wing heaters, again, in the roof of the drift. But the take-home point here is the data and how we're comparing with the predictions in terms of temperature, evolution, and also the fact that as we've cooled, we've lost this evidence of the heat pipe or convective effects. As the water is draining, vapor is diminished in the vapor phase.

This gets back to, and I've made a couple changes that aren't in your copy here to point out to you, I changed the number 74-4 and 76-3, so you might want to mark those in your copy. There was a typo there that I picked up. But this is getting at the air permeability data. We go in and do periodic air permeability measurements. That provides us information on the evolution of fracture saturation. But you also have to back out the mechanical effects. Any effects of expansion and contraction along the fractures could, in fact, change the air permeability.

So, this shows one array from the observation drift. The color codes here show the air permeability distribution prior to even turning on the heaters. And on the left is two of those integrals, 74-4, shown relative to the heated drift, and 76-3, shown relative to the heated drift. This is data shown in the triangles, and different predictions accounting for hydrologic, mechanical and then hydrologic mechanical coupled effects, and how well we're predicting evolution of fracture saturation as a function of time.

Skipping now over to some observations that we've made in terms of water that we've collected from some of the boreholes in the drift scale test. I think it would be a year ago about this time, we had presented some results of some water chemistries, waters that we had collected from the drift scale test that had very high chloride contents, and that caused some significant pause. We really, really aggressively went out there while we were seeing those kinds of high chloride concentrations. We determined in a very short time period through some laboratory experiments and additional field experiments that that was due, we attributed that to degradation of packing material or testing apparatus material that had been put into the block.

Since that time, we've also collected some additional water from another borehole that's shown again what we consider unexpected water chemistries. They were very dark yellow colored, had very high conductivities, and they contained high concentrations of transition metals in particular.

We went through a similar investigation. We immediately suspected that it had to do with something that we had introduced into the test block. In fact, we've gone through a very similar process that we followed with the high chloride waters, and have determined that's likely due to thermal degradation of actually neoprene, or tubing that we've introduced that was used for injection of air, and also for collection of the water and gas.

Our lesson learned here is very similar to what we've had with the fluoride. We need to be very careful about what we introduce into the system. We have a process set up for managing that. Clearly, we weren't totally successful when we first instrumented the drift scale test. So, this is very a important consideration as you move into the repository. You do not want to put things into the repository that could produce water chemistries that we don't expect, and potentially would be deleterious.

So, that's what's out there. Again, the lab experiments have shown that the neoprene does break down at relatively low temperatures.

Another observation that we've made in the drift scale test that you may or may not have heard about. We have a camera that we run in periodically along the roof that had infra-red and video capabilities. And on top of one of the canisters, the mock canisters, approximately a little over halfway back from the bulkhead, we saw a red spot, it looked like a rust spot on one of the canisters. And that was something that caught our attention. This was observed shortly after we turned off the heaters, so we immediately were interested in whether that represented some kind of dripping back into the drift as we were cooling.

We've gone in and we've actually modified the system to be able to go in and take a sample of the material, and it's mostly iron oxide. It happens to sit below a rock bolt, so we think that this is likely discrete flow back into the drift along that rock bolt. We're trying to collect additional information, and we continue to run the camera in and out looking for that kind of evidence in other parts of the drift, and we're also doing some modelling to account for the effect of the rock bolt boreholes and how that might effect near drift thermal seepage.

Moving to Chlorine-36 validation. I probably don't need to belabor the objective here. Again, in the '96, '97 time frame, Los Alamos National Laboratory collected a significant dataset from the exploratory studies facility that suggested that there was bomb pulse Chlorine-36 exposed in the northern part of the ESF and observed in the repository horizon.

That's a very important observation. It's accounted for in our conceptual models for flow. The DOE made a decision in the later Nineties, because of the importance of this observation, to go in and attempt to validate those observations. So, there was an independent team set up. Los Alamos was still involved in terms of analyzing some of the splits, but the USGS and Lawrence Livermore put together a program to go in and take independent samples and validate the observations of bomb pulse Chlorine-36.

As the Board is very aware, I've been working on this now for a couple years, and we continue to have differences between what the USGS, Livermore dataset looks like versus the Los Alamos dataset. Los Alamos continues to be internally consistent and reproduce their previous observations. We have that discrepancy between the two laboratories, the two groups.

The team, USGS, Los Alamos and Livermore, are currently writing up what we've done to date. That will include their perspective on the sorts of experiments that one could go do to resolve the issue.

This bullet here is something that I said at the last meeting that I just want to reiterate. Our current conceptual model for UZ flow, the UZ process and TSPA models don't rely directly on the bomb pulse Chlorine-36 data, but they do respect it, and at this time, we do not plan to modify any of our conceptual models based on the discrepancy between the datasets.

The bottom bullet as well I really want you to take home here. DOE is pursuing an independent study, meaning we're looking for a completely independent party to go in and set up a sampling and analysis program to further investigate Chlorine-36 to chloride systematics, completely independent meaning not involved in peer reviews in the past, not part of this team, et cetera, et cetera. So, we feel it important, we understand the Board's concerns, and DOE is also concerned. So, we are pursuing that as an option.

Moving now to secondary fracture minerals, this is work that Zell Peterman and his co-workers at the U.S.

Geological Survey at Denver are heavily involved in. Here, we're looking at great stage fracture minerals as exposed in the Topopah Spring in particular, and this is work that's been going on for several years. You've heard about this in the past. But, again, the objectives here are to look for evidence of how fast the fracture minerals have been growing in the UZ as a way to establish linkage between how the climates vary and how that compares to long-term average percolation flux. It's an independent line of evidence that gets at how well we're estimating current as well as long-term percolation flux. So, it adds confidence to those UZ flow and transport models again.

Let me back up. There's quite a bit of backup on almost all these subjects in my presentation. I won't point to it, but it's obviously fair game if you want to get into questions. That might help me answer some of the questions.

This is an example of the work at the U.S. Geological Survey where they're using ion-probe techniques to actually date at a very small scale opal, opal within the fractures. Opal co-exists with the calcite. This is just an example of some of that data. Here's an opal here. The scale here is on the order of millimeters, if I remember correctly.

But, this just shows the individual data points, and next to it is actually ages in hundreds of thousands of years, with the air bars on that. And from that, they can then fit and make an estimate of growth rates through the Pleistocene over the past one and a half million years, and you can see there's very small, less than a micron per thousand years of growth in these fracture minerals.

These are consistent, continue to be consistent with the long-term average percolation flux, current time at one to ten millimeters per year within the repository horizon.

Still focusing on U.S. Geological Survey work, here moving over into summarizing a lot of what you already heard from the fluid inclusion studies that have been going on by DOE for several years, and also been more focused on in the past three or four years. The USGS did work cooperatively with Jean Cline's study on fluid inclusion, shared samples, collected additional samples of their own to look at the timing and distribution of the fluid inclusions.

The conclusions that they've come up with are consistent with DOE conclusions in the past. The fluid inclusions in the calcite were two faced fluid inclusions, indicate that they've been deposited over temperatures from as high as 90 degrees C. to ambient. There's a relationship between the high temperatures and being in the older parts of the deposits.

There's also a dataset on oxygen isotopes in the calcite that correlate with and corroborate fluid inclusion information. And, finally, the final conclusion, again, this is no different than we've concluded in the past, the fluid inclusion in the oxygen isotope data suggests that we've at ambient temperatures for the past two to four million years within the UZ.

Still focusing on U.S. Geological Survey work, and focusing more on water/rock interaction, and back to understanding the long-term percolation flux and how much water has flowed through the UZ over time, the USGS has also put together a very nice program looking at uranium series isotopes from pore salts. So, they flush, simplistically they flush the rock and analyze the pore salts. You can do it with strontium isotopes. I'm going to talk today about the uranium series work that they've done.

But you can look at whether or not the U-series is an equilibrium or disequilibrium, and that tells you something about the hydrologic conditions. It's a function of how much water has flowed through the system, and it integrates those facts throughout time.

So, it's another independent line of evidence that builds confidence in the UZ flow and transport model, we hope builds confidence in the UZ flow and transport model. In this particular case, it does in fact build confidence.

There's a slide in my backup that shows some of the data. But to think about it, we've really looked at two situations. One, samples from the proposed repository horizon away from faults, and we've also looked at samples within faults, and in this particular case, within the Bow Ridge Fault that's exposed up by Alcove 2.

These bullets here kind of wrap up what we would expect to see. Going in, if you look at a deep rock away from a fault that hasn't had a lot of water flowing through it, you'd expect to see basically equilibrium between the uranium and thorium isotopes, activity ratios of about one. In the data in the back, you'll see that's consistent with what we've seen so far from the Topopah Spring samples away from faults.

If you go to a faulted area where you've had focused flow and larger amounts of water flowing through the fault, you'd expect to see some disequilibrium, ratios greater than one, maybe as high as six, seven and eight, in terms of activity ratios. And that's actually consistent with some very preliminary results from the Bow Ridge Fault samples. This work is continuing. At this point, I would call this preliminary work. But it's consistent with what we expected the system to tell us.

Still focusing on U.S. Geological Survey work, and now switching gears over to the geochemistry of the pore water. As you're aware, pore water compositions in the unsaturated zone are key as the starting point to understanding how the water chemistry will evolve in the rock, but also within the drift.

There is water contained in the rock in the welded tuffs. The evaporation effects, and I'm going to point a lot to Joe Farmer's presentation this afternoon, he'll talk a lot more about how this, I'll call boundary condition or input, combines into our overall picture of the waste package environment. But I wanted to at least show this data, and I think it will dovetail nicely with what Joe is going to tell you. But we need to know the pore water compositions to understand the hydrologic system.

How do we get the pore water out? It's not actually a simple, not just a matter of pulling it out and saying okay, here's some water. The non-welded tuffs, we can actually squeeze them, or actually put them in a vacuum and freeze the water, move it around by cold traps, and extract the water, and you can get very good recovery.

The welded tuffs, because they hold onto the water really tight in the matrix, you can't actually squeeze or freeze it out. So, you have to spin it in an ultracentrifuge. The USGS has a centrifuge, and this is work that's really come on line in the past couple years. So, it's very important observations, but it's not a straightforward technique to actually get that water out of the welded tuff.

I just want to make that point. I mean, we feel very good that we're getting very meaningful information. But it's not a straightforward extraction.

Once we do get the water out, we do a series of chemical and isotopic analyses. And the next slide will summarize some of those observations. This is the Y-axis. I lost my label somewhere along the line. This is in milligrams per liter on the Y. This just shows various elements, alkali earths, alkalies, as well as some of the key anions, sulfate, nitrate, chloride, fluoride. Manganese and strontium are shown here. This is actually in micrograms per liter. We've multiplied by 1,000. Zell multiplied by 1,000 so he could get them on the same scale.

This just shows the variability that we've seen in this case 28 samples from the Topopah and the cross drift, the medians as well as the tails of our observations.

Moving into the cross drift, some of the work--let me back up--some of the work that I alluded to in the USGS section has come from samples in the cross drift, but it's all coming from the underground program. I'm going to give a very brief discussion of what's been going on with the U.S. Bureau of Reclamation, Steve Beeson's folks, looking at fracture and lithophysal distributions in the Topopah as exposed in the cross drift. That was discussed at some length at the last meeting during my presentation.

I'm also going to briefly give you an overview of our recent observations from Alcove 8, the drift to drift test, and also talk about preliminary results from our seepage tests in Niche 5, and then an overview of the observations from the systematic seepage test that we've done in boreholes along the cross drift. And, finally, I discuss where we're at with the bulkhead experiments.

Just to recall, the bulkhead experiments, we had four bulkheads set up in the ECRB in the cross drift, and we are not ventilating the whole back half of the cross drift. This section has actually been ventilated now for on the order of five to six months because we've been doing some drilling back in here for other programs. But, from the second bulkhead all the way back, it continues to be unventilated.

So, first the lithophysal fracture studies, there's two backup slides that show some results of variation of percentages of the lithophysal, abundance and size, et cetera, as well as fracture density in the backup. But this is very closely linked and integrated with the work that's going on in thermal properties and mechanical properties that I'll allude to later in the presentation.

It's a very important link. They're collecting the information in particular at the locations where we're doing those thermal and mechanical tests, in particular. They're using a variety of methods. They used visual estimates when they did the initial mapping of the cross drift. They're looking at photomosaics. They're doing detailed traverses, and also doing surveys of the larger lithophysal cavities, meaning greater than 50 centimeters.

This is just a cartoon-like figure that shows when I talk about lithophysae versus spots versus vapor pathways versus fractures, this is a picture to lay out kind of the nomenclature of what we're talking about. Sometimes geologists get wrapped up in all these cool words, and it might, for the non-geologists in the crowd, this might help you all in terms of decoding.

The lithophysal cavities are the openings of the cavities. The spots look like cavities, except they're filled with stuff. You've got fracture. In the lower lithophysal, the fractures tend to be short and they tend to terminate in the lithophysal cavities, these cavities. In the non-lithophysal units, they tend to be longer fractures, and you don't have as many cavities.

So, if you've been down there, and I know a lot of the Board we just took down there, there's striking differences between the different units in the Topopah in terms of the abundances and character of these things, and that's important to understand for hydrology as well as mechanical and thermal properties.

Moving now to hydrology in the cross drift, I'm going to start with Alcove 8. This is a test that we're doing where we have Alcove 8 in the cross drift. Recall that the cross drift goes over top of the ESF. There's about 18 meters of difference between the two in elevation, and we're taking advantage of that geometry and doing a drift to drift test, and we're evaluating flow and seepage here at the scale in tens of meters. It's a great experiment for evaluating scale and effects, and it's supporting the seepage and transport models.

This is just a schematic of that test. Again, the cross drift here with Alcove 8 coming off, ESF underneath, and Niche 3 here. We have down-looking and up-looking boreholes that are used for real time measurements, real time active measurements of changes in moisture, looking for the moisture front. We're actually ponding water now in an infiltration plot on the floor of Alcove 8, and seeing how much water we collect in the niche underneath.

If you recall a couple meetings back, we had done an experiment along a fault as exposed in the back of the alcove. I talked to you all about that. I'm going to focus now on our more recent experiments. Recall though in that fault experiment, we started with lithium bromide on the order of ten parts per million, and then we added higher concentrations of lithium bromide, and also added fluorobenzoics and other types of tracers to look at matrix diffusion effects.

We're planning to do the same sorts of things in the large plot experiment. But where we're at right now is we're still just applying water with approximately ten parts per million of lithium bromide. It's an infiltration plot. The Board also saw this on a tour when we were out there in September. But, we have a large plot here, twelve separate zones. We're applying water. Here's the cumulative application of water on that plot since we started the test back in August.

And the following plot shows as a function of time how much water we collected in Niche 3 below. This is preliminary information. You can see we actually saw break-through in less than a month, meaning we started the application and we saw water, if I remember correctly, the water broke through just about the time that I was up here talking to you at the last meeting. And it was faster, I won't say it was faster than we expected, when we were excavating the alcove, we used water to control dust, and we actually saw a wet spot in the niche underneath as we were constructing. So, we expected to see some evidence of relatively fast flow along some connected pathway between the alcove and the niche. So, the break-through wasn't terribly surprising.

We're in the process of modelling kind of an as-built of the block. In other words, we had introduced water during excavation, and we also introduced water during the fault tracer experiment. So, we're in the process of modelling those results, but talking to the principal investigators and the modelers, they're not surprised by any of these results.

Moving to seepage models, and input into those seepage models. Recall that we've done a series of niche tests where we've constructed a small--alcoves and niches are basically the same thing, just a little different size. We've done four niche experiments in the middle non-lithophysal exposed in the ESF, and we've got one niche excavated inside in the cross drift in the lower lithophysal.

And, here, as opposed to the Alcove 8 and Niche 3 experiment where we're able to look at flow and seepage over a much larger scale, here we're getting a drift scale seepage at the scale of meters. We're injecting water into boreholes above the niche, and then quantifying how much, if any, water drips into the opening itself.

This is a picture that may or may not be terribly informative, but I'll give it a shot. This is actually, the drawing is probably more informative, this is looking down Niche 5. And one of the things that you can imagine would be an issue or something we would have to address would be the mass balance. If you drip water in a borehole above, if you don't see it in the opening, okay, great, where did it go?

So, what we've done is we've excavated--this is difficult rock to cut these kind of slots in. We tried to excavate a slot to actually quantify, so if water did not drip in, we would be able to try to get a closer to 100 mass balance and collect the water that was diverted around the opening.

These letters here are going to mean something in the next figure. What we've done is we've done a second, and now we're at a third phase of seepage experiments, and this data is real time being incorporated into the calibration and validation of the seepage model for the license application.

An example of one of the seepage tests in Niche 5, a little confusing on the axes, so bear with me for a second. This is time. We're plotting two different things. We're plotting release, which is how much water we released in the borehole as a function of time, and that's on this scale over here on the right.

We're also plotting how much we collected, A plus D is the total amount of seepage that we collected in the slot, as well as from the roof, so, on the sides and the roof. The tarp seepage is how much we collected on the side.

But the take-home point here is the difference between these two is the seepage threshold, or how well the drift is acting as a barrier to water dripping in. So, this is very recent results, and again being incorporated and being used as calibration and validation of the seepage model.

NELSON: Mark, what is the left scale?

PETERS: The left scale corresponds to the seepage rate here. So, I'm sorry, I know it's confusing. This scale applies to these data. This scale applies to these data. We've introduced a total amount of water, and we're collecting some of it. Okay? That's a little confusing, but they couldn't really show this data on this scale because its number is so small. That's an important point, very low seepage flow.

So, this kind of wraps up what I've already said. The data is being used in support of the drift scale seepage model. They've continued to demonstrate that a capillary barrier exists.

One of the interesting things is they didn't see a lot of active dripping water into the slot, but they have photographs that unfortunately didn't project very well, but nonetheless, they show liquid above the ceiling, and it actually reaches the wall, and they seem to see evidence of flow wall to wall. So, it's not dripping, but it's flowing along the wall.

LATANISION: A slot being what, a short circuited--

PETERS: No, if you back up to--this is the slot, that thing that we excavated off to the side to try to increase our ability to collect water that was diverted. Does that make sense?

LATANISION: Yes.

CORRADINI: All right, so let's go back. There's got to be a mass flux of water where this phenomenon would stop. Have you thought about theoretically when that would be?

PETERS: Yes, that's what the threshold concept is. Basically, they--I say they, Berkeley is doing a lot of this work. You can think about it in terms of there's a percolation flux below which you will get no seepage, meaning you have to have a lot of water flowing through the system in order to overcome the capillary barrier to get dripping in. That's the seepage threshold concept.

So, they've actually, if Bo was up here, he would be able to talk much more authoritatively about it, but he can talk you through an argument where depending upon what part of the Topopah you're in, there's a given flux below which you will get no seepage.

CORRADINI: So, that's being considered or being formulated?

PETERS: Right. But if you go to the TSPA, and Peter will be able to speak to this much better than me, but if you look at the TSPA, at least for SR, I think 20 per cent of the drifts saw seepage, on the order. I was answering it from a process level. But when you go to the TSPA, there's a more conservative approach.

Still on seepage, but here getting at heterogeneity, you can have niches at different locations. But one of the aspects of the system is clearly it's heterogeneous. So, we've also set up a program, this is again Lawrence Berkeley investigators that are primarily conducting this work where we've drilled systematic boreholes along the cross drift in the lower lithophysal piece of the Topopah, and we're doing systematic, again, liquid release experiments, but along the up and down dip of the lower lith. And we're doing air permeability experiments as well as liquid release experiments.

Some bullets that summarize our observations from those. It talks more about variability in fracture properties and seepage. We see varying response. Some of the locations you don't see water enter the formation. In some cases, you get complete diversion, and in other cases, you get limited seepage. But our bottom line conclusions to date are there's discrete preferential flow paths, the small fractures and lithophysal cavities, and again, I'm in the lower lithophysal here. The lithophysal porosity doesn't have a large participation in the liquid flow paths. And, finally, this is all about variability and heterogeneity, and this is being addressed within the drift scale seepage model. So, this data is also being used in support of the seepage models.

Switching to bulkhead investigations, I pointed out already where we bulkheaded off the back half of the drift. This is another area that I know the Board is very interested in how we're doing in this particular testing analysis program. Again, we've isolated the back half, no ventilation, looking for return to in situ conditions, and any evidence of seepage. That's what our initial objective was.

Our objectives have evolved because recall we've seen evidence of moisture buildup in different sections behind those bulkheads as a function of time. We continue to feel very strongly, based on multiple lines of evidence, what water we've collected and the chemical analysis of that water, as well as how the moisture is distributed when you go back and look in that drift, when you open up the doors, it suggests that condensation is the dominant phenomenon.

We continue, we're going to collect additional water here probably in the next month or so when we go back into those three sections that are still unventilated. That will continue to address this hypothesis. We feel strongly we'll continue to see evidence of condensation, but the data will tell.

Also, we've got a modelling program that's started to compare our observations to what we expect in terms of our analysis and modelling of the system. So, looking at seepage in the rock as well as in-drift processes. So, this is a work in progress. We put a priority on it. We understand the Board's concerns with this area.

Moving now, still in the unsaturated zone, but below the repository horizon to the Busted Butte experiment, which was conducted a little bit southeast of Yucca Mountain proper. Here, we've done a large scale injection experiment using a variety of tracers looking for flow and transport, looking at flow and transport processes in the Calico Hills unit, the bedded unit, equivalent to what is below the repository horizon.

Here, we're looking at a variety of aspects of the flow and transport model in the unsaturated zone. In particular, I'd like to emphasize the fact that we're looking at scale and comparing laboratory sorption measurements to what you see in the field.

Some of the goals, this is two bullets that restate what I've already said, adding confidence to our site-scale predictions. That's our goal here.

One example of what we've seen, and I apologize, I noticed that the projection, my fault, I lost a couple arrows, so let me walk through what you're look at here. The take-home point with this is as you look at the details of the test bed, there's actually small innerlayers of pumice and ash within the units that have produced some interesting observations.

What you've got here is a picture of the injection face. So, I'm looking down, looking at the face of a rock. These holes here are actually several meter long boreholes where we've got injected tracers, and then the dotted lines here are off of a perpendicular face. We've drilled holes where we're collecting the water and analyzing the tracers.

So, what you're looking at is normalized concentration in all three of these plots, first is distance down the borehole. This particular one is from Borehole 16 here. This particular one, and this is where I lost an arrow, comes from the array of 12, 13, 14, 15. And this bottom one here comes from 9, 46, 48. Okay, so I lost two arrows, there and there. I apologize for that.

But, the take-home point here is you've got a pumice layer here, an ash layer here. Look at the difference between normalized concentration between here and here, showing the effects of that pumice layer. So, there's some interesting permeability contrasts in the system, and what we're seeing in terms of tracer break-through suggests a strong role for those interfaces in terms of how the break-through has occurred within the block. So, that's being incorporated into the test specific model for Busted Butte.

In general, broad conclusion, the Busted Butte test shows that rocks behave similarly in terms of capillary, in terms of hydrology. The permeability contrasts and the boundaries are important in terms of transport. They seem to be more important than the fractures, at least at the Busted Butte experiment.

The experiment is consistent with results to date, and the modelling we've done is consistent with our current conceptual model for flow and transport through the Calico. And, finally, it supports the modelling parameters in the site-scale model.

Our saturated zone program again works very closely with the Nye County Early Warning Drilling Program. We have a whole series of objectives that we have to support our saturated zone flow and transport model, collecting lithologic data, hydrologic data, water levels, also doing hydrologic testing, collecting samples, and doing laboratory sorption experiments. The U.S. Geological Survey does a lot of work collecting water and doing hydrochemical analyses. And, finally, also continue to have plans to do a large scale alluvial tracer test. That's pending resolution of some of the water issues.

I'm going to talk today briefly about work that the

Survey has done in the area of lithostratigraphy, as well as hydrochemistry, and also talk briefly about some results from Los Alamos, Paul Anderson and his folks, laboratory sorption measurements in alluvium.

Back up real quick. This shows the locations of the Phase 1, 2 and 3 Nye County boreholes. Nye County is not going to be speaking to you all today, but I think you're aware that they're just in the midst of finishing up Phase 4 of their drilling program. And, so, in order to put it in context, this is a separate diagram that shows the location of the three Phase 4 wells that have been drilled to date. Yucca Mountain is up here. This is Lathrop Wells here, to get you oriented.

So, in terms of the lithologic, lithostratigrapic work, there's been cross sections built both north, south, east, west cross-sections that Rick Spangler and his folks have done that were originally constructed using Phases 1 and 2 data. They have now been updated with the Phase 3 data, and they're in the process of collecting information from the Phase 4 holes, and that will eventually be incorporated into these cross sections. These are being used as corroborative information to the framework, the hydrologic framework.

Just I hope a pretty picture. What they're doing with the boreholes, this is one example from Nye County 27P. They are actually using a really interesting technology where they're taking digital images of the borehole walls. They can then take those, they're digitized images, and they can do really a lot of great analysis in terms of fractures and dips and all those sorts of things in the laboratory. So, this is just an example of one of those digital images. That's actually from the core.

Moving into hydrochemistry, this is work that Gary Patterson and his colleagues at the USGS have been doing for several years. The idea here is we use a substantial amount of data for calibration of the SZ flow fields, and other pieces of data are used for either validation or corroboration of the flow fields. But, it's very useful information for looking at the different I'll call them hydrologic domains or facies within the system, and it gives us a real good idea of how much variability there is not only in 2-D, but also in 3-D.

So, where is the data coming from? They continue to sample the Nye County wells. As they're drilled, we go in and take samples. When the Inyo County program gets started drilling further down gradient near the Funeral Mountains, they will also be collecting water from those wells, and they're also taking advantage of other sampling programs on the test site, as well as in Amargosa Valley.

What they discovered is if you look at the hydrochemistry data, you can actually break up the system into what they call hydrochemical facies. And these are consistent with the hydrogeologic framework domains that are used in our saturated zone model. But this just lists out the different hydrochemical facies that are identified based on the hydrochemistry data.

You've got the Yucca Mountain volcanic aquifer. You've got the Fortymile Wash system as you go to the southbound gradient. You've got the Norwegianal aquifer, Bare Mountain here, the Amargosa River, and finally the way down gradient Eastern Amargosa Valley where you transition from volcanic to alluvial aquifers. So, these are, again, consistent with our framework of our SZ model.

Moving now to alluvium sorption, this is a diagram that we've used in the past to lay out the location of the alluvial testing complex, but it's a good way to lay a framework. We've actually collected alluvium samples from 19-D, Borehole 19-D, and I'm going to show a couple of follow-on figures where we've done some laboratory experiments looking at sorption, colloids, as well as neptunium and uranium and alluvium.

A very busy diagram. This is talking about colloid transport. We've got normalized concentration versus time. Here, we're looking at two columns filled with alluvium. You put natural colloids as well as microspheres, and plutonium is actually associated with the colloids, and you've also got tritiated water. And these are showing break-throughs as a function of time for all these components.

The conclusions are at the bottom. You can see the natural colloids are actually unretarded in the alluvium. The plutonium that was sorbed on the colloids was recovered. There's no soluble plutonium recovered. And, finally, and this is important from the perspective of scoping our field experiments, because we've in the past used these spheres as surrogates for colloids, so this is a very important conclusion from the field testing perspective, in that the different sizes actually behave similar and dissimilar to natural colloids, depending upon their size, at least in this particular set of experiments.

Moving to uranium and neptunium sorption and transport, this is results, again, for that sorption test, as well as dynamic recalling experiments. You see slightly moire sorption of neptunium than you do uranium in the alluvium. This is, again, alluvium from actually three different boreholes. You get a wider distribution of Kd values when you go to the dynamic recalling tests. And that's an important consideration when you start talking about how this data feeds into the sorption characteristics as included in the saturated zone model.

This work is preliminary, but is being incorporated into our models for transport within the alluvium in the current iteration of the saturated zone models.

Moving now to volcanism, we've got, again, as you're aware, an Igneous Consequences Peer Review ongoing. DOE has that ongoing. We're completing that. I believe there's another meeting in later February on that peer review panel. In parallel, we're evaluating different consequences areas, dike propagation, extrusive events and how those may disrupt the proposed repository. We're also evaluating the aeromagnetic data that the USGS has collected in cooperation with Nye County, with the counties, and evaluating the probability of intersection based on that aeromagnetic data.

Just to put it further in context, this is a regional map, a very simple regional map showing the Timber Mountain Caldera, Yucca Mountain, showing the distribution of existing basalt cones in the area, and their age distribution, and showing how, and pointing out that at the time of the Probabilistic Volcanic Hazard Assessment in the '95 time frame, we incorporated these observable features, as well as presence of seven anomalies that were buried, as identified by geophysics.

More recent aeromagnetic surveys, this is a color image. The take-home point here is you've got additional information now that allows us to further evaluate the presence or absence of anomalies. We've identified additional anomalies. Under the figure, you're probably seeing circles maybe. These are actually the seven centers that were identified in the PVHA time frame in '95.

If you click again, there's additional ones that show up. These are actually potential anomalies that are now being looked at, and we're actually in the process of thinking about sensitivities to probability based on those observations. So, this is work in progress. I'm not prepared to say a whole lot more about it. It's truly work in progress.

Moving now to what I'll call the engineered barrier system. Here, thermal properties, we're looking at thermal properties in the lower lithophysal in particular using an integrated laboratory and field program. We're collecting additional samples and doing laboratory measurements as a function of saturation and temperature, and also larger scale measurements in the field to look at the effects of lithophysae on thermal properties.

There's a lot of backup on these tests in the field in particular in your backup. But this is just to bring you back to the fact that the field experiments are actually occurring in the cross drift tunnel in the lower lithophysal.

An example of some of our field measurements and what we're getting by way of thermal conductivity versus what we're using in our models in terms of estimates for thermal conductivity in the lower lithophysal unit. You can see that the wet values are above the model values, but they're well within one standard deviation. When I say wet and dry here, that's a semi-quantitative wet and dry. The wet measurements are below boiling, and then we dry a certain amount of rock, and we then calculate what we call dry thermal conductivity. But we feel real good about how well these are actually matching in terms of our model analyses versus what we're getting in the field.

Also, again, we're doing a laboratory program. This shows thermal conductivity as a function of porosity. The majority of this data is laboratory data. It is all laboratory data. At one temperature, the lower lithophysal samples show the effect of porosity, as well as the effect of saturation. But these laboratory data are small scale samples, so you're losing the effects in general of lithophysal cavities, whereas, when you go to the field, you start to better account for those cavities. And the field laboratory data are, we feel, consistent, and we feel very confident that we're headed down the right track with thermal properties.

Mechanical properties I think is a very similar story. We're doing a series of sampling of large diameter cores and doing experiments in the laboratory, as well as conducting in situ field tests at different locations within the Topopah Spring, and comparing the laboratory and the field measurements, and integrating that with that earlier fracture lithophysal work that I mentioned that the Bureau is doing.

This is showing the locations of three of those mechanical field tests. They're called pressurized slot tests. We actually cut two slots in the rock, pressurize the rock, try to get failure, and can then through stress/strain relationships, calculate Young's Modulus ratio, and get characteristics of the rock. We've done three of these tests, two in the lower lithophysal, and one in the upper lithophysal, and those are all completed in terms of their field work.

Some of the preliminary results from these tests, there was modulus here. Again, these are all lithophysal units, so they have cavities, a significant number of cavities. If you compare the results from the lithophysal rocks versus what we had done in non-lithophysal rocks in Alcove 5, you can see there's a pretty significant difference. We attributed that to the effects of lithophysal cavities.

And all of these results that we're collecting in the field and lab are all being incorporated into our drift degradation models that are being updated for LA.

This is just to try to drive home the fact that we've got a laboratory program. Here, you've got a range of

Young's Modulus as a function of strength for different samples from different parts of the Topopah, again, all lithophysal samples. Remember, the field measurements for Young's Modulus were down in this range. So, you can see the effects of lithophysae derives you much lower values.

Now, this will again link in I think with what Joe is going to talk about this afternoon. One of the aspects of understanding the waste package environment is understanding what kind of dust might gather on the engineered barrier surfaces. The U.S. Geological Survey has done a lot of work looking at, in recent years, in the past year or two, looking at dust chemistry as we see it currently in the ESF.

We've collected dust. What's the source of the dust? You can get it from construction activities clearly. There could be ambient dust being brought in by the ventilation. But they have taken samples, and this is just an example of some of that data. It shows actually this is normalized--that got cut off, too, I apologize--this is actually a plot. We've normalized the concentration of dust relative to the bulk rock. So, you take a piece of Topopah Spring, analyze it, and then analyze the dust, and we're comparing the two.

And the data shown here is for different sizes of dust, different mesh sizes, and it shows concentrations of some of the key cations and anions for, again, dust samples from the Exploratory Studies Facility. And this is very important information that folks who think about the environment are incorporating into thinking about what can go on in terms of evolution of the environment in the drift. There's more backup on this, too, quite a bit, tabulations, and all kinds of other things in the backup.

Another aspect of the environment that I want to talk about briefly. This is going after things we've been introducing into the system, and how that might impact water chemistry. This is a diagram that attempts to conceptually lay out if we had a rock bolt supporting a drift and you grout that in place, you've introduced grout into the system. What can that do to the near-field and also the in-drift water chemistry? Clearly, the grout will react, and it could produce significantly higher pHs than we get at ambient.

We're starting an experimental program at Livermore, Carl Steifle and his co-workers, where they're doing reactive transport column experiments, looking at the effects of grout, and how that affects water chemistry as a function of time. And there's quite a bit of data in your backup. This is the conclusions to date.

We're basically looking at, again, evolution of water chemistry. So, if you take a drift that's been exposed to grout, very high pH, and you then put it in a CO2 atmosphere, they actually observed that it gets neutralized, or the pH gets much lower on relatively short time scales. And how much CO2 is available is clearly important in driving that process.

When you look at the details of what's controlling the evolution of the water chemistry, it's actually the calcium hydroxide as opposed to the calcium silicate phases that's controlling the chemistry.

They've also been thinking about how one could go about looking at, let's say, tailing the grout chemistry such that you could better control the pH. One way to do that is clearly to add silica to the system. They've done some experiments in an autoclave, again looking at on the order of 5 per cent silica added, and you still have portlandite in the calcium hydroxide phase controlling the chemistry. So, you still get high pHs. So, the preliminary conclusion is you just need to consider higher silica mixes.

As you age the grout, you get calcite precipitation. That will reduce the reactivity. So, over time, you would expect the combination would help in terms of not producing real high pH solutions.

And, finally, this is ongoing work. It's being incorporated into the thinking that's going into the in-drift chemistry models. So, you've got dust, you've got this sort of work, and you've also got the work that I alluded to at the beginning, an evolution of pore water chemistry that the

USGS is doing. All that is part of the picture.

Last slide, a very brief discussion of some of the examples of the work that's going on at Argonne National Laboratory in the waste form area. This is an example of some of the work that they've done looking at glass dissolution, high-level waste glass dissolution. What's plotted here is basically dissolution rate, and the way they're monitoring that is by boron release from the glass.

So, it's dissolution rate versus pH. This is actually a set of tests at 90 celsius, where they've added iron corrosion products to look at how that might affect dissolution rate. You can see there's no large effect. This is an eta. I figured that out this morning. This is a model parameter that addresses the pH dependence in the system. But, the bottom line here is it shows the pH dependence on glass dissolution, and also that there's no effect on glass dissolution from iron corrosion products. This particular one happened to be a key technical issue with the NRC.

This kind of information, again, is part of the long-term testing program at Argonne, as well as PNL, and is being incorporated into the AMRs, the analysis model reports, as we speak.

To wrap up, I've walked through a lot of information. This is an ongoing program that's providing our basis for the license application that John or Margaret alluded to. We continue to address the uncertainties. We feel that it's providing us additional confidence in our processes and, again, supports the initial submittal of the license.

That's all I have.

CORRADINI: Thank you, Mark. Questions? Paul, Priscilla and Dick.

CRAIG: Paul Craig. Thanks, Mark.

Occasionally, we get presentations here where we're a little--we wonder what the person actually was talking about.

Turn to Number 31, if you would, please. Every once in a while--this is an example of what needs to be highlighted. The issue of what capillarity does is controversial, and it's to throw capillarity in a computer model, and it's much harder to do it in real life. This is the first one I've ever seen in real life that shows that there's a threshold. If I understand the picture correctly, it is a sharp threshold.

PETERS: Yes.

CRAIG: It would be real nice if one could extend this and figure out whether it applies in a real repository situation. It conveys a message that the capillary barriers really do work.

PETERS: And, again, I need to credit the folks at

Berkeley. They've done a lot of the testing. The datasets, they have datasets for--this is one example, this is the kind of data they've been collecting now for three or four years in other niches as well. You know, it's nice to hear that it's--you know, we could probably show more of this, because there's more of this kind of stuff out there that they've used to calibrate the model.

NELSON: Yes, I think that story needs to be told more fully and in different ways from the way we've had it before. This is Nelson, Board.

Mark, I feel comfortable asking whatever it is that occurs to me because I know you can handle it, and I know we're going to miss you. So, let's get started.

First, we asked in the past about rock fallout to date, and how well that was being tracked and fed back into the idea of drift degradation. Can you give an idea of what information is being learned, gathered?

PETERS: Yeah. We talked about this several times. If Mark Gorham was here, I think he'd tell you they are using what I'll call more qualitative, you know, in terms--I think what you're after is if you walk down the tunnel and you could look at an existing tunnel and see how it's behaving and how is that understanding incorporated into the model. Is that your question?

NELSON: No, I think that there's a prediction possible about rock mass condition and how it might deteriorate that somewhere is in your drift degradation model.

PETERS: Right.

NELSON: And it has something to do with changes in stresses and humidity or moisture content and also the heating cycle. To what extent are even the drying, like in the ESF or ECRB that just happens because of ventilation, precipitating some fallout?

PETERS: You're getting air slacking. My observation is is you're seeing some air slacking, I think that's the right term, on the ribs from drying out, and you're getting some slacking. We're not getting any significant, what I call significant degradation. You're seeing some key block formation, and I know they incorporated those observations, I won't say directly calibrating their model, but they're certainly aware of some of those phenomena and how that compares with their long-term model predictions.

So, if you talk to the folks who are doing those models, they could tell you how they're incorporating or how they're thinking about that in terms of their models. But they're not using the straight calibration data.

NELSON: Okay. So, the rock fallout to date is systematically recorded?

PETERS: We walk it down, the engineers on the site walk it down for safety reasons. So, there is that data, and also the model types go out there frequently, and they walk it down, and they understand how it's behaving. But the data exists.

NELSON: Okay. Just two more things. One, you talked about how the neoprene, how you were coming to an understanding of how some of the things you introduced into the rock do affect what goes on. I think there's probably a whole generation of new instruments that you could expect to come along on performance confirmation, new thinking, and lessons learned, not necessarily regarding the science, but regarding the measurability.

Is there going to be a period of time where that kind of thinking not only about what instruments performed well, but also what didn't, and where might new instruments be developed, new strategies be developed that feed into performance confirmation, and the science yet to be done?

PETERS: Yes. They're in the process of defining performance confirmation program, PC, and once they have that defined, then that group that's in Performance Assessment is starting to and will continue to look at what I'll call sensor technology, things like that. I'm also involved with the S&P piece, and we've had some discussions, no commitment implied here, that possibly we would also work with them to help look at what's out there. There's a tremendous amount of sensor technology in the complex that we have yet to think about.

NELSON: And I think it's possible to push back on the sensor developers to develop the sensors that you need.

Finally, I'm worried about that red spot. The Swellex, were those Swellex bolts?

PETERS: We used Williams. I believe those--

NELSON: Williams Mechanical?

PETERS: Yeah, we used different ones at different places in the tunnel, and I'm not sure I'm going to be able to tell you exactly what we stuck in the crack there.

NELSON: Well, what I'm wondering is where is the water being suggested to be from? Was that inflation water? Why is the water there?

PETER: I'll confirm with you, but my guess is they're probably Swellex first. We're not collecting the water, unfortunately. We're seeing evidence, we think we're seeing evidence of it actually dripping. So, I think that's a very good question. We don't yet know the actual source in terms of whether it's introduced as you're installing the rock bolts, or whether it's discrete fracture flow returning to the drift.

NELSON: Swellex usually drain out pretty well.

PETERS: We didn't see any draining.

NELSON: You've got others than that one that could be evaluated as well. So, you plan on actually looking at the rock bolts and seeing what the corrosion was from?

PETERS: Particularly post-test, yes. We did it in the single heater tests, and I'm presuming an outcome, but when we get to three years from now, there's no doubt in my mind we'll go in and try to sample some of those bolts, do cool tests, and also sample them to look at their alteration.

NELSON: Thanks.

PARIZEK: Parizek, Board.

Mark, congratulations again on a heck of a lot of information, and maybe we can ask you some questions later when I digest more of the details, if possible.

PETERS: Sure.

PARIZEK: But a couple of points right away. You had a design of a repository. What you put in it may have some consequence. Obviously, there's very low pH water that may cause alloys to disappear, and so on. But, so far, there's some experiments that cause you some problems.

PETERS: Right.

PARIZEK: Such as this question of neoprene. On the one hand, you had also the packer case before. Has some thought been given to this in terms of design of the confirmation testing plan, as well as even a critical look at all things you propose to put in the repository to hold it up in order to put waste in there, not to have chemical surprises creating an environment that might be harmful to waste packages in the future?

PETERS: Yes. We actually have a process which clearly didn't work 100 per cent perfectly. I'll give you a word, it may not mean much to you, but if you hear people talk about the determination of importance evaluations, we do those for all of our programs for underground and at the surface. So, if we introduce anything into the system, there's an evaluation of waste isolation, meaning are we impacting long-term the repository performance. There's also an evaluation focused there on test interference.

In the case of the pack and the neoprene, we had a process, but it clearly didn't work perfectly. Those evaluations were back in the '97 time frame. It's not an excuse. That's just how long ago they were. So, that process--a process like that has to continue in my estimation through repository development. The lessons that we've learned specifically here are being incorporated into that. But, yeah, we're going to have to be very careful, particularly in waste isolation, that we're not introducing things into the system.

PARIZEK: That's an ongoing thought process.

PETERS: Right.

PARIZEK: So, it doesn't really compromise observations you make later on that might be harmful in terms of even just confirmation tests, let alone repository performance.

PETERS: Right.

PARIZEK: No mention was made of the colloid transport in the Calico Hills or Busted Butte. Do you have any new observations you can offer on what's been the outcome of the colloid transport in the Calico Hills?

PETERS: I talked about that in the past. I think as you will probably recall, we had some problems with the field experiment. We were using spheres, not natural colloids, and I won't be able to remember off the top of my head how big they were, I think they were on the order of 100 to 200 nanometer size spheres, and we were getting a problem with them actually flocculating and actually gathering at the injection point and not transporting through the rock.

So, in terms of field information, we don't have a lot of meaningful information from Busted Butte. They've done some column experiments with crushed Busted Butte material. Dick, I'm sorry. We could get you more information.

PARIZEK: There's not much new then?

PETERS: There's not much new from what you've heard in the past.

PARIZEK: We were just trying to get all the value we can out of the experiment, to the extent possible.

On Slide 33, you had some indication of water flow. And the question is what sort of seepage rates are these equivalent to in terms of a climate state? When you put the water in these experiments, also the other slide that shows what you put in versus what you collected, are these really out of the realm of--

PETERS: Yeah, I think in millimeters per year. But the current flux in the repository horizon by multiple lines of evidence is, what, one to ten millimeters per year.

PARIZEK: You had grams per second, or something?

PETERS: Yeah. We've done those calculations. It ends up being thousands of millimeters per year. We're over driving, it's even beyond what we currently feel very strongly is appropriate for pluvial type.

PARIZEK: So, they're extremes?

PETERS: Right.

PARIZEK: Way beyond what's expected.

PETERS: Really, Alcove 1, Alcove 8, they're all extremes.

PARIZEK: That's good to hear.

The other question I had was the unsaturated zone flow model, the U.S. Geological Survey secondary mineral studies, you had both the fault zone mineral studies versus the non-fault mineral studies. What came out of the fault zone mineral studies in terms of some sort of a percolation rate through the mountain? Does this support what's in the Lawrence Berkeley model for flow through the unsaturated zone in faults?

PETERS: I might have confused you. What I showed on the faults and non-faults was actually uranium series.

PARIZEK: Secondary minerals.

PETERS: It was actually pore salts.

PARIZEK: Okay, pore salts.

PETERS: Yeah. I call them pore salts. They took a rock and flushed water through it, and Brian is in the audience, he can correct me if I'm wrong, but they were looking at kind of bulk rock, uranium isotopes. The fracture minerals themselves, that's where they were doing the ion probe data.

PARIZEK: Right.

PETERS: And Brian can jump in here, but I'm not sure how much they've done in terms of looking at fracture mineral dating within the faults.

MARSHALL: Brian Marshall, USGS.

Actually, the uranium 234, 238 disequilibrium studies that are underway now are on bulk rock. So, dissolving the whole rock to see if we can map out any zones of preferential flow. We know from our previous secondary mineral studies that we have large amounts of uranium with high amounts of excess 234. And, so, that 234 uranium had to come from somewhere. It comes from the bulk rock through water/rock interaction over long time periods.

PETERS: But the calcite opal U series geochronology that you're doing in the fracture minerals in the Topopah, have you done any of that into the faults?

PARIZEK: Yeah, it's a long-term average through faulted rock.

MARSHALL: In general, we have not found very much calcite or opal, secondary minerals, that have been identified.

CORRADINI: Dave, did you have a question?

DIODATO: Thure can go first, and then I'll go.

CORRADINI: Okay. Thure, do you want to go ahead? I'm sorry.

CERLING: Thure Cerling, Board.

Just on Slide 62, which was chemistry of the dust, I was just struck by the fact that it looks like the mixture of rock and salts, and so I was just wondering what the source of the salts were.

PETERS: Partly probably dust brought in from outside , probably in some cases residue from the construction water.

CERLING: So, there's a significant antigenic flow?

PETERS: Yes.

CORRADINI: Dave, go ahead.

DIODATO: Diodato, Board Staff.

Earlier, Jeff Williams introduced a potential repository layout with five phases. And in terms of confidence and uncertainty in the program predictions about performance, there's, one, the empirical technical basis that, you know, is the gathering of data, and that's kind of your field, testing and the gathering of data, and process modelling, then the extraction for PA. So, uncertainty can creep in at every stage of that.

So, what I'm wondering from your knowledge, is about the extended characterization the rocks, both geologically and hydrogeologically in, say, 2, 3, 500 meters UTM, north, in that part of, you know, Panels 2 and 3, relative to the other lower southern, more in the main part of Yucca Mountain, and then how that might work in. In terms of performance confirmation, you had a capital P, capital C, and I don't know if that's different from some other thing, and if that characterization--

PETERS: I'll tell you what I mean by that. Can we go to 3? 4, sorry.

DIODATO: So, north is to the southwest in that? Okay. North of the ESF about 500 meters, and then beyond.

PETERS: We evaluated the layout that you saw. Jeff showed Panels 1 through 5, and we feel it's adequately characterized.

DIODATO: How does that compare with the degree of characterization you have within, say, the ring bounded by the ESF?

PETERS: Well, clearly, we have additional information when you have the ESF here and the cross drift here. We've got surface wall coverage all up in this area as well. If you look at a map of Panel 1 through 5 and an overlay, surface boreholes in the ESF, we've evaluated that and feel that it's adequately characterized.

DIODATO: And I'll ask other people about the process modelling, so I won't--

PETERS: Say that again.

DIODATO: I'll ask other people about the process modelling, the PA, I see Peter Swift in to come on.

PETERS: Yeah, that's probably somebody else's question.

CORRADINI: Dan?

BULLEN: Bullen, Board.

Can we start with Slide 13, please? I hate to come back to Chlorine-36, but I guess I just have sort of a follow-on question. When you say that your conceptual models do not rely directly on this data and will not be modified based on the results to date, will they ever rely on this data? And do you expect them to have some impact, specifically with respect to fast fracture flow?

PETERS: Let me be clear what I mean by that. This needs to be real clear. The data that's actually used to calibrate the flow fields is chloride data, total chloride. The Chlorine 36 observations that were made in the mid

Nineties that suggested fast path--and, by the way, there's other data that suggests we have areas of fast flow in the mountain--our conceptual model respects that dataset, meaning we have fast flow in the conceptual model.

If you take the--let's ignore the Los Alamos data for a minute, and you take the USGS/Livermore data at face value, they see no evidence of bomb pulse. They would suggest pore waters hundreds of thousands of years old, no fast flow, ignoring any other data. You know, there's no intent on our part to change our conceptual model based on that at this time.

BULLEN: Okay. Can we move on to Slide 20? You stated that the pore water extraction is really hard to do.

PETERS: I probably overstated it.

BULLEN: It looks hard to me, so, you don't have to overstate it. I guess the question I have is are there any natural processes that you'd expect to get real pore water, you know, in contact with the waste packages without having to buffer the effects of the matrix rock? I mean, we're going to hear a presentation that will probably talk about concentration of pore water this afternoon. I think my crystal ball tells me that.

So, I'm just looking at how can you get pore water out and concentrate it, besides ulltracentrifuging the mountain?

PETERS: So, how do you get the concentrated pore water out in the fractures and get it into the drift?

BULLEN: Right. Just curious.

PETERS: It's hard. But, it's a question of how--I'm not going to answer your question directly, but the fracture, what's the composition of fracture water? What's the concentration? Is it in equilibrium with the matrix? I think that's a real good question to ask.

I did overstate my point, but this--it's not easy to get the water out of those welded tuffs. As you know, we haven't seen dripping. We've collected water out of them, and when we have, it's usually when it gets moved around by heat, and it collects in larger volumes. To get ambient pore water chemistry, you have to go to an ultracentrifuge.

BULLEN: That's a good point. That's what I, you know, wanted you to make. Moving on to 35.

LATANISION: Before we move on, can I interrupt?

BULLEN: Please.

LATANISION: I'm missing the mystery here. Isn't the issue just heating the rock? Isn't that the process that drives the water out? What is the mystery here? I'm missing the mystery.

BULLEN: Bullen, Board.

Actually, heating the rock is one thing, but do you get the same chemistry of the pore water after it's moved through the matrix, is my question. The chemistry of the pore water is important because it's the chloride concentration and all that. But when you heat it up, does it have the same chemistry? In this case, they're trying to preserve pore water chemistry. In the other cases, do they preserve pore water chemistry, was the sort of basis for my question.

LATANISION: Okay. And we don't know the answer to that?

PETERS: We've sampled, if I'm understanding what you're saying, we have sampled water that's been moved around by heat, collected it.

LATANISION: Okay.

PETERS: Analyzed it. It looks pretty similar.

BULLEN: How subtle are the differences, I guess is my question.

PETERS: The sample we've analyzed that's been moved around by heat, I can't remember, I won't be able to give you by element, but it's close to a condensate, with evidence of interaction with calcite and opal. But it looks like J-13 in a lot of respects.

BULLEN: Bullen, Board.

Because J-13 water and pore water aren't the same, in my book.

PETERS: I understand.

BULLEN: Okay, thank you. That was my point.

PETERS: But, we've got that, we've got the stuff we moved around in the heater test, and we've got J-13.

BULLEN: So, you have the smorgasbord there from which to choose.

PETERS: I wouldn't say it's a smorgasbord. No, we can understand the differences between them, and those are part of our thinking when we--what kind of waters we put into the tanks at Livermore, and et cetera.

BULLEN: Well, maybe a follow-on to the water is Figure 35 then, because, let's see if I can get this one right, what you're looking at here basically is the bulkhead condensation, which you stated is basically condensation is dominant, which I think is that second to the last bullet. Is there a potential that there's a fraction of seepage that's in there, and do you have any models that might predict that you've got partial seepage, partial condensation, which basically gives you a more dilute seepage, if you will, than the result that you see?

PETERS: I can't rule it out totally. But the water chemistry that we do have, it's dilute. It's condensate. There's basically nothing in it.

BULLEN: Okay, that's fine.

PETERS: A very dilute seepage.

BULLEN: I guess I just wondered if there was a mix of the two.

PETERS: Well, I can't rule that out at this time. Once we have additional water chemistry data, coupled with the modelling program, I think we can address that.

BULLEN: Great. Figure 56? Let's see, is that the right one? Yes. You're looking at basically some thermal conductivity measurements when you talk about wet versus dry. Do you have a change as the rewetting front comes through, and can you take a look at it from the perspective of the model of, you know, when will it be wet, when will it be dry? Is there a time element that you can drive here, or is this just bulk data that you can use in the TSPA?

PETERS: From the field experiments?

BULLEN: Yes, from the field experiments, can you see, like, what the rewetting potential might be?

PETERS: We're just now getting to the point where we're turning them off. So, I think I have to answer that later.

BULLEN: Okay. Put that in the to do file.

PETERS: Okay.

BULLEN: And then my final one is actually 59, where we're looking at the mechanical tests. And, you know, you notice that the thermal test facility gives you--well, actually the moduli measured here, going from three gigapascals to, you know, 20 gigapascals, I guess does this suggest that you're going to have to change the ground support from rock bolt to mesh, or are you just going to have to rock bolt the daylights out of the place, or are you going to line it with gunite, or what do you think might be a suggestion here, since we've got some significant differences in the rock strength, I guess?

NELSON: This is not strength.

BULLEN: Okay. But I'm equating my simple engineering analysis here, because this is not as strong--is it not as strong, is that right, Dr. Nelson?

NELSON: It's not as stiff.

BULLEN: Not as stiff, okay. Well, not as stiff.

PETERS: Difference.

BULLEN: Okay, difference, I understand. But, do you think that basically the ground support is going to have to be modified?

PETERS: The ground support folks are in the final evaluation of what ground support they'll carry into the license application design. To my knowledge, it's not changing.

BULLEN: Okay. Then I'll sneak one last one in from the audience because Sally Devlin is my friend. She just wants a little update on her bugs. I know you have limited time. Do you want to talk a little bit about what Joann Horn is doing at Livermore? Are you still sampling bugs, I guess is the question?

PETERS: Are we still sampling bugs in the tunnel?

BULLEN: Actually, in the water and on the canisters was her question.

PETERS: We're still doing testing on MIC in the lab, Joann.

BULLEN: We've seen Joann's work, and Joann has basically gotten bugs from the mountain, extracted by identified counties, and done the work. There are bugs in the dust. It wasn't mentioned because you didn't have time.

PETERS: Well, I mean, we've characterized--Larry Herskin has done the ambient population. Joann worked with him. There's not an active program continuing to collect bugs from the field. The USGS has looked at organic acids and pore water. That's also being incorporated.

BULLEN: Okay. Thanks, Mark, and thanks, Mr. Chairman, for letting me ramble.

CORRADINI: You're welcome. Next question? Dave, then Ron, or are you done?

DUQUETTE: Duquette, Board.

Figure 35, we don't have to go back to it, but as you know, the Board is somewhat concerned about what you've now identified as condensation. And you'll have to forgive me, because I'm a relatively new member to the Board, but is that because of current human intrusion? Is it because of the placement of the bulkheads, or do you expect that same condensation to be in place once you begin emplacing the canisters?

PETERS: We're seeing a phenomena. We've introduced some water into the system when we mined it. What we're seeing back there is associated with heat sources that we've introduced. What was driving it in the early days we think is the tunnel boring machine that's parked at the back end was hot, and we had a temperature gradient that was driving the phenomena. Is it something we would potentially see in the repository? I think so. We're going to have variable heat sources along the drift. That's why I personally, this is me talking, I think it's very important that we model the results and understand it not only from a seepage versus condensation, but also what the heck is going on inside the drift.

DUQUETTE: Obviously, I think it's very important as well.

PETERS: Was that a clear enough answer? We've introduced heat sources. There's transformers and things in there that are hot, and you can see the phenomena are associated with those heat sources.

DUQUETTE: Yes, I understand that. My question really had to do with whether you expect the same kind of condensation once the vault is in operation. And I think what you've said is you do expect it.

PETERS: Well, that was me talking. Peter maybe has a different perspective, or others. You might want to reask that question.

CORRADINI: So, can I just follow up on that? I'm curious about the local, what you said was a potential source, which was from the mining and then driving it with a heat source. Wouldn't that be affected then by the circulation or the forced convective flow you have in the early phase where you would essentially be pulling all that moisture out which you had introduced? I'm looking at a time scale issue here.

PETERS: During the operations period, you'd be force ventilating, you'd dry the rock "X" meters into the rock. But once you close it off, after you close, it will start to rewet. It's akin to this. I think that's the point.

But let me back up, the water chemistry, you know better than I do, if it's a condensate, it doesn't cause a tremendous amount of problems for us as a straight condensate. But then you've got to worry about dust on the surface and how that all interplays.

CORRADINI: Okay. But just so I'm, because we were both kind of talking to each other, is what was the source of the water, part of the source of the water was the initial mining operation. And that would have passed through on a time scale issue. If it's coming from what you had dried out is coming back, then I understand that we're now talking about different time scales.

PETERS: Right.

LATANISION: Number 39, please. The conclusion that the fractures are less important, this is Item Number 2, what's the analysis that leads to that conclusion?

PETERS: Go back one slide. This is a different part of the stratigraphy. We're down at the very bottom of the Topopah Spring in what's called the vitrophere, the chilled part, the glassy area. This is all bedded tuff. There's some fractures that occur in the upper part of the unit. And when they did the detailed testing in that area, they didn't see significant affects of the fractures in terms of how it behaved in break-through. They saw more of an influence of the heterogeneities in terms of the sub-layers. We're not talking Topopah Spring here. We're talking a different part of the geology.

CORRADINI: So, what I thought you just said was the fracture flow dominates to a point, and then at a lower elevation, the resistance becomes higher, and that's what then controls the flow.

PETERS: Right.

CORRADINI: Am I interpreting correctly what you just said?

PETERS: I think so. These particular units are I'll call unfractured. They're sandstones. They non-welded.

CORRADINI: Other questions? Carl?

DI BELLA: Carl DiBella, Technical Staff.

I've got a question about the dust, Mark. You mentioned that the dust could possibly have three sources, construction, man made sources, and the atmospheric dust. And I looked in your backup, and it looked like your dust sampling was by way of just vacuuming the walls of the tunnel. It would seem to me that the major component, or at least a significant component, of the dust in the waste packages after 20, 30, 50, 300 years of ventilation might be the ventilation. And I'm wondering if you have any plans for measuring the dust that is in ventilation air. And as I say that, I have a vague recollection that someone has done something in this area, perhaps Nye County.

Could you answer whether there are any plans to look at what's in the air? And I would think actually there might be somewhat of an organic component to that, spores and that sort of thing.

PETERS: I'm not sure of the status of the analyses, but I know the USGS was working with the field folks to set up a filtering system to try to do just that. I just don't know where we're at in terms of actually collecting them. It's been discussed, Carl, and I would say it's planned, but I don't know what the status is. I mean, it's clearly another piece of the puzzle that we need to address, and they're there already thinking about it. I just don't know the results. We could find out.

CORRADINI: I see more Staff questions. Leon, did you have a question?

REITER: Yes, Leon Reiter, Staff. Just two quick questions.

How does the seepage threshold in the lithophysal unit compare to the seepage threshold in the non-lithophysal unit? The second question is you mentioned, or talked about the ongoing results and updating models, and stuff like that. Maybe this is a question for Peter more than you, but what is the cutoff for the licensing application for TSPA that you can use this kind of thing?

PETERS: Okay, first question, my recollection is is that the seepage threshold concept, take that at face value, the non-lithophysal is, to my recollection, a thousand millimeters per year, and the lithophysal is lower. That's my recollection, and I'll confirm that with Bo, or somebody up in his shop.

Second question, the timing. We're in the process of preparing the analysis model reports that will be the basis for LA as we speak. So, the information that I've talked about here that was collected up through the end of the calendar year is being incorporated into those AMRs. The data will continue to be collected. So, that then feeds TSPA. The AMRs are finished in the May, June time frame, and then TSPA will be completed for LA. The data will continue to be collected in a lot of these programs. It will be used as corroborative for the purposes of '04.

REITER: I want to get to the general feeling. You can say that data collected after June will have little or no impact?

PETERS: I wouldn't say little or no impact. It won't be in this generation of models in terms of our calibration or anything like that. But it will still be available to corroborate and also further validate. I wouldn't say no impact. There's data that's being collected that's being incorporated in terms of, for example, calibrating the model, they're going to calibrate the model, it's been calibrated, they're not going to go back and calibrate it, but this is an iterative process, as you well know. Those will be updated once again as we go forward with amendments, if we pass the initial license. So, this information doesn't get lost. There will be an ongoing program and we will continue to update the models in the TSPA as we go out. That's the answer.

CORRADINI: Bill?

BARNARD: Bill Barnard, Board Staff.

Mark, how did you sample the splotch, the red splotch?

PETERS: They actually went in with--I didn't actually see it working, Bill, but my understanding was is it was a little extension that they put on the camera box, and they went down with a swipe and swipped it manually and brought it out and XRD'd it.

BARNARD: So, you had somebody inside?

PETERS: No, no, they went in, they hooked it up onto the camera itself that went in, controlled it from 32 meters away.

CORRADINI: Priscilla?

NELSON: Just two more things. Nelson, Board.

You have several times referred to scale, scale effects, and I think this project is somewhat unique in the wide variety of testing at different scales that's been accomplished, but I don't think there's been a coherent statement package made of all the conclusions that can be pulled regarding scale. And they're so incredibly important that I'd encourage the project to try to pull those scale observations together, the scale effects observed in different tests and different size openings in different kinds of properties. So, just the whole rock engineering community would benefit very much from that kind of insight.

I wanted to ask you one question regarding the input of all of your thinking here for the igneous consequences evaluation. To what extent do you think the igneous consequences models would have different conclusions, depending upon the lith versus non-lith rock?

PETERS: I'm probably not the right guy to answer that question.

NELSON: Okay. Because it seems that a lot of the conversation was really focused towards non-lith and fracture interactions, and it seems like things might be a whole lot different with the lith rock.

PETERS: In terms of dike propagation?

NELSON: Yes. So, I'm wondering to what extent you are linked into the igneous consequence modelling in terms of making sure they're characterizing the rock well to understand what the expected consequences are.

PETERS: Yeah, I don't know. Peter, are you out there? I'm going to ask you a question. When we think about dike propagation through the repository, do we account for differences in the country rock mechanically?

HARDING: Ernie Harding, BSC. I'm not going to offer much illumination on this topic. But, I will say that the same group that is doing the rockfall and geomechanical work is also looking at the mechanics of dike propagation in the rock units. So, the group is aware. I'm talking about the consultants led by Mark Lord on our staff, and they are aware of the differences in properties for the different rock units that we're talking about. Does that help?

NELSON: Is there an impact, or should I ask Mark?

HARDING: Well, as you've seen from other slides, the stiffness and strength of the rock units do vary. So, we know that, so that will have to be factored into the mechanics of dike propagation.

NELSON: Yeah. Nelson, Board. More than just stiffness, is the presence of the porosity?

HARDING: Right.

NELSON: And bleed off the maintenance and pressure at the--anyway, I'll talk to Mark.

CORRADINI: We have a couple more questions from Board members. We're at 11:45, which is the time for public comment. I have a public comment register of six people, none of which indicated they wanted or needed to talk before lunch, which means we would have it occur at the end of the day.

DEVLIN: I would like to talk before that.

CORRADINI: Okay. And nobody else?

SPEAKER: I would like to also speak now.

CORRADINI: Okay. If it's before lunch, then nothing at the end of the day; is that correct?

All right, so let me do this. Let me take the last two questions were Paul, is that correct, Paul, and Dick? Briefly, we'll go to public comments from Sally and--go ahead.

CRAIG: Okay, with any luck, I now have a microphone that works. Paul Craig.

This is one of these naive comments that has to do with the role of new science. And we've had a lot of conversation about the cutoff times for doing the modelling. The history of the project has shown that science has occasionally been quite unstable. The understanding of the mountain has changed a lot over the decades.

Currently, it looks like it's fairly stable. We haven't had any big surprises for quite a while now. I'm going to say it seems to me it's reasonable that if you don't have any big surprises in the science, that it doesn't make a whole lot of sense to go back and recalculate models, because it's an exceedingly laborious process and it won't provide you with any major new insights or any surprises.

On the other hand, if it should turn out that you do get surprises from the new science, then somehow or other the system has got to find a way to take that into account. There's no way to avoid that. Maybe thinking in terms of surprises in the science versus non-surprises in the science is a useful way to think about the handling of new information. I just lay this out as a hypothesis or proposal.

PETERS: Did you want me to comment on that at all?

CRAIG: That's not necessary. But if you feel like it, please. What I'm trying to get as is a continuing concern we have about the cutoff time.

PETERS: I understand. The only comment I would make is that there will be pieces of the program, and that's what I meant by performance confirmation strictly. That will be a condition--that will be part of the program that's in place to continue to confirm what we assume in the '04 license. That's a condition of the license. So, there will be things that will be reported to the NRC, and there's the long-term science program that John and Margaret have planned out that will continue to collect information. I mean, the licensing process, as you know, allows for continued updates as we learn additional information.

CORRADINI: Dick?

PARIZEK: Parizek, Board.

Just a quick point. There's no mention made of analogues. Is there anything new on the analogue front, the analogue report? Anything going on in Mexico?

PETERS: There was a report put together four or five months ago that synthesized everything that we had done on analogues to date. The primary program that's still in the plan for the near-term is to try to do the work at Pena Blanca. But that was delayed by the problems that we had with some logistics, and now it's also been deferred by the continued resolution.

Analogues is being looked at as one of the areas by the Science and Technology folks. No commitment implied here, just that that is one area that they're looking at very closely.

CORRADINI: Okay. According to the form, I have three individuals that wanted to speak briefly, Ms. Devlin, Sally Devlin, Mr. Grant Hudlow, and Dr. Jacob Paz.

So, Ms. Devlin?

PAZ: This information I'm going to give. I'll try to be very brief. My name is Dr. Jacob Paz. Don't write it. I'll give you all the information written. I have submitted a research proposal about in August. I got the reply that the proposal has no technical merit. In light that one author is a fellow of the National Academy of Science, the second is a former Assistant Administrator. I hand delivered rebuttal comments, including review of the literature materials. I never got an answer.

I'd just like to read the comments. This research letter is to point out substantial different support in the literature for research approval set forth by the proposal. Apparently, the reviewer did not consider scientific literature or failed to explain why their opinion is so different substantially from others that have already done research in this area, and published their results in scientific journals.

The study was conducted as an exploratory study. I want just to mention that it is difficult, if not impossible, to understand why the reviewer came to such a conclusion. I have mailed to Dr. Chu a letter about three months ago. I never received any communication.

Finally, I make a suggestion that the Nuclear Technical Review Board approach the National Academy of Science, energy and environmental system division, to provide above scientific input. The BES, the Division, could provide expert advise through independent and impartial input on complex matters health risk issues, which I have raised.

In my material, I also include some abstracts. I included some federal information in the literature, which I have cited in a paper which I'm planning to submit. I'm going to submit a supplementary guidelines for conducting health risk assessments of chemical mixtures by the EPA. In addition, there's a memorandum of understanding of the DOE, baseline risk assessment to exposure. I'm going to give you a draft report for cesium, cobalt, PCB, strontium and trifluoromethane, and I'd just like to read from Page 25. And this is governmental recommendation.

Neither--examined the toxicity of five mixtures through--are available. Similarly, the physiological pharmakinetic models described are not available. And absent different data, they recommended a compound approach--that only 10 per cent of (inaudible) in the data--the potential of interaction of compounds are also lacking.

There is another draft manual which they're going to draft on guidelines for assessment of toxic action on chemicals, and the last document is interaction for arsenic, hydrozene, jet fuel, strontium-90, and trifluoromethane. All this information exists in the literature. This is just background information.

I'd just like to make a comment that I have not had any study on the movement of radionuclides. Specifically, what I'm concerned about is chromium. The mountain is loaded with manganese oxide. The study has not been completed. We have to think on the current regulation, that Yucca Mountain will become a RECLA site, a CERCLA site, and later a mixed waste site, and we have to mark this framework unless the court or the Congress preempts.

That's all. Thank you.

CORRADINI: Thank you, Dr. Paz. Ms. Devlin?

DEVLIN: Thank you, Mr. Chairman. And I came in a little late, so I might have missed the governor with his hand out greeting you. But I guess I didn't. Welcome, and, of course, all my helpers in the audience, to Nevada, and thank you so much for coming. This is the first time I've ever said I'm sorry you're not in Pahrump, because our highways, because we're getting a huge Wal-Mart, are completely torn up eleven miles in each direction. So, again, thank you so much for coming.

The reason I insisted on speaking is I know that nothing will curb anybody's appetite for our gorgeous Las Vegas food. Therefore, I am giving this report, which I couldn't get copied because I couldn't get to the copying place, which is on nuclear waste may pass through ports. And, as you know, I am Madame Transportation, and what this says is that 21,572 tons, or almost 19 per cent of all the commercial power plant waste destined for Yucca Mountain will go to 15 commercial ports, including Lake Michigan. And this, to me, is perfectly horrible, frightening, and so on.

And while you were all working so hard, I was goofing off and studying. I had a mentor who taught me how to build roads, build asphalt and concrete roads, and all that that entailed, and I did the one thing that most people, as you know, don't do. I got the cost of it--I've done dozens of reports on this--and we're talking over a thousand dollars a mile, or a million dollars a mile. And, of course, we have no roads here in Nevada. We're a nine hazard Intrastate 95, or whatever you use, 160.

The other thing I had to learn was how to build the railroad, which I had help from from Washington, 300 pages worth, and other books. And now I know how to build a railroad. And my feeling is that is well over a billion miles was cheap. But, of course, now that I know you're talking barge and shipping, that is even cheaper, and it worries me even more.

Now, I was hoping that there would be someone from the Coast Guard or at least the American Bureau of Shipping who designs the ships, and someone else here at this meeting when we talk a little bit about transportation, so I hope they'll connect with me. And I certainly welcome W. Arthur, III, and I hope you'll keep me informed about your meeting in February, because that is my field. And I'm going to end with just pounding this with you.

They are talking concrete containers of 500 tons that can sink and they can pull they up for 30 days. That was just in one report. So, I thought you would be interested in this.

And I will close with my computer knowledge, because you know I'm the only one who ever passed the course in Pahrump, and it is http://www.detnews, one word, .com/2002/nation/0204/03/a08w-455450.htm. And that is to get this report which I couldn't get for you.

So, thank you. And remember barging is dangerous, especially in 15 ports, and you cannot use the Amargosa River, gang, I'm sorry.

CORRADINI: Mr. Hudlow?

HUDLOW: Thank you. I'm Grant Hudlow. I'm the CEO of Allied Science, Incorporated. We do resource recovery, and we make waste tires, scrap tires, city trash, that kind of thing, damaged, and it turned into usable products. I've also done some work on transmutation.

I have here a letter of January 24th from Margaret Chu, and she's trying to answer, send to NWTRB comments that they're urging the DOE to up the performance a little bit on this Yucca Mountain. The NWTRB has people that are, a few people that are industrial people, turn around experts. The Advisory Group to the NWTRB also has a few of those people. Those are very special people.

In the American industry, which leads the world in this sort of innovation, those are the people that make things happen. Those are the things that get people to get things done. Those are the people that are capable of directing others that don't have their level of skill. And it's fortunate that we have those people available.

It's unfortunate that we don't have any of those people in DOE. Al Alms was in the DOE in a different division. I talked to him about that. He had kind of expertise. And I asked him why he wasn't passing that on to his people below him, and he said very simply, he was an Assistant Secretary, he said very simply, "I can't. I don't have time. My job is to get money out of Congress."

And I'd like to urge Margaret Chu to get people like this in the DOE, get them in top positions. They can make this project happen. The people that are there now cannot. The people that are there now are going to make the worst disaster we've ever seen on this planet.

CORRADINI: Okay. We're going to break for lunch now, and come back at 10 after 1:00. See you in an hour or so.

Thank you.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

CORRADINI: We'll get started with the afternoon session. I have one announcement I wanted to make. I wanted to announce that there will be two meetings of the Board Panels next month. On February 24, there will be a joint meeting of the Site Characterization and Repository Panels on seismic issues. Topics will cover earthquake ground motion, pre and post-closure analysis and design, as well as drift stability and structural response.

The following day, on Tuesday, February 25th, the meeting on the operation of the overall waste management system will be held on topics such as waste acceptance, transportation, repository receipt and emplacement underground. Jeff Williams, in fact, made mention of that in his presentation.

Both of those meetings will occur in Las Vegas at the Best Western Tuscany Hotel, again, on February 24th and 25th, Monday and Tuesday. If you want more details, you can essentially look at the Board's website and we'll provide the details there.

I'll turn over the afternoon session to Professor Duquette. Dave?

DUQUETTE: Thank you, Mike.

Most of you know who I am. I'm David Duquette. This afternoon, we will have five presentations, beginning with an overview of corrosion studies sponsored by the State of Nevada, followed by an update on the progress of materials testing at Lawrence Livermore. The third presentation will cover waste package manufacturing and closure.

Following the afternoon break, an invited speaker will address us on the influences of paleosols on fluid flow and solute transport. And the final presentation of the day will describe the planned analyses of the waste isolation capabilities of the barriers within the Yucca Mountain repository.

At the conclusion of that presentation, Dr. Corradini will entertain any comments that members of the audience may have to direct to the Board, and I might remind you to sign up at the back of the room if you do have some comments to make.

The first presentation this afternoon is on corrosion studies is on corrosion studies sponsored by the State of Nevada. It will be presented by two individuals, Dr. Roger Staehle and Dr. Don Shettel.

Dr. Shettel has been a consultant on high-level nuclear waste disposal since 1986. He's currently with Geosciences Management Institute in Boulder City, Nevada. He's a consultant to the State of Nevada. Previously, Dr.

Shettel has consulted on low-level radioactive waste disposal in the United States, and on high-level radioactive waste disposal in Canada. He's also worked in various capacities as a geochemist. He has Master's Degrees and Ph.D degrees in geochemistry and mineralogy from Penn State, and an undergraduate degree in geology from the University of Michigan.

Dr. Staehle is an adjunct professor at the University of Minnesota in the Department of Chemical Engineering and Materials Science. He's the former dean of the Institute of Technology at the University. His research interests include predicting the corrosion performance of engineering equipment, stress corrosion cracking, passivity and corrosion in aqueous environments. He's a member of the National Academy of Engineering, having been elected in 1978, and is the recipient of the Willis Rodney Whitney award for outstanding contributions to corrosion science in 1980.

And, with that, I'll turn the floor over to Dr. Shettel.

SHETTEL: I'll stand over here and try and stay out of people's way.

This diagram represents an outline of my talk, as well as several processes and types of water that I'll be discussing in some detail.

Types of water we have first is precipitation, which is rain and snow. The second is fracture flow water, or sometimes referred to as vadose water. Matrix water is what's contained in the pores in the rock. Four is refluxing zone, which represents a mixture of vadose water and pore waters, and basically everything that's above the repository. There was a question this morning about how do we get the matrix or pore water out of the rock. And, basically, it's heat from the repository, and then this water basically comes out through the fractures and micro-fractures, and in some cases, there may be micro-porosity around some of the micro-fractures. And from there, it can be mobilized by heat, driven up by heat, and can move down by waters infiltrating down from above.

There are two types of waters that are below the repository, perched water and ground water. And I won't talk about these very much at all.

Types of processes that can occur at Yucca Mountain. Starting at the top, we can have soil zone interactions, we can have vadose water-rock interactions in fractures, and then something that's not discussed very much at all until somewhat recently, we have an optimal biotic growth zone that forms somewhat of an umbrella over the heated repository, and this growth or microbiological activity seems to peak around 45 degrees centigrade.

Inside of that, we have the water-rock interactions that are within the refluxing zone, and this involves boiling of water at the bottom where the temperature is highest, and up above when that steam goes up in fractures and whatever, and it condenses and you can have dilute condensates, and mixtures of just about anything and everything in between that.

And we have in-drift processes, which Dr. Staehle in the next presentation will talk about in some detail, but I will illustrate some examples of this. And, finally, mixing of vadose and groundwater down below, which I won't talk about anymore.

These are the water types that I mentioned before. A little more information on them. Precipitation is dilute calcium bicarbonate. Fracture flow of vadose water seems to be a sodium bicarbonate. We don't have a lot of samples on this. Composition is generally unknown. And then the matrix water in the vadose zone is what I'm going to spend most of my time talking about. And above the repository level, this is a calcium sodium chloride type water. Below the repository level, it's a sodium bicarbonate water.

And then in the refluxing zone, as I mentioned before, is the heated zone, we can have mixtures of all types of water, except those that are below, meaning groundwater and perched water. And we can have quite a range in composition of this water from dilute condensates to concentrated brines.

Now, this is a cation ternary of a Piper diagram, but basically we're showing the relative proportions of cations such as calcium, magnesium and sodium, plus potassium. The main point I want to make with this slide is that there are two major types, we have a number of different water types up here, but there are two major types of pore water. These are the waters that are above the repository level that are enriched in calcium and magnesium, relative to those that are below. And then below the repository level, we have the sodium bicarbonate type water, this is J-13 for comparison, and we have a pretty good break-down of the two types of water above and below the repository level in terms of cations, in terms of anions.

I forgot to mention the ECRB are cross drift waters in here that are essentially along the cross drift. They are a semi-horizontal plane, but these are in the Topopah Springs repository level, but they're slightly above the repository level, so they also fit into this pattern of pore waters that are above the repository and that are somewhat more calcium magnesium enriched than those that are below the repository level.

The same type of diagram for anions, looking at sulfate, chloride and bicarbonate. We seem to have a better break-down here. The waters that are above the repository are more chloride and sulfate rich. Below is essentially groundwater type. We have very bicarbonate rich waters. The ECRB waters do not plot on here for a reason that I will get into on the next diagram. And for comparison purposes, we have the UZ pore water of Rosenberg, Gdowski and Knauss, which is actually from Sonenthal, falls right here about in the middle of the range of above the repository pore levels, and then J-13 down here falls about in the middle of the range, pore waters that are below the repository level.

Now, the reason that anion concentrations from the ECRB, or cross drift waters, I didn't show any data for that on the previous slide was we have chloride and sulfate over here. These are the measured bicarbonate concentrations, but the ionic balance for these waters is very poor, extremely poor, and this is due to the presence of some acids, propionic, fulvic and acetic acids primarily. And if you recalculate the data to achieve an ionic balance by adjusting the bicarbonate values, you get this green line. And then essentially the difference between the green and the blue line bears some relationship to the amount of organic acids. And this is unlike pore water that is extracted from cores from drillholes, and these are short cores that are taken in the cross drift, the tunnel, and the question is what causes the presence of these organic acids. And I'm speculating it might be a microbial or fungal process, and I'll get a little more into that in the conclusions.

Next we're going to look at some of the environments. Most of you are familiar with the soil zone. Vadose water rock interactions and fractures involve silicates, carbonates, and I remind you that we still have a trace element problem as far as corrosion goes. There are some minor minerals in the fractures that contain lead and other elements that are not favorable for corrosion.

Again, this optimal biotic growth zone, which forms a large rather diffuse umbrella above the emplacement drift and outside of the refluxing zone, because it's a lower temperature than the refluxing zone, but it requires wet and warm conditions to promote the maximal growth of bugs. It also involves the loss of nitrate, phosphate and possible can enhance sulfide by sulfate reduction, and it's also a very dynamic zone because it will migrate with the temperature as the repository heats up and then cools down.

One of the more important zones above the repository is the refluxing zone. And, again, this is a mixture of vadose waters, pore waters, infiltrating waters from above, precipitation, whatever, whatever can get down to that level. We can have precipitation of minerals in the boiling zone, dissolution of minerals in the condensation zone, as well as heated water-rock interactions, and also can have a dynamic position with temperature, again, because the repository heats up and cools down from the thermal pulse.

In-drift processes, which Dr. Staehle will discuss in more detail. Most of these you're familiar with, except these bottom two, which are turning out to be very important. We can have acid volatilization if solutions, brine solutions, or whatever, that hit hot metal surfaces, and if the salts that result from hitting hot metal surfaces dry out and later rewet, we could have hydrolysis of salts. And this can result in very low pHs, even negative pHs.

Some quick examples of in-drift processes. We might be able to form soft stalactites from a dripping fracture, as well as salt deposits on hot metal surfaces from evaporation of water. But these stalactites might break off occasionally and contribute to the salt. And, again, if the salts dry out and then later get rewet, you have hydrolysis of salts, you can form very acidic solutions.

The diagram on the right, we can have rock fall as well. This could cave in the drip shield and essentially form a funnel where you are funnelling were funnelling all the seeps from above down onto the drip shield, and you could also have condensate return from under the drip shield coming back onto the canister.

A few more. We can have the steel sets collapsing onto the canister from a rock fall, or whatever. Again, we have avenues for condensate to return onto the top of the canister. And, finally, rock fall itself can create depressions in the top of the canisters, and these might form evaporation pans, again for the evaporation of water on top of the canister and the formation of salts, and acids that could be emanating from the high temperature evaporation of salts. And these are some of the processes that we envision could happen in the drift, that I don't think have been widely considered by the DOE.

And finally conclusions. Pre-emplacement waters are not very evenly characterized chemically, some better than others, mainly because of the number of analyses. Vadose zone matrix pore waters are extremely variable. They are calcium rich above the repository level, sodium rich below the repository level. And above the repository level, you have the highest sodium chloride levels and the most variable nitrate.

Cross drift waters apparently have been affected by man. And microbiological activity more than likely is producing these organic acids. The question is has man introduced the bugs to this environment, or is the mining environment created by the tunnel, enhancing the growth of bugs that are already there. And I think the USGS is probably working on this question, but it's important issues that needs to be resolved, because it involves acids.

Post-emplacement waters above the repository evolve from mixtures. Pre-emplacement waters modified by biotic growth zone, refluxing zone, and in-drift processes. Now, the importance to corrosion from these waters is that these post-emplacement waters cannot be characterized or sampled or analyzed. The question is could they be modelled? Certainly one could try to model them, but they involve a number of complex processes, thermal gradients, microbiological activity, and essentially non-equilibrium thermodynamics. That makes modelling very difficult.

And then the corollary to this is sub-boiling, immersion testing of EBS materials in groundwater is both unrealistic and non-conservative. And, by that, I mean the DOE wants to put the repository up in the vadose zone, but most of the testing of EBS materials, and certainly essentially all of the published total systems performance assessment of EBS materials is done as if the canisters are down in the saturated zone. So, there's a serious disconnect here, and this is a serious error in thinking.

And that concludes my talk if there aren't any questions.

DUQUETTE: We'll take the questions after both talks.

STAEHLE: I'm Roger Staehle. I'm going to talk about the corrosion part of this. We have a relatively short time, and I'm going to focus to very specific ideas. So, some of the slides that are in my pass-out, I'll let you take a look at and read.

I want to focus attention on this figure, because this is the central issue, or represents a diagram of the central issue in the corrosion behavior of these containers. And, essentially what we're looking at here is a surface of the container with some deposits on the surface. We're looking at heat passing through it. We're looking at a system which has oxygen in it and, therefore, it's going to have a gradient and electrochemical processes.

We're looking at the input of chemicals from the mountain, as Don discussed, metabolic processes, human intrusion chemicals, deposits from dust that we discussed just before lunch, radiolytic processes.

The point I want to make, the single important point I want to make with this diagram is that this surface is a very transformative surface. So, whatever comes to it is going to be transformed. It will be concentrated. It will be electrolyzed. It will be heated. And, so, what ends up on the surface here is not going to look like what came down. And this is essentially the central problem in predicting performance of the waste container, and, so, the work that I'm going to talk about deals with some of the chemical processes that are occurring at this surface, not really definitively, but illustratively.

An analogue which is important, perhaps known to some of you, is the analogue of the PWR, pressurized water reactor, steam generator and this crevice where the tube intersects the tube support. And, here is a super-heated zone where chemicals concentrate. Several months ago at Argonne, this subject had 30 of the best people in the world, the best people in the world working on this problem, and with the well-defined environment on the secondary side of this, we still do not understand the chemistry in this crevice, this crevice problem, this concentrating problem, to give you some idea of the difficulty in making predictions.

So, I'm going to talk then about some experimental work that we've carried on in the Nevada program. This is work that's been conducted by Dr. Pulvirenti and Professor Barkatt at the Catholic University. The approach here is to characterize local environments. But, as I say, what we're looking at is just a very thin slice of what's going on at the surface, but very nice work.

We're going to look at the evaporated environment, and the residual environments. We're going to start with solutions that were identified in a paper by Rosenberg, Gdowski and Knauss in 2001, and take the compositions that they identified as a starting place. And these are compositions of waters that are both from the saturated zone and the unsaturated zone. We're going to compare the concentration, the properties of concentrates and residuals from these locations.

These are the sets of concentrations. It's not worth discussing this in detail. The thing I want to point out is that in the work, the Livermore work, the Rosenberg, et al. work, they were interested in starting out with a starting point, this is 1x, this just means this is the way the water comes out. This is the saturated zone. Then they worked to develop concentrations at various levels for both the unsaturated zone and the saturated zone.

We are taking these more highly concentrated chemistries containing these species at concentrations like these, and then taking the chemistry still further and concentrating them. So, this is one experiment. I'm sorry this is a little bit of a problem here. But, essentially boiling the solution, and then measuring the instantaneous pH of the solution that's evaporated. That's one set of experiments.

And, to show you the result then of this set of experiments, the experiment here is now--this is the pH of the condensate that's coming out of the flask. This is the volume fraction distilled. And, so, we're progressively starting with, in this case, two cases, one is essentially the direct J-13, it's modified, and a J-13 150 times concentrated. This is the J-13, 150, and this is the EJ-13.

But the point here is the material in the saturated zone as it's concentrated does what? It becomes alkaline, and that's the general pattern I think that's been observed, certainly by the Livermore people as well, certainly a tendency.

Now, on the contrary, if we look at the unsaturated zone, this again is pH, this is the volume fraction distilled, so we're progressively distilling more and more solution. And these are different concentrations. This is a one time UZ in glass. This is the 62 times concentrated glass, same thing in Teflon, and then a higher concentration in glass.

But, the answer is the same, that as we take the UZ pore water, the unsaturated zone pore water, and near the end of the concentration, it becomes very acidic. So, this is what you would be looking at on the surface of the container that's hot, pouring off the chemistry and leaving some residuum, this would be what would be evaporated going someplace.

This is a variation on the theme. This is the pH of the condensate versus volume fraction. But, now simply taking the species in the--this is all from the 62 times concentrations. So, these are the concentrations of the calcium chloride and the potassium nitrate, magnesium sulfate, so on, taking them essentially one at a time. And what happens here is that again in these cases, the pH decreases substantially in the condensate, but the greatest pH lowering results from a magnesium nitrate in the solution. And it seems to be that the magnesium nitrate is playing the biggest role here.

These are the anion concentrations versus volume fraction, starting again from an initial 62 times concentration. This now is the pH of the solution, and these are the concentrations, or the pX concentrations. So, this is concentrations increasing in this direction, and pH going in this direction. So, this is for the individual species. These are the various icons here. And, so, what's happening again is that for these, the starting solution, the pH then is becoming acidic, and this is how the species are concentrating in the condensate.

There's some thoughts here about what's going on. This is, for many of you I'm sure, a clearly transparent idea, that we're essentially getting acidity because we have volatile acidic species coming over in the condensate.

Now, let's look at the beginnings of somebody's bottom line here. We're looking now at the corrosion testing of Alloy 22 in these condensates. So, this is the pore water. The next to last 30 milliliters, the final 30 milliliters. This is from the higher initial concentration, over 1000x, the next to the last 30 milliliters, final 30 milliliters. And these are the corrosion rates measured at

130 C and 90 C. We're looking at corrosion rates, this is in micrometers per year, micron per year. For those of us who are engineers, you can drive this by 25, and that makes it mills. So, that's essentially, what, 20 mills?

So, these are very high rates, and we're dealing here with just the general corrosion. This is not stress corrosion. I'll show you in a little bit it looks an awful lot like a lot of pitting.

Now, the next set of experiments we ran was to take this unit here that was boiling, and transfer it to here where we have this small capsule extractor, so we can look at both the condensate at a somewhat lower temperature, and we can look at a constant temperature solution in the bottom.

The corrosion rates we observed, again, if we look at, for example, in the 62 times solution initial concentration, corrosion rates, what is this, 4 mills a year. If we look at a residual solution in the bottom, let me tell you what the residual solution is like. It turns out when the deposit forms, it has a variety of geometries. And in the interstice of these geometries, there's actually a clear residual solution. So, this is a solution that's at equilibrium, or probably not quite equilibrium, but in contact with this deposit. And we're looking now at a corrosion rate which is, what, 400 mills a year. All this is lower temperature, considerably lower corrosion rates for the

1000x pore water concentrate.

Again, looking at the residual paste, something embedded in the residual solid, and looking at the collection cup, again, relatively high rates certainly in this collection cup at 77 Centigrade, and in this paste. So, essentially what we're seeing in the way of corrosion rates, both in the condensate and in the solid from which the concentrates come, are pretty significant corrosion rates.

Now, we have some pictures from SEM. This is from the collection cup at 78 Centigrade. You can see this as well as I can. It's a bunch of pits. The 144 Centigrade from the Number 21 experiment, you can look at the chart there. But the point I want to make is that the 144 Centigrade, 20 days, this is C-22 in a residual solution corroding fairly rapidly.

And then we've also looked a little bit at Titanium in the same solutions, and the same geometries, residual solution, residual solid, and the collection cup. And in the residual solution, this is at the bottom of the constant temperature system, we're looking at about 40 mills a year, more or less.

There's a little bit of work here on a C-22 specimen that was embedded in the moist paste. It doesn't look so easily accessed here, but clearly, significant penetration as seen by the SEM.

I want to, this is sort of a quick shift, but I don't want to dwell on this very much, this is the sort of experiments that were done by Bergin on Alloy 600, Alloy 690 and C 276. And the point here is he investigated combinations of sodium oxide, silicon oxide, water, and a variety of combinations, and what he discovered was that the region which produced stress corrosion cracking at, what, 300 Centigrade, give or take something, 315 Centigrade, was a fairly narrow region.

Now, the reason I'm showing this is to make the point that the regions in which cracking occur, stress corrosion cracking, are generally well defined, but they're also generally narrow. And, so, the trick in making predictions is to figure out what are the regions where cracking occurs, and see how that matches with the environments. And that's an area that needs some work.

So, in conclusion, the first point, which is I think obvious from the figures, the continued evaporation of concentrated unsaturated zone pore water produces significant acidity in both the residual and condensed environments. Whereas, in the saturated zone, essentially the same kind of experiments produce alkaline environments.

The acidity in these unsaturated zone environments relates in general to the higher concentrations of magnesium, nitrates and chlorides.

These environments, the residual evaporated environments are significant corrosive to both C-22 and Ti-7. Corrosion rates in the general rate of about a tenth of 1 millimeter a year were observed, although we found as high as 10 millimeters a year.

And then the point here that I made before is that the environments that we're studying here are really a small, small subset of the possible environments that are certainly possible, even in our steam generator, which is quite diverse. We're going to do future work to sort some of this out.

And, finally, this combination of the wide range of chemistries and the surroundings, a heated surface, this is the figure I showed you in the beginning, the formation of surface deposits over time will produce corrosive conditions on the surfaces of the container that can't be readily quantified nor their effects on corrosion predicted.

And, I think that essentially, the intellectual problem we have here is the problem of bounding, the surface chemistry bounding the corrosion, and we can't even bound it in a well defined system like a steam generator. I think we have to ask a pretty serious question about how easily we can bound the situation on the surfaces of these containers.

DUQUETTE: Thank you, Roger. Questions from the Board? Dan?

BULLEN: Bullen, Board. I actually have questions for both of you. Maybe first, Don.

I was actually interested in your optimal biotic growth zone at 45 C. that's kind of going to be out in the periphery there. Is that also limited by both temperature and water and food availability? I guess I'm just interested in a little bit more information about that.

SHETTEL: Shettel. And the answer is yes, all three. The most important thing is water, moisture, and the next important is nutrients, such as nitrate and phosphate.

BULLEN: Okay, thanks. I'll move to Roger just for a quick one here.

You notice that actually all of these environments that you find are evolved in a very high temperature regime. Would you expect to be identifying a different set of conditions, or a less aggressive environment if the temperature of the waste package never exceeded 85 C.?

STAEHLE: Some of these aggressive conditions were at 77 Centigrade.

BULLEN: But the precursor was basically a refluxing boiling in the Soxhlet Cup.

STAEHLE: Yes.

BULLEN: And, so, I was just curious as to whether you'd expect to see those types of conditions if you never got to the condition where you had reflux, not to say that there's not water moving at 80 degrees C., I understand that, but I guess I'm just wondering how aggressive are you expecting the environment to be without going to those high temperatures?

STAEHLE: Well, Dan, as you know, I learned the fine art of hand waiving from many of my friends, and participated exuberantly with all of them in waiving my hands on subjects. But I think the problem of answering a question like that, which is certainly an important question, is that we really don't understand the system very well. And, so, for me to speculate on, well, it could be this, and it could be that, yeah, we could do that, and someone else would come up here and, well, it could be this, and it could be that, and I think what we need to do is to develop a more rigorous set of structures, intellectual structures, fundamental structures, about how we consider how this behaves.

And what I wanted to point out with that first slide was that what we're looking at on the surface of the metal, regardless of what the temperature is, and so on, is a very transformative kind of a circumstance. What comes in and what comes out are quite different, and we know that from the PWR experience. So, I'd be reluctant to--I mean, maybe we need a case up here to work on this.

BULLEN: I'll take you up on that. But, actually, the one last question that I have, if you go to your conclusion slide, could you just slip that back on there, that last one, the one that says conclusions?

STAEHLE: Yeah, the conclusions? Sure.

BULLEN: Yes. I'm intrigued by the very aggressive nature of some of the last things that you said. Specifically, if you take a look at Number 3, where it says you observed rates that are as high as 10 millimeters a year, this is one that if you picked the right environment and you put a waste package in, in two years, you ought to be able to drill a hole in it. So, this is something that in a real term field test, if you can do it, would show you that you've got a problem. And I guess the question is can you dream up a scenario, or identify a scenario, where you, I don't know, sparge water into the drift, or something, so that you've got enough concentration effect to do that? And would you expect the ability to develop a realistic scenario that shows these kinds of things during--I mean, in experimental phase?

STAEHLE: Well, I think it's possible certainly. I mean, to answer your question, yes, it's possible to develop that.

My concern, my fundamental concern about this is our capacity to think about how to deal with this. I mean, we're looking at a rifle shot, it's almost a one dimensional kind of set of data, out of the multiplicity of things that can occur, not only when you consider the issues that Don has raised about what's in the water above the container, but also this transformative process which if you take a given nuclear plant and look at two adjacent heated crevices, you'll find totally different chemistries. This is in a system which you will know is a well defined outside bulk.

And in a lot of this work, we back off in that industry to taking maybe some simpler approaches of trying to keep the water pure and being careful, and a few other motherhood kinds of things.

I'm very uncomfortable with speculating at the moment, except to say that by doing one set of I think fairly intelligently chosen experiments, we have produced these kinds of results. I think even with this of experiments, we could afford to do even better, do more, forgetting about the semi-infinitude of the rest of it.

BULLEN: Last quick question. In your distillation and concentration, you drove that pH down to a half or a half, minus 1. Any analyzing nature that you can draw upon that shows those kinds of environments that occur? I guess I'm just trying to grasp what things would be an analogue that you'd say well, here's where it happens in nature. It might be similar. And I'm having trouble coming up with one.

STAEHLE: Well, I think in nature, you'd have to think about places where the sun is very bright, where you are evaporating solutions and you could achieve some kind of super-heat. But rarely in nature do you even have the kind of gradients. I mean, say, for example, the electrochemical gradients we're talking about. And, I think in this kind of a system where you have oxygen and you have metal base, conductive, as I say, I think the problem I see that we as a community have is that we don't have an intellectual structure that we've kind of figured out how to think about this. I mean, as I say, I can waive my hands and tell you lots of things, but I wouldn't believe them anymore than you would.

SHETTEL: Let me answer that analogue. I think one possible analogue would be hot springs, such as Yellowstone, anyplace you have boiling solutions, deposition of minerals and salts.

BULLEN: I guess I was just wondering if the pHs have been measured and have they gone down to, like, minus a half and those kinds of things?

SHETTEL: They can get fairly acidic in geothermal type situations, not necessarily as acidic as we see here.

BULLEN: Yeah. But you're also not looking in the crevice, too.

SHETTEL: That's right.

BULLEN: Thanks, Don. Thanks, Roger. That's all I have.

DUQUETTE: Ron?

LATANISION: You know, I think it's possible to find environments that would cause virtually any material to fail.

Latanision, Board. I think it's possible to find environments, or create environments, that will cause virtually any engineering material to fail. And, so, the question that I have is whether this--what did you describe it as? A one dimensional attempt at this. The question, you know, I can understand how you might generate these environments in the flask and boiler that you've generated. But, it's really not clear to me that in an operating repository environment, these conditions will prevail and generate environments that will be this aggressive.

You know, how do you address that?

STAEHLE: Well, you know, in every experimental program, you have to start someplace. And the start here was to take a very straightforward set of solutions that we had--actually, the Livermore people had done the lead work to identify some of the concentrations, and then to heat the surface, and we know the surface is heated. We know what the inside temperature is. We can calculate heat fluxes. We can make adjustments for coverage. And we can reach some idea about reasonable heat fluxes.

We're not too far off from that actually. I think we're a little bit on the high side of the heat flux. But, you know, we're in a temperature range and a heat flux range which I don't think is that far from something that's relevant.

Do I think that things will happen exactly this way? No. But we haven't even examined the set. I mean, what is the set? We haven't superimposed, for example, potential gradients on this, I mean, the things that, you know, we all know how to do.

And as I said before, my problem, my concern about this, and I think this is partly the question you're raising, is how do we think about this problem? Because it's very clearly a very intensely transformative thing. The introduction of heat changes the crevice story. We don't know much about the kinds of deposits we're going to get, their chemistry, their thickness, their thermal conductivity, and how it changes over time.

So, what I'm concerned about is really certainly the issue you raise about, well, is it reasonable, is this a reasonable idea? Well, we've done reasonable things. We've taken a solution that made some kind of sense. We've used heat fluxes that make some reasonable sense, maybe a little bit on the high side. We have looked at both condensates and residual materials, and it's a, you know, it's not a bad place to start. Is it perfect? Is this what's going to happen? I can't tell you that. I can tell you, though, someone had better start doing this kind of stuff.

SHETTEL: Ron, Don Shettel. Let me add to that answer.

And I think it is a reasonable environment, because if you had intermittent dripping on a canister from a fracture or fault and salt over time builds up there, dries out and then rewets, you can have hydrolysis of the salts and build-up of the salt cake, and these types of things that we see in the flask. So, I think it's really reasonable.

LATANISION: Yeah, I guess I'd agree with that to a point. But if you have, you know, dripping and essentially a flash evaporation process, it just doesn't seem to me you're going to great a voluminous amount of acid or--

SHETTEL: Well, if the brine is concentrated to some extent before it drips and it's already at a somewhat higher temperature, then the flash isn't going to be that fast anymore. It's not like you're just dripping distilled water and you get a flash right away.

But I think the other side of this is you have to ask are the experiments that DOE is doing reasonable? I mean, they're immersing samples in groundwater, sub-boiling. This is really a saturated environment. And I think that we're a lot closer to the real environment.

STAEHLE: Well, but I think in fairness, we have a problem of how do we think about this problem? How do we engineer with it? And this is a complex problem.

DUQUETTE: Mike has a question.

CORRADINI: I guess I'm not a chemist, so I'm going to ask a different question. You have a steady flow, infiltration of water, and you have a steady heat source. Can you come to a situation where you actually have enough energy, or some unit of time, to get to this steady concentration? I can see where you do in a batch process, as you did the experiment. I'm not clear with a hand calculation you can prove you can do it in a steady stead process with the energy that you're producing from the decay of the brines. Have you done that calculation to prove that you're in the ballpark?

STAEHLE: Well, are you essentially talking about the super-heat problem?

CORRADINI: You mentioned heat flux. I think you have a hell of a big heat flux to get this sort of what I'll call evaporative process going. I'm not sure if you have that hell of a big heat flux in the real situation. So, I have so much water coming in and I have so much energy boiling away the evaporate thing. I'd like to see a hand calculation that shows me I'm in the ballpark. Have you done that?

STAEHLE: Yeah, we have done that. And I think that Joe Farmer is going to address that somewhat this afternoon also. But the question of what temperature you end up with, I mean, if you made a perfect insulate on the outside of this, it would get hotter than hates. I mean, that's very hot.

CORRADINI: Right.

STAEHLE: So, the question is what's the interim coverage situation? What's the interim insulation situation? And, so, the temperatures you get depend an awful lot on what you assume about coverage on the outside. And, again, I think it's a problem of how we think about it. It's the same essential question that I responded to Ron about.

CORRADINI: Right. I guess in some sense I'm kind of with Ron over here thinking, okay, so you've created a water chemistry environment which seems quite interesting, but I'd want to make sure that somehow it fits in within the sphere of what's possible. So, that's why I was asking from an energy standpoint if you'd done the calculation.

STAEHLE: We actually did a series of these with various coverages, and found this is not--this kind of thing is at the high end of what we consider to be a rational coverage. But, I wouldn't want to dispute--I mean, there's a lot of ways of making those calculations, and I think we need a significant serious enterprise here to work some of that out.

DUQUETTE: We had a question from the--Dick first, and then Dave of the Staff.

PARIZEK: Parizek, Board.

A question to Don regarding the organic acids. Were they inferred to be present, or have these been measured? I think you showed us a graph that suggested there might be some organic interaction.

SHETTEL: I believe that data is from a paper by Peterman and Marshall, and Brian Marshall was here this morning, perhaps he can answer it. But I believe they were measured.

PARIZEK: Okay.

SHETTEL: If not quantitatively, at least qualitatively.

PARIZEK: And then you gave some pore chemistry of water above the repository level and below. Excluding the groundwater portion below, do you understand why there would be differences in those waters?

SHETTEL: Yeah, the chemistry is controlled by the rock types.

PARIZEK: The rocks change that much from above to below?

SHETTEL: Yes.

PARIZEK: And then the question, Roger, about the question of dripping water from condensation versus dripping water coming through the rock mass above, would you be as worried if you knew it was just condensation? You have dusty waste packages, you're sitting in a repository, that's condensation that's dripping on the package versus dripping water coming through the mountain. Would it make any difference in your analysis? We've been hearing a lot about condensation, it's not so bad, but maybe it is bad, I just want to know whether you've thought about it.

STAEHLE: Well, the short answer to the problem is the same answer I've given to Ron and others, Dan. You know, we can imagine water can come down this way or come down that way, and probably the truth is it will come down all these ways. And I don't think that we have kind of worked our brains through reasonable design concepts as to how this would happen, and I'm just uncomfortable with speculating, not that I don't enjoy speculating, but I think in a responsible answer, I just don't feel that, you know, my sort of down deep feeling is that we've got a lot of water out there. We're heating up the roof, and water is going to move, and we know that. Now, what it does and how it does it is just not clear to me, I mean honestly.

DUQUETTE: Dave, you had a question?

DIODATO: Yes, a couple questions. First of all, I appreciate your efforts to think about the aqueous geochemistry of the near-field environment. I think it's useful kind of thinking.

One of the things that I gathered, and I'd like you to tell me if I'm right or wrong about it, is that there could be some heterogeneity in the geochemistry in the near-field environment. Is that correct with the microbes, and different things going on, you could have some heterogeneities in the geochemistry?

SHETTEL: Yes, I think if you look at those diagrams, you'll see there's quite a bit of heterogeneity. But the groups separate fairly cleanly. And there is some overlap as well.

DIODATO: One of the things that's not in your presentation that I've been thinking about lately is redox potential. Is there heterogeneity in redox potential as well, you see a range of values of redox that you might, you know, from oxidizing?

SHETTEL: Most of those species are not redox sensitive other than sulfate, and in the vadose environment where you have a gas phase, it's oxidizing.

DIODATO: And the microbes would never alter that in any way?

SHETTEL: Micro environments involving microbes could alter that, yes.

DIODATO: Okay.

STAEHLE: Well, you know, basically you have an oxygen saturated ambient, and once it gets out of there, then you have to deal with the oxygen problem on the surface. But you're thinking about the oxidation state of the species in the rock.

DIODATO: Yeah, and the waters, and some of the water-rock interactions, and that sort of thing. Which gets us to the second part, which is kind of a--your use of the term pore water, do you mean water in the matrix in the vadose zone, water in the interstices of the matrix in the vadose zone?

SHETTEL: Yes. All of the data that I show is basically from the USGS, and that is pore waters are squeezed from the rock, just like Mark Peters talked about this morning.

DIODATO: This is just a plea, and that is that this problem is complex enough without adding extra terminology. We have the unsaturated zone and saturated zone. It's the same as vadose and friatic. And the program has really, you know, used unsaturated zone and saturated zone. So, that's an easy way to think of it.

Both unsaturated zone and saturated zone have igneous rocks that are fractured to various degrees. So, there's waters that exist in the matrix in the saturated zone and in the unsaturated zone. So, the statements like vadose water-rock interactions in fractures, I mean, it seems like it's terminology that confuses things unnecessarily. So, if you'd just stick with, you know, matrix water or fracture water, unsaturated zone.

SHETTEL: Well, no, I think we have to add the modifier that specifies whether it's above or below the water table.

DIODATO: Right.

SHETTEL: I think we have. Vadose means above the water table.

DIODATO: Yeah. But you said vadose water in fractures.

But what you really mean here is matrix water coming into the fractures. Anyway, my point is that this terminology, you've created some confusion.

SHETTEL: Well, yeah, you're right. It is very complicated, and that's part of the problem. We're dealing with some complicated processes here, and they need to be explored.

DUQUETTE: Priscilla, you had a question?

NELSON: Nelson, Board.

Roger, we've heard from you before a couple of times, and I must admit after this presentation today, particularly I think the difference between J-13 and pore water from rock above the water table, I'm getting the feeling that there's no hope in understanding water evolution that's going to happen in this repository. So, I want you to make me feel that I'm wrong. Will you do that?

STAEHLE: About this?

NELSON: About what I just said.

STAEHLE: Well, I think, as I said, to me, we have, as a community, we have a big intellectual program. I think this is complex. I mean, I've spent 30 years of my life working on this steam generator problem, which is very complex, where the system is well defined, the outer system. I look at this and I say, you know, this is more complex because there's more things going on. And the input chemistry is highly variable.

And on the other hand, I think that it's a reasonable idea that we could, with the talent that I'm aware of here, and probably talent that I'm not aware of here, that we could develop some intellectual structure, some thought processes to say, well, look, this is essentially how we have to think about this problem. It has to do with temperatures unidentified, and others, and I think we can come to a way of developing a conceptual framework that acknowledge this complexity. So, I don't think that's an impossible problem.

I don't know that we can be perfectly predicting, but I think we can deal with the complexity in an intelligent way. So, you know, this is just me, I don't think anything is impossible.

NELSON: Nelson, Board.

Well, when the project is there, tell me.

STAEHLE: I'm sorry. What?

NELSON: When the project is there, tell me.

STAEHLE: When the project is there?

NELSON: Is at that place where it does understand everything.

STAEHLE: Well, nothing is there. But I suggested to several people today in fact that a constructive approach to this thing you're identifying would be to get the people together who are interested, and begin to build a structure that we could work together on that would deal with this complexity, and make some sense out of the complexity, even if we can't be perfectly predictive. And that's kind of the system we're in. I think this is a very great challenge. I don't think it's an unreasonable challenge. I mean, people have seen challenges like that before.

DUQUETTE: Thank you, Roger and Don.

That brings us back on time. The next presentation this afternoon is by Lawrence Livermore. It will be given by Dr. Joe Farmer. Dr. Farmer obtained his B.S. at Virginia Polytech, his Ph.D. in chemical engineering at Berkeley. He's had a number of years of experience in a variety of basic and applied research endeavors, including electrochemical processing, corrosion, electroplating, electroforming, optical characterization of films, and a number of other areas. He's the recipient of a number of best paper awards in his field for a variety of different papers. And, today, he's going to give us an update on the materials investigations at Lawrence Livermore and bounding the environment in contact with C-22.

FARMER: First of all, I'd like to thank the Nuclear Waste Technical Review Board, DOE and BSC for the opportunity to speak to you today on behalf of the repository. I am personally a believer in the program, and view this as a privilege to be able to do this.

I'd also like to acknowledge a large number of technical contributions that will be presented in this presentation. This doesn't represent my own work, but I'm presenting this to you, and it represents a lot of hard work by a large number of programmatic scientists and engineers. And a list of those contributors is found in the last two slides of the presentation, or the last two pages of your handout.

The title of this presentation is Chemical Environment Evolution on Alloy 22.

In today's presentation, I'd like to give you our view of what the in-drift environment will be. And this view is based on many years of experience, and we believe that these views are plausible.

In particular, we will concentrate on three types of brines that might be expected in the repository environment, specifically in the drifts. These include deliquescence brines, seepage brines, and calcium chloride brines. I will discuss for you in sequence the testing program related to each of these brine types, and believe that we have a fairly well thought out and methodical approach for looking at the impact of these types of brines on the materials that we're constructing the waste packages, drip shield and repository out of.

I will also then go on and tell you about some new work that we've undertaken to better understand environment-surface interactions. I'm sure, as many of you know, if you go to a Pourbaix Atlas of Electrochemical Equilibria and look for Pourbaix Diagrams, you see a lot of diagrams for single element materials in simple water. So, these are the types of Pourbaix Diagrams that we're used to seeing.

Now, clearly, even such simple information as this is very valuable to us. But, clearly, we need better understanding of the oxide film stability on the surface of the waste package. So, in order to do this, we now have developed a programmatic capability to produce Pourbaix Diagrams for multi-component alloy systems in complex environments as we expect to see in the repository. I think this represents some cutting edge work that the program has done in the field of corrosion science, and I will give you sort of a Whitman sampler of some of the work that we've done in this particular area.

As Roger mentioned to you, we're also going to discuss with you some of our work related to heat transfer. Dr. Gdowski has done a very nice job of assessing heat transfer through mineral deposits on a waste package surface.

As you know, we've been concerned for some months now over the possibility of hot spot development on the waste package underneath these mineral deposits. We have actually done an analysis and will share that with you that I hope will shed some light on this.

As most of you know, the in-drift environment will evolve from a number of complicated interactions. We start out with a number of naturally occurring waters in the mountain. These waters of course get there by a number of routes. But once in the mountain, these naturally occurring waters then undergo complex interactions with radiation heat and microbial growth, and create a perturbed natural environment.

And, of course, it's this perturbed natural environment that the waste package and drip shield actually see. Once we develop this perturbed natural environment, which is of course the topic of today's presentation, we have a stage set for a number of modes of corrosive attack of the waste package and drip shield. These modes of attack include uniform corrosion, localized corrosion. The localized corrosion, of course, could be manifested either as stochastic type pitting processes or crevice corrosion, and also stress corrosion cracking. And perhaps more appropriately, we should view this as environmental cracking, where it could be stress corrosion cracking or hydrogen induced cracking. These modes of failure come together to give us an overall waste package performance.

Of course, the in-drift environment will determine the longevity of both the waste package and the drip shield.

And as we seek to understand this in-drift environment, it's important for us to appreciate some of the very basic aspects of the repository that we seek to build.

First of all, the drifts that we're going to construct are in an unsaturated zone of highly fractured welded tuff. Secondly, these drifts comprise an open system. That is to say these drifts, during the preclosure period, are free to communicate gas with the outside world. So, it is, in fact, an open system, and this has some important implications in terms of the types of environments that can actually evolve inside the drift.

In order to fully characterize the drift, we've long realized that of course we need to have a detailed understanding of the temperature of the drifts, the relative humidity, the chemistry of aqueous solutions that occur in those drifts, and we have to develop an understanding of how these aqueous solutions plausibly interact with metal surfaces.

And just perhaps a thought for the day. It was occurring to me as I was listening to the last presentation there are a number of things in life that are possible, but only a limited number that are actually plausible. For example, I can tell you that I pilot the space shuttle. That's certainly possible, but for those of you who know me well, that is not at all plausible. So, I think we have to keep this in mind as we screen a number of these waste package environments, because as Roger pointed out, they're very, very complicated, and a large number of them, so we have to use, frankly, some good common sense in terms of screening these.

As we look at the in-drift environment, we of course started out with a very large menu of waters to choose from. I believe the ones that are most relevant to the wide predictions for the waste package and drip shield include fracture flow types of water, pore waters, as Roger has discussed with you, seepage brines, brines that can evolve inside the repository, perhaps from fracture flow, and deliquescence brines.

The first step in actually understanding this in-drift environment is to use some of the expertise that's evolved over the years to make reliable predictions of temperature as a function of time. So, the waste package temperature is a key in understanding the evolution of the waste package surface environment. This curve was actually taken from the SSPA document, and as most of you realize, there's a peak temperature of around 180 degrees during the first 100 years.

One of the first things that I would like to point out to you as we look back at this published chart is we realize that these temperature predictions are quite conservative. This particular calculation makes some very interesting assumptions. For example, it assumes that you have a simultaneous instantaneous loading of 10 to the fourth waste packages into the repository to create these temperature profiles. Clearly, this is not the case. It would require that the men at the repository work very, very fast, and probably not practical.

So, we believe that the sequential loading of the repository will probably be something on the order of 30 degrees Centigrade lower than this conservative prediction of temperature.

Linked up with the maximum or the peak waste package temperature during the first 100 years, we have a minimum and relative humidity. And this is very much consistent with what most of us believe.

We, like our colleagues from Nevada, tend to look at the types of water at the mountain in a very similar way. We, of course, have precipitation, fracture water, matrix water, and an in-drift chemistry that we are most concerned with, and finally perched water and groundwater. All of these waters are essentially bicarbonate types of water, with the exception of the matrix or pore water that Roger discussed with you. And these, instead of being bicarbonate types of water, are more typically chloride sulfate types of water.

During the remainder of the presentation, I'm going to discuss with you these four corrodant environments that may or may not occur in the repository, and tell you about the types of experimental activities we have in order to address these four situations.

The in-drift chemistry we believe requires a fairly detailed understanding of deliquescence brines, seepage brines, and calcium chloride brines that have been postulated to evolve from the evaporative concentration of pore waters in welded tuff.

The deliquescence brines result from interactions of dust and water, dust deposits on the surface of the waste package, perhaps up underneath the drip shield, and then we have absorption of water into that deposited dust film, creating deliquescent brines. The operative slide turned a pH of 6 to 9 as being low to neutral pH, but in my view, I would view this as near neutral pH.

We believe that these salt mineral assemblages, or that the salt mineral assemblages pertinent to real dust, are very complicated solutions. They are not just chloride. They have nitrate, bicarbonate and many other ions that serve as inhibitors and buffers of the system. And they're also present on the waste package in relatively small quantities.

We then have seepage brines entering the drifts. These seepage brines can, of course, contact a hot drip shield, hot waste package, and become evaporatively concentrated. These seepage brines can also interact with grout to produce relatively high pH solutions in the range of 9 to 12 1/2.

These brines are also complex, but have present in them buffer and inhibitor ions, which tends to make the solutions fairly benign. And we know that from some of our estimates, that these are volumetrically more important than deliquescence brines.

And, finally, we concern ourselves with the postulated boiling calcium chloride scenario, brines that we would postulate to be somehow produced from the evaporative concentration of pore water if you could take those pore waters out of the pores and move it over to the surface of the waste package. Frankly, we don't know of any good mechanism for making that occur. In the laboratory, we have to use ultracentrifuges to extract this water, and I don't think we have sources of sustained high G in the repository.

If we do, in fact, get these calcium chloride brines occurring in the repository, they're thermally unstable. So, as our colleagues have shown you, as you heat these brines up, you can get dysproportionation of the calcium chloride brines and form hydrogen chloride gas. We know now that when this hydrogen chloride gas is formed in a real drift situation, it's distributed and diluted in the drift gas. Furthermore, once it's in that drift gas, it tends to react with surfaces to undergo neutralization.

I would like to now discuss with you some of the tests and data that are being done in these specific areas. Some of this you perhaps have seen before. But the next three segments of this presentation review work being done on deliquescence brines, evaporatively concentrated seepage brines, and, finally, this class of calcium chloride brines.

The objective of the deliquescence studies are, first of all, to characterize aqueous films that may or may not form on the waste package surface due to deliquescence. But, we in the program believe that this is a likely scenario. And then after we quantify and understand these deliquescent brine films, we then want to understand the modes of corrosive attack that can occur underneath these films.

In regard to the test conditions that we're using, we typically have a fixed relative humidity and temperature, and thus far, we've been looking primarily at two types of artificially deposited salts, calcium chloride and calcium nitrate. We make measurements in a modified thermogravimetric analyzer, a TGA, that enables us to control the atmosphere and the temperature of the sample.

This is a picture of Dr. Gdowski's TGA at Livermore. It, in essence, is a quartz microbalance with samples contained in an environment control chamber. We have temperature and RH sensors that allow us to have a precise knowledge of exactly what type of environment the samples are seeing. This particular apparatus is sensitive to weight changes on the order of ten micrograms, and is capable of operating up to temperatures of 150 degrees Centigrade, which of course are relevant to repository conditions.

If you take this ten microgram resolution and translate that into a thickness of corrosion deposit or film, it has a thickness something on the order of 100 to 1000 angstroms. And just to convey to you sort of the order of magnitude of this thickness, the optical penetration depth of light into a silver reflective surface is on the order of 100 angstroms. So, these are very, very thin layers. So, Dr. Gdowski's instrument has a very high degree of resolution.

These are some data from the quartz microbalance for three different temperature levels, 100 degrees Centigrade, 125 and 150 degrees Centigrade. And this illustrates for you the process of deliquescence on salts deposited on the waste package surface. This deliquescence process actually has two sequential steps. The first step in the deliquescence process involves the absorption of water. So, during this absorption of water, we actually have a net increase in the mass of the suspended metal sample, the sample suspended from the quartz microbalance. In this particular case, we see a mass increase of almost two milligrams.

At relatively low temperature of 100 to 125 degrees Centigrade, these deliquescence films are relatively stable. Not much happens to them after they deposit and after they absorb water. However, if we go to higher temperature, 150 degree Centigrade, similar to the temperatures that are being investigated by the State of Nevada, we do in fact see that these calcium chloride deliquescence films undergo dysproportionation, and we actually lose chloride mass from the surface of the sample.

This loss of chloride mass is manifested as the production of a very small amount of hydrogen chloride gas in the environment, which is swept away in an open system.

We use energy disburse spectroscopy to actually map small white crystallites that typically form on these metal substrates. And when we look at the stochiometry of these white salt deposits, we are led to believe that the composition is basically that consistent with calcium hydroxychloride.

We do comparative corrosion studies. In addition to looking at Alloy 22, we look at a number of companion alloy systems, sibling alloys, if you will, alloys in the same family. Alloy 22, frankly, is the best of the bunch. So, by doing these comparative studies, it at least allows us to be certain that our corrosion tests are sensitive to the types of corrosion modes that we're trying to test.

In essence, during these deliquescence experiments at 150 degrees Centigrade with calcium chloride, we've seen no localized corrosion of Alloy 22, but we have indeed seen the localized attack of the lesser material, Alloy 825.

These are some quantitative data once again from the quartz microbalance. And, again, we see the characteristic absorption of water into the salt deposit, with the subsequent decomposition of this deliquescence brine at elevated temperature of 150 degrees Centigrade. And, of course, the characteristic profiles that we see for the Alloy 825 and Alloy 22 are similar.

When we look at the micrographs, our photographs of the surface, we of course see the small white deposits of calcium hydroxychloride on the surface. However, we see no localized attack of the Alloy 22 substrates. However, when we look at Alloy 825, we see a number of pits forming, pits, of course, indicative of localized attack. So, in this deliquescence brine scenario, we see no localized corrosion of the Alloy 22.

This slide is, in essence, a summary of the points that I've made. Again, we see white precipitates form during these deliquescence experiments that are characterized as calcium hydroxychloride. We see no corrosive attack of the

Alloy 22, but localized attack of Alloy 825. When we do see localized attack of Alloy 825, that localized mode of attack is normally stifled, which means it ceases to penetrate the surface.

I'd now like to turn some of our attention to the seepage brine scenario. As we mentioned before, fracture waters and many of the other brines that we might expect to find in the repository or near the drifts is of a bicarbonate type. We've done many, many types of evaporative concentrations over the years, and this is one that you have probably seen in past times. But, this would be representative of the evaporative concentration of a typical bicarbonate type seepage brine.

Generally, as our colleagues from the State of Nevada discussed with you, when you evaporate these types of brines, you generally see an increase in boiling point, in this particular case up to levels of 112 to 114 degrees Centigrade. You see a corresponding increase in pH. The pH will sometimes rise to a level of around 12, 12 1/2. You see the simultaneous increase of both the chloride and the nitrate concentration, and this is very important because chloride is an aggressive ion that actually brings about localized attack. Nitrate serves as an inhibitor preventing localized attack. So, if both of these things are increasing together, they tend to counter-balance one another. So, this is a very important thing to be aware of as you do the evaporative concentrations of these seepage type brines.

We for many, many years have been doing a large battery of tests in the seepage brines. As most of you know, we had a large number of samples exposed in the long-term corrosion test facility, and these are representative of some of the seepage brine scenarios that you've heard about today.

We used similar brine solutions to do stress corrosion cracking tests of various types, and we do measurements of corrosion potential, repassivation potential, and transpassive potential in banks of potentiostats that you find around the program in the various laboratories.

In a nutshell, if we look at realistic plausible scenarios, scenarios where we take a bicarbonate type seepage brine and do an evaporative concentration, if we use a multiple crevice assembly and actually polarize that to a high anodic level, we see crevice corrosion. The very aggressive effects of the chloride anion are offset by the presence of buffers and inhibitors in the solution.

However, if we take an artificial situation, such as near saturation sodium chloride, and we do similar experiments where we polarize it anodically, we can, of course, induce crevice corrosion. So, again, all things that are possible are not plausible, and I think it's very important that we make sure that the environments that we test in are both possible and plausible.

And this is just a closer look at these samples. Again, in an expected environment created by the evaporative concentration of a bicarbonate type water, we see no crevice attack. But if we take a saturated sodium chloride solution polarized at high potential, we can of course induce crevice attack to occur.

I'd now like to turn attention to these calcium chloride brines. We do not believe that this is a likely scenario, but even so, we're spending a lot of time trying to characterize these media and look at their interactions with Alloy 22. The objective of this particular study is actually to measure both the potential and temperature thresholds for the for the localized attack of Alloy 22 in these very concentrated calcium chloride solutions.

In these tests, the calcium chloride concentrations that have been investigated range from 10 to 18 molar, and at 18 molar, as most of you know, a calcium chloride solution starts to take on the nature of maple syrup.

We've also investigated inhibitor levels, nitrate chloride ratios of 0 and .1. The nitrate chloride ratio of .1 of course corresponds to the chloride nitrate level of 10. So, we've looked at solutions with and without nitrate inhibitor, and we've looked at temperature ranges from 45 to 160 degrees Centigrade.

In order to measure the repassivation potential and the transpassive potential, we've used ASTM standard cyclic polarization techniques with temperature controlled electrochemical cells and precision potentiostats. Three generic types of sample configurations are used, prismatic samples, standard ASTM disk samples, and multiple crevice assemblies. And we generally do a large battery of surface analyses after these tests are run.

The open circuit corrosion potential is determined in an independent experiment. We actually monitored the open circuit potential of a specimen in the appropriate solution for a prelonged period of time, and record the corrosion potential at the point in time where that corrosion potential has achieved a stable level.

The repassivation potential is measured by first ramping the potential of the sample to a very, very high anodic value, to a level where we intentionally induce break-down of a passive films, spontaneous depassivation of the surface. And after we achieve that wholesale break-down of the passive film, we then reverse the potential scan and go in the negative direction. At the potential, during the negative going scan, where the sample undergoes spontaneous repassivation, a reformation of this protective outside film, we define that potential at that point in time as the repassivation potential.

And when you look at polarization curves, that's generally manifested as the intersection of the hysteresis loop during the negative going scan with passive current density.

Here are some data at 105 degrees Centigrade in a 5 molar calcium chloride solution. In this particular case, we have data both with and without nitrate. And the important thing to note about this particular polarization curve, in addition to the fact that the passive current density is about 1 microamp per square centimeter, is the fact that as we add nitrate, it pushes the potential required for breakdown of the passive film to progressively more and more anodic levels. So, again, the presence of nitrate greatly stabilizes the passive film for these materials.

Here's a similar test, and in this particular case, a disk sample, again, as we add nitrate to the sample, we have to push the sample to much, much more anodic levels in order to achieve localized corrosion. We also have multiple crevice assembly data under these circumstances. But, frankly, because of the limited time, we wanted to show you one of each type of sample.

We take the threshold potential as the difference between the corrosion potential and the repassivation potential. When this differential voltage, or the difference between the corrosion potential and the repassivation potential collapses to zero, we know that there's a possibility of having spontaneous breakdown of the passive film, and spontaneous localized corrosion.

So, in essence, we measure this potential difference as a function of temperature. And, of course, the intersection point represents a temperature level where one might begin to think about the possibilities of localized corrosion.

In these calcium chloride brines that have been postulated from the evaporated concentration of pore water, there is evidence that the potential difference approaches zero somewhere in the range of 140 to 160 degrees Centigrade, and in this particular case, in the absence of nitrate.

There's a typographical error here I'd like to point out to you. This is reading no nitrate in the heading, but it's actually nitrate added at a nitrate chloride ratio of .1. Here again, we see the intersection of the difference of voltage line at a temperature of around 140 to 160 degrees Centigrade.

As I mentioned to you before, we want to develop a more detailed understanding of the interaction of the waste package surface environment, or the in-drift environment, with the waste package surface. In order to get a better handle of this, we have undertaken the prediction of Pourbaix Diagrams. So, we now have region of stability charts that we've generated for the Alloy 22 as a function both of electrochemical potential and pH.

And, of course, since there are an infinite number of experiments that one can perform, and as Roger pointed out to you, the environments are extremely complicated, having such predictive capability is a valuable thing.

There's been a lot said about heat transfer through mineral deposits on the waste package. It's been said that there's a possibility that the waste package surface temperature might rise perhaps hundreds of degrees to very high levels, and that this might somehow drive thermogalvanic corrosion. So, you know, we take these types of problems very seriously, so a lot of modelling has been done to address this.

It's also been said that there is a good comparison between the types of heat fluxes that we see on the waste package, and the types of situations that one might expect in an Alloy 600 steam generator.

We have done some searching through the literature, and we know, for example, our own power densities are in the order of .3 to .4 kilowatts per square meter. If we compare this with an Alloy 600 steam generator of the type that we've discussed earlier today, we find that those power densities are about three orders of magnitude higher, .14 to .22 megawatts per square meter.

So, from our perspective, the power densities are vastly different, orders of magnitude different. So, it's hard for us to see on the power density level how these two situations are similar. And, also, the steam generators use Alloy 600, and it's well known that Alloy 22 is a superior material.

We've done heat transfer calculations. This slide and the one following show you the details of those calculations. But, in essence, we have gone in and estimated the--I'm sorry, actually, the surface waste package temperature as shown here is a function of deposit thickness. So, at a deposit thickness of 1 centimeter, we see a maximum increase in the waste package surface temperature of around 3 degrees Centigrade. However, with the thickness of 4 centimeters, we see that the waste package temperature only goes up 13 degrees Centigrade.

So, while it's very important that we do these analyses and be aware of them, this doesn't seem like it shows problem for us.

These are some of the quantities that were present in those equations, and certainly any of you who would like to check these calculations, please do so.

In summary, we believe that there are four types of water that we can concentrate on in terms of the types of water that might impact waste package performance. These include deliquescence brines from dust-water interactions, seepage brines from fracture flow, and these calcium chloride type of brines.

We believe that the deliquescence brines and seepage brines will be expected. And since we have trouble understanding how we can get the pore water out of the rock and over to the surface of the waste package, we actually feel that these are unlikely, but we are continuing to investigate them.

The deliquescence brines from the dust-water interaction have very modest pH values, and no localized corrosion of Alloy 22 has been observed in this situation at temperatures as high as 150 degrees Centigrade. Seepage brines from fracture flow produce fairly benign solutions, and even at the boiling point of some of these evaporatively concentrated bicarbonate type waters, we see no evidence of crevice attack, even when we polarize multiple crevice assemblies at relatively high anodic potential.

We corroborate the results from the State of Nevada, and indeed these calcium chloride brines are very unstable, especially in open situations. You heat them up, they decompose, they form hydrogen chloride gas, and in an open system, this gas would most likely be swept away and perhaps neutralized.

We recognize the importance of this and are continuing to investigate. We've done a lot of electrochemical testing, as you see, in these types of environments, and are continuing to do so, to build our confidence and understanding of this scenario.

So, thank you very much.

DUQUETTE: Thank you very much, Joe. First question is by Mike.

CORRADINI: You said something at the very beginning, it was on your Slide Number 10, where you said acid gas is unlikely. So, you said, first of all, under the calcium chloride brines from pore water, that it was unlikely because you saw no way to physically transport that sort of water, given the composition--

FARMER: Because, in the laboratory, we have ultracentrifuges to overcome the capillary and the surface tension forces.

CORRADINI: All right. I'm with you there. Then you went on to the acid gas composition, and you said the same thing, and then you added a certain thing that I didn't completely catch. You said that in the producing of this, it produces HCL, and it goes off--do I have this approximately right?

FARMER: You take a calcium chloride brine, and that calcium chloride brine, as in Greg's experiments, can be formed either as a deliquescence type film, or it can be formed, as Roger and the folks at Catholic University have done, in a beaker. Either way you can form this type of brine.

When you form it and you heat it up to a high temperature, 150 degrees Centigrade, you begin losing chloride mass from the solution, and this has to effects. First of all, the chloride mass that is lost in the solution along with water vapor, recondenses, and of course that chloride anion has to pick up a cation to maintain electroneutrality. So, as is pointed out, you create an acid gas, or hydrogen chloride, in the vapor phase.

The residual solution that's left behind, you also have to maintain electroneutrality there. So, you've, of course, removed chloride, so that residue left behind, at least in total, if you look at the whole envelope of that residue, would become alkaline. And this, I believe, is why we observed the calcium hydroxychloride left behind.

DUQUETTE: Dan, you have the next question.

BULLEN: Bullen, Board.

DUQUETTE: I'm sorry.

CORRADINI: I was going to ask hopefully an intelligent question. But, can I at least let Ron ask something related to this? Because I'm still not clear what I think you just told me. What I think you just told me was that what we saw in terms of the beaker distillation allows the chloride to reflux back into the residue, where in reality in the drift, it would escape to somewhere else in the space.

FARMER: I believe that's true.

CORRADINI: Is it?

FARMER: Yes. Because if you take a typical Soxhlet extractor, you know, it's in essence a closed system, and that's why you have a Soxhlet extractor.

CORRADINI: It's a constant volume process?

FARMER: Yes, it's a constant volume process, and you actually want to continuously reflux that vapor. Of course, in the drift, that's not what we have. And, also, I might point out that the temperatures of these surfaces where we might postulate condensation, they're going to be high. I mean, I don't quite understand how this condensation occurs.

LATANISION: Latanision, Board.

That was precisely the point I was going to touch on. Your argument is that the acid vapor becomes airborne, but it's swept away and does not condense on any of the surfaces.

FARMER: That's correct. And, also, there are competing surfaces.

LATANISION: And the reason for that is because they're warm?

FARMER: Yes.

LATANISION: But, I mean, are they sufficiently warm? I mean, how warm? How do you know condensation would not occur?

FARMER: Well, I think at the present time, as I understand it, there's about a maximum 10 to 20 degrees Centigrade temperature differential between the hot waste package surface and the drift walls. So, if you start looking at these types of temperature differentials, while you can perhaps say, well, at some point--

DOERING: Addressing this question about the recondensation, we've done some tests at the Atlas facility, and we've actually looked for recondensation on the packages and where would that go. And we really haven't found any that would go back on the drip shield or the package itself. We've looked on the liner where we had the modelling of the drift, and we have found maybe some slight there, but nothing on the packages. They were heated. They were modelled exactly like that. So, we do have some tests that corroborate that.

LATANISION: I guess the corollary then would be why is it not possible to imagine HCL vapor, whether it's in the condensed phase or not, as being corrosive? We know that hot halogenated gases are corrosive.

FARMER: That's true. And certainly I don't want to give you the impression that we're being flippant about this. We are, in fact, involved in a very large, detailed program looking at the viability of producing these types of environments, and then once they're formed, looking at how they interact with the waste package surfaces.

But, you know, from our perspective, instead of taking, you know, a large volume of this sort of synthetic pore water that, you know, you'd probably never be able to collect a volume like that, and then evaporatively concentrating that or turning it into calcium chloride and then decomposing that to make the acid gas, I think perhaps a more realistic experiment would be to look at similar temperature ranges, but actually use rock.

And if you do this with actually porous rock and look at the, you know, if there's some mechanism for the water getting out of the rock, it will, and if there isn't, it won't. So, that would be my view of a realistic experiment.

DUQUETTE: Dan?

BULLEN: Bullen, Board.

First, my compliments on presenting a great deal of information in a very short time. I think I finally found someone that if I could have recorded this presentation, my students would agree can speak more quickly than I. So, you gave us a great deal of information.

Could we go to Figure 7 first?

FARMER: Sure.

BULLEN: And I also want to compliment you on showing Figure 7. The reason I want to compliment you is for not showing me pre-emplacement period where the relative humidity is going up. I like that chopped off. I know it's an artifact in the model, but I always hated it when you showed me stuff that wasn't real. We know in the drift, it's not high relative humidity. Okay?

Moving on to 8, that would be great. I'm very intrigued with your break-down here of expected, expected and unlikely and unlikely. And when you have unlikely and you're trying to do a TSPA, that's something that you, you know, assign a low probability to. So, you still have to deal with it in the types of calculations that you're going to deal with.

If you take, for example, and we move on to the last figure that I want to talk about, which is Number 28, is there a possibility that if you didn't go to 140 degrees C. on the waste package temperature ever, that you're unlikely went to very, very unlikely, or maybe even impossible, and so that you could get to the point where I don't have to deal with it at all under 180 degrees C.?

FARMER: I see where you're headed. I know how Colin Powell feels now going to the United Nations. Clearly, the first thing I want to point out about these--let me also, I would like, frankly, to acknowledge and thank, you know, colleagues from the State of Nevada. You know, we of course have fun with each other, but, frankly, I think they have done a great service to the program by pointing out these environments, and we're investigating them and we're gaining a great deal of insight based on some of the hard work that they have done.

That being said, you know, clearly this is embryonic work, and I say that because you look at the relatively few data points that we have on this curve. Clearly, we're moving in the right direction, but the job isn't finished.

In particular, if you look at where the data points are distributed, you see that there's a great deal of uncertainty between 100 and 140 degrees Centigrade because those environments are very difficult to formulate, and though this is an intermediate temperature range, these are kind of difficult to run. But, we are in fact getting data points in this region right now.

So, our plan at the present time is actually to populate these charts, and this is Dr. Rebak's data, and he's a master at this, but we're trying to populate this chart with more and more data points so we can build up confidence. I would say then at the point when you know very precisely where this intercept point is in this worst case condition that I think anybody can imagine, you then know that if I operate below that, under any condition, I'm never going to have localized attack. That would be a very powerful thing to say.

But, frankly, the reason that we don't like operating the repository at a higher temperature right now is it's unclear to us how you create these solutions. Because, you know, as I said before, in the laboratory, we have to use ultracentrifuges to extract the solution, and it isn't clear to us in the repository how this occurs.

BULLEN: Bullen, Board. Just one last question.

After you've identified all the data and you've shown the thresholds, would it be unfair for me to say that if you operate it at lower temperature, you would have a higher margin of safety?

NELSON: Nelson, Board.

I have a question. Is it not possible, or does your analysis include the possibility that during heat-up, you're moving a lot of pore water around the mountain, and not all of it is going to find it's way to the pillars. There's going to be an uneven deposit of the top of the boil-out.

FARMER: That's correct.

NELSON: You're going to have places where there is pore water that has not drained. Why is it so difficult to imagine reentry of that kind of water into the tunnels?

FARMER: It isn't difficult to imagine. I guess my view is that in the vicinity and close proximity to these waste packages, it's hard to see. You know, in these hot environments where you would worry about this calcium chloride, you know, up at 180 degrees, 160 degrees, it's hard for me to understand exactly how you get this liquid aqueous phase from the pores over to a hot waste package surface, create this concentration process, you know, boil off the HCL. That's a conceptual block I have.

NELSON: Well, I can imagine it.

FARMER: Okay. Well, we are working on it. We're going to work very hard on it.

NELSON: Please. It would be really good. We need to hear about the evolution of thinking about this.

FARMER: You bet. And thank you very much.

DUQUETTE: Thure?

CERLING: Cerling, Board.

I'm just wondering what is the mechanism by which the nitrate seems to neutralize the corrosion problem?

FARMER: You mean mitigate it?

CERLING: Or mitigate it, yes.

FARMER: I think it's basically one of competitive absorptions. You know, the chloride, there are many theories, let me preface it by saying there are many theories having to do with localized attack of oxide films, and Ron and Joe Payer and others are, frankly, much more experienced and expert in this than I am, so I'm clearly preaching to the choir. But one theory is, of course, that the halide anion forms a halide nucleus on the surface of the passive film, and it has relatively high solubility, and that interactions of the nitrate with those halide nuclei can actually inhibit their dissolution and can actually tend to inhibit their formation. So, that's one concept. But, you know, frankly, how proven that mechanism is, I couldn't tell you.

Actually, Raul, could you comment on that?

REBAK: This is Raul Rebak. Yes, there is not an explanation for that, and I don't think anybody knows for sure. Another very logic mechanism, I would say that nitrate reduces to nitrogen, and to lower balances, and in that process, absorbs a lot of protons to form water, so that it reduces acidification in places in which localized corrosion occurs.

ABKOWITZ: Abkowitz, Board.

Let me preface my question by telling you that this is not my field of study, so I'm speaking for all the folks out there that don't have a clue what some of these charts really say.

Having said that, it's clear that a great deal of work has gone into this problem, and you speak with a great deal of competence as to where the program is at this time.

So, to use your nomenclature, I was curious whether you feel that it's plausible that we don't have a corrosion problem to worry about?

FARMER: Is it plausible? I think that--I've learned from Roger Staehle that it's possible, but I cannot yet tell you it's not as plausible.

ABKOWITZ: Well, then what's required to do that, and how important is that to influence the TSPA process?

FARMER: Well, frankly, I think the thing that we have to do in order to--we have to increase our confidence. You know, there have been many, many fronts. This is like fighting a war, you know, getting this PA pulled together, and there are many battle fronts. So, frankly, the resources of the program are deployed on these different battle fronts, and if there's a problem over here, more resources go.

So, I think that Dr. Chu and the program office are now instituting a science program, and frankly I think the science program will do a great deal for the project in terms of building confidence.

For example, you know, we have a very strict schedule and budget timelines that we're meeting. So, we create the data to the extent that we possibly can to fill in this void. But, clearly, there was a question about the nitrate chloride mechanism. Well, we can guess what it is, we can postulate what it is, but there's still a gap of scientific understanding there.

So, I would say that we could tell you with a great deal of confidence that no, corrosion is not going to be a problem under this set of specific conditions, with some additional work. But, frankly, even as I stand here today, I believe we have a very robust repository program, and, you know, I think it's very important to the country, and I think it's been well executed. But that doesn't mean that the job is completely finished.

ABKOWITZ: If I could follow up? I've heard this reference to the Science and Technology Program extensively today. It gives one the impression that this is a $10, $20, $50 million a year program with sustained funding. My understanding is you may have $2 million available this year, and that would coincide with essentially the time frame for which issues need to be evaluated relative to getting into TSPA. Is that correct?

FARMER: I don't know the budget numbers. I would have to defer that to someone else.

DUQUETTE: Normally, we don't have speakers speaking to each other, but the previous speaker and this one seem to be so diametrically opposed, I don't know if either Roger or Donald would like to make a short comment or ask a question.

SHETTEL: Don Shettel for the State of Nevada.

I fail to understand the importance of why you're concentrating on environments that you even yourself consider unlikely, such as calcium chloride. That's not an environment that the State considered important. I mean, Catholic only looked at a few binary solutions in passing. Most of our work is on pore water, which is certainly based on a natural composition.

FARMER: Yeah, Don, we looked at these calcium chloride solutions because when we looked at the range of possibilities, we realized that the boiling calcium chloride scenario was a worst case. So, you know, we wanted to bound the worst case.

SHETTEL: Well, you may think it's a worst case, but we don't think it's a worst case. We think the worst case involves magnesium. And you're not working on that one yet.

FARMER: Well, I perhaps overstated. I would say that, you know, looking at this class of salts, you know, magnesium chloride and calcium chloride, I should have been more general when I said that, looking at these boiling near saturation brine solution, we view those as worst case scenarios, without inhibitor, without buffer. So, we wanted to go in and characterize the material performance in the worst case scenarios. And we have an experimental matrix set up to do that, and as you can see, it's being executed and data is being accumulated.

DUQUETTE: We want to get back on schedule. I think I'm going to cut the discussion here. If it's quick.

SHETTEL: The other involves nitrate as an inhibitor. We think that only applies at a certain range. I mean, if you put these samples in nitric acid, I don't think you could say that nitric acid would be an inhibitor. So, I think there's a range of concentration of nitrate where protection of the alloy occurs, and that you can't just in general say that nitrate is an inhibitor of corrosion under all conditions.

FARMER: Well, that's true. And as I said, you know, it's generally believed that there's a competitive absorption mechanism. But, you know, that would assume perhaps more normal water compositions than nitric acid.

SHETTEL: And we're getting failure at 70 degrees. So, the high temperature part of your experiment is not necessary to induce corrosion.

FARMER: Well, as I said before, you know, I think we have to examine the conditions where you're getting failure. I mean, for example, all of us realize that in a metallographic laboratory, we can use boiling apparegia to digest these, so we can do elemental analysis by atomic absorption. So, it's been known for, you know, quite a long time that you can use boiling apparegia, which is basically what you folks are using, to dissolve these materials. That's not new knowledge. But the plausibility is another case.

DUQUETTE: I think that's enough for now. You two can discuss it in private. Thank you very much, both of you. I may be sorry I opened that Pandora's Box.

The next talk this afternoon will be on waste package manufacturing and closure. For those of you who have the itinerary for this afternoon, there's going to be a slight difference in the speaker. The presentation will be by Jerry Cogar.

Jerry had been Superintendent of Fabrication Operations for Babcock and Wilcox. He then became supervisor for Manufacturing Engineering in the Waste Package Development Department of OCRWM. He currently is working on EBS components, and has joined BSC and is Manager of Materials and Fabrication Technology. A member of AWS, ASME, and Fabricators and Manufacturers Association International.

Jerry.

COGAR: Good afternoon. Jack Cloud in the manager of Analyses and Component Design, which is responsible for both the fabrication and the design of the waste package. He was unable to be here. He has jury duty. So, I'm going to give his presentation to you.

Certainly, the subject of manufacturing and closure of the waste package is a very meaty subject and a thorough discussion would take a lot longer than the 15 minutes allotted here. So, what I intend to do is give you an overview of the recent developments and then kind of a bird's eye view of what our strategy is.

The waste package prototype we believe will play an important and critical role. Experience has shown that on former DOE projects, that early manufacture of the prototypes is necessary to ensure timely decisions relative to the design, procurement and fabrication.

We want to use the prototype as a method to determine reliable qualified suppliers that will be out there.

The prototype will be constructed to the exact requires of the actual production model. It will support decisions such as material availability, the capacity and capability of material suppliers and fabricators, market decisions, quality control, costs, and to buy American issues, and the distances that the waste packages will have to travel when they're shipped from the fabricator to the Yucca Mountain project.

The demonstration of the fabrication process. Experience has shown that the actual fabrication will identify issues that will require resolution. Design changes are likely. Fabricating prototypes gives us time to react, re-analyze, and to redesign if necessary.

Our goal is to ensure that the waste packages can be manufactured in the most efficient, cost effective, and quality-like manner. We are interested in ensuring that the waste packages can be fabricated in normal fabrication shops using conventional manufacturing methods and techniques and machinery.

Waste package prototyping is an integral part of the design process. It's important and is fundamental and it's a tenant of our philosophy and strategy. So, looking at the first prototype, we intend for that to coincide with the LA submittal.

There's significant value and benefit in having a manufactured waste package at the time of LA submittal. It shows material evidence of our work and our progress. It will provide confirmation of design and fabricability, and will help establish credibility in what we are doing. But, we don't want to tie the delivery of the first waste package directly to the LA submittal. It is a first of a kind, first article. There's material availability issues, potential quality issues with the fabricator. There usually is with a first of a kind product. And the logistics. The fabricator is likely to be several thousand miles away from Las Vegas.

There will be 15 prototypes in the schedule, and the schedule, as you just saw, will be on the following page. What are we going to use the prototypes for? We want to use them to verify the closure weld processes, such as the welding system, the NDE, stress mitigation, inerting, leak detection systems, vision systems and the robotics.

Future destructive and non-destructive testing is another use for them, and those will be determined on a need basis. Those could include ring core method tests, ASME proof tests, drop tests, metallography, and so forth. It will be used in a training facility to prove out the weld cell closure processes, and proof test the mechanical handling systems.

It will used to train operators and for the ORR. The schedules provides flexibility. There's one of each of the waste package designs and the possibility of duplicates of whichever one we choose, or that the project future needs dictate.

If we find that we need more practice pieces, it has the flexibility to manufacture quarter scale, half scale, or even the top part of the waste package at virtually the same cost.

So, what are we going to manufacture the waste packages to? We're going to manufacture them to the ASME Code. For the past nine months, we have used ASME Code experts. These experts are Roger Reedy and Rick Swayne. They're recognized industry experts.

The NRC and the YMRP, the Yucca Mountain Review Plan, provide guidance to the review and acceptance of the

Safety Analysis Report stating that they will confirm that the waste packages are designed and fabricated in accordance with ASME Code Section III, Subsection NB or NC, and as found in many sections of the YMRP.

So, the inner vessel, what's the inner vessel going to be fabricated by? It's an ASME pressure vessel that will be designed and fabricated in accordance with the rules of Section III, Division 1, Subsection NC, Class 2, and it will be code stamped.

The corrosion barrier or the outer cylinder is not a pressure vessel. However, because of the importance to the long-term performance, and in order to ensure quality, integrity, and to enhance the credibility of the corrosion barrier, it will be fabricated in accordance with the ASME rules, Subsection NC, Class 2 by the same fabricator, and in the same shop. This is all discussed in an ASME Code position paper, and it is not a code stamped vessel.

The internals. The purpose of the basket is to ensure proper geometry of the fuel and to preclude criticality events, to maximize the amount of fuel in any given waste package. It will be designed and fabricated to the appropriate codes and standards, but it's not a code component and it will not be code stamped.

I mentioned the ASME Code position paper. The paper is currently in final edit, and it's expected to be finalized in the next 30 days. The position paper will articular the ASME Code position, applicable strategy, and a basis for all waste package components. It will describe the rationale for selecting Subsection NC over NB, and it will describe which NC rules will apply to the outer corrosion barrier and which will not. And it will have other significant ASME Code issues.

This slide shown the tentative schedule for the prototype. There's 15 over six years.

So, what's the status of the first procurement? We are in the process of putting together finishing touches on the schedule for the first prototype procurement, and we expect that to be done in the next couple of days.

The fabrication specifications and drawings are nearing completion, and the final version will be out in the next two weeks. The ASME design specification is complete. Pre-qualification document is complete and is scheduled for release the first week of February. The responses to the pre-qual. are due back at the end of February.

The RFP, request for proposal, is scheduled for release in March. The schedule for procurement of the first prototype, it's still under development, as mentioned, but the table below, as you can see on the slide, has the approximate schedule, and we're not going to deviate from that very much.

The sketch here shows a typical waste package, for those who have not seen one. They come in various sizes, depending on the fuel type. But to give you a sense of the scale of the waste package, the length ranges from 12 to 20 feet. They're approximately 4 to 7 feet in diameter. And the empty weight ranges from 40,000 pounds to 75,000 pounds.

As you can see, we have the outer corrosion barrier here. It's Alloy 22, high nickel alloy, the stainless steel inner, the basket, the stainless steel lid with the spread rings, the middle lid, and the outer lid.

We recognize that there are various welding processes that we could use to close the waste package, and certainly there's a lot of discussion on that.

We have analyzed the welding processes that we want to use to close the waste package. We've done that twice. There's two reports out on that, and the waste package closure weld reports. Those reports were done several years ago, so we've decided to reexamine the issue. In the past year, we've hired Dr. Carl Lundin from the University of Tennessee. He's recognized as a world expert in welding.

Dr. Lundin confirmed that the original selection of Cold Wire-Gas Tungsten Arc Welding was the proper vehicle to close the waste package at this time, and that is documented in a paper by Dr. Lundin.

We just completed a six-month, or are completing a six-month value engineering study that resulted in design modifications. This study was commissioned last year to do the study on the waste package closure design. The study is in the final comment incorporation, and expected to be completed within the next couple of weeks. The study results in the waste package closure design modifications shown on the following slide. And we can get to that in a minute, and I will discuss some of the benefits of that.

This is the site recommendation design on this side, and the proposed design on this side. Starting from the inside out, you will notice that the site recommendation design had a four inch deep narrow groove weld. The new design has a spread ring design that has a seal weld there, there, and at the end, a much simpler design.

The middle lid had a full penetration weld in this. It has now a weld that has a 10 millimeter throat. The outer lid, which was at one time a massive lid due to the induction annealing that we were going to do on it, is now a flat lid with a much simpler design there. The weld grove essentially stays the same.

The benefits of this design modification are several. The time in the weld cell was decreased over 50 per cent. It actually went from about 99 hours to 43 hours per waste package. We eliminated the thermal stress mitigation, and depending on the outcome of development programs that are ongoing at the present time, we will use either laser peening or low plasticity burnishing, which is now called controlled plasticity burnishing. It certainly is less complicated from a fabrication and closure standpoint.

We have reduced the risk for the licensing, operations, performance uncertainties. As an example of that, the effects of the thermal stress mitigation have been eliminated. There was always a question of will it cause more harm than good, so we just simply eliminated it.

There was a cost savings of approximately $1 billion. Actually, it was around $940 million. And this, as I understand, recommended by the Project Operations Review Board.

The weld process equipment contracting strategy recognized the need to build weld systems. And when I say weld systems, it's a whole weld cell. It's a welding system, the stress mitigation system, the NDE system composed of UT and Eddy current, the inerting systems, the vision systems, the leak detection systems, the robotics, and the integrated control system, which can be very complex when you have a number of operations.

When we got the scope and we determined that specialized experience and expertise was required to design and develop these integrated systems, we contracted with INEEL, who has developed these kinds of systems for hot cells, and we asked them to design and develop and build the first cell, and the process equipment that goes along with it as a prototype. They have both the experience and expertise to do this work.

However, for various reasons, we recognize the need for a commercial contractor to be part of the long-term strategy as well. INEEL, because of their national lab status, can build the first prototype, but they cannot compete with commercial firms for future prototypes and the actual production models. We want a commercial input to the design and the development of what is doable and cost effective in the private sector. The integration of the commercial contract and the scope of the work is indicated on the schedule on the last slide.

We also realized that BSC needed specific expertise. To do this, we hired a chief welding engineer from Nooter Corporation. He's been on board with us now for about six months. We're also in the process of hiring an integrated controls specialist from one of the premier companies in the U.S., and we hope to have him on board shortly.

The commercial contractor. The commercial contractor will be competitively bid, and he will work directly with INEEL during the building and the construction of the first weld cell. It is planned for the commercial contractor to build the second generation of prototypes and beyond with INEEL serving as a consultant to the commercial contractor. The schedule on the last slide graphically shows the contracting strategy and the integration between INEEL and the commercial contractor.

Again, we firmly believe that the development of prototypes are an integral part of the design process. We expect the design and the development of the weld cell process and equipment to be an iterative process. We intend to have, or at least tentatively have scheduled for five prototype systems.

What's the uses of the prototypes of the weld cell? Well, the five prototypes will be installed in the training facility and used to establish proof of concept and operations, perform closure operations on the waste package prototypes, provide operator training, be used to establish procedures and process for the ORR and the operations, and potentially could be used to perform the ORR. We could potentially use these in the actual fabrication facilities as well, and at closure facilities as well.

The weld cell equipment development schedule, as mentioned previously, the slide will indicate the contracting strategy, the division of responsibility, the durations of the activities associated with the design, development and construction of the weld cell process and equipment.

And as you can see, this slide details what we intend to do. It has the design and develop by INEEL, then the building of the first prototype, which integrates with the commercial contractor. Then the design of the prototypes with the commercial contractor, and the consulting contract, and then the actual ones farther out.

And that concludes the presentation. Are there any questions?

DUQUETTE: Duquette, Board. Would you go to Slide Number 9, please? This is the change in the design from the site recommendation design. I understand why you've done some of the things you have. But, one of them I don't quite understand is what I'll call a support ring, which is the outer ring that will be in the trunions, and so on and so forth. You've gone from a solid piece on the left-hand side to a welded piece on the right-hand side, at least right there. Now, that reduces some of the robustness of the design and introduces two more welding operations that you didn't have before.

Can you give me--I presume it's going to be put on either as a split ring or else as a sleeve that will slide over the main cylinder, and then be welded top and bottom. Can you tell me what the rationale was for going away from the solid design on the left to the component design on the right?

COGAR: Certainly. When you look at this design here, this original design had a weld here that held the top of it on, and it also had a fillet weld at the bottom. If we move over to this, what's actually happened is this has all slid down, and you have your weld here, and then we have a weld up here that holds the top of it. So, we have the same two welds. The weld of the trunion ring to the cylinder has not changed significantly. The difference is that this weld that originally held it, held this part, was a fillet weld here, and now it's like a groove weld over there. And the weld that held the trunion here originally is not shown. This is actually the outer cylinder, and there was a fillet weld here.

DUQUETTE: You've also shown only a fillet weld for the middle lid on the right-hand side, versus a full penetration weld. That will reduce at least some of the structural capability of that unit, I presume, to carry any kind of load. And what was the rationale for that, just making it easier to do?

COGAR: Yes. Well, the middle lid does not have any structural benefit to the package. The middle lid was there simply because at one time, we could not get a compressive depth greater than about 3 millimeters. So, we were not sure that we could get that. So, in order to have a compressive depth of this 6 millimeters, approximately 6 millimeters that we need, we had to do a mitigation on this lid and this lid to achieve that. Now, the data shows that we can get up to about 8 millimeters of compressive stress on the top lid alone. So, we simply don't need the compressive stress on the second lid. It becomes less important.

DUQUETTE: Do you need the middle lid at all?

COGAR: That's a PA question that I wouldn't want to address.

DUQUETTE: Dan?

BULLEN: Bullen, Board.

Could you actually go to Figure 3 real quick? This is just a quick question. You have an ASME code position paper going in. Do you expect it to be developed to a full-blown code case sometime post LA, or are you not going to go that far?

COGAR: My understanding is that we don't need the code case. What we are using is just basically the ASME, Subsection NC, and we don't need a code case.

BULLEN: I would agree. I was just wondering if you were going to carry it on.

COGAR: No, we're not.

BULLEN: It would be an extra expense. Could you go to Figure 10, please? You've got some great benefits in reducing weld time and eliminated a little bit of stress mitigation. Have you thought about going to a single pass option, like maybe an E-beam or a friction stir weld, and cut that time down even more?

COGAR: That's a question that gets asked quite frequently, and it's a good question. There are a lot of faster processes. The problem that you run into when you try to design a weld system is you have to think of the whole system. And the problem we have is we have a remote hot cell heated vessel that you're fitting a lid into. Getting fit-ups in the area of 1 to 2 mills in that situation is very difficult. And traditionally, with EB welding, with laser welding, you need that kind of close tolerance fit-up, and we just can't seem to guarantee it.

Now, are there ways to do that? Certainly, there are. We could heat the lid and expand it out. But now you make the weld so much more complicated. So, if you look at all those things and you look at what happens in catastrophic failures, what happens if the EB blows a hole in it, well, maybe you can repair it and maybe you can't. With cold wire, it's a very safe process. Yes, it's slower, but it's a very safe process, very clean process. And if you do have a failure of some kind, it's also very adaptable to repair that failure.

BULLEN: Bullen, Board. Just one other comment.

I agree that it's a little risky to go with a one pass and the fit-up is a challenge. I would just suggest that in light of what Dr. Chu has said with respect to learning from the international community, the Swedes are doing a great job of telling you that you probably shouldn't do E-beam, and they've gone on to friction stir. So, maybe in three or four years, you might want to take a look at their friction stir capabilities, because that might be something that would be a little bit more amenable.

COGAR: Yeah, friction stir is another problem. The friction stir doesn't work on Alloy 22 at the present time because they can't find a shaft that's hard enough to do it. So, we understand that technology is going to evolve, and we understand that within 10 to 15 years, they're going to replace all the weld systems in the waste handling building anyway, because they wear out if you weld 500 packages a year. So, at that time, the technology in the commercial sector should catch up with us and with what we need, and then you have to look again and say what do you want to do now. And probably it's going to be one of those exotic systems, or what's now exotic systems.

BULLEN: Thank you, Jerry.

DUQUETTE: Any other questions from the Board? From the Staff?

(No response.)

DUQUETTE: If not, let's take a short ten minute break, and it will put us back on schedule. We'll see you all back here in ten minutes.

(Whereupon, a brief recess was taken.)

DUQUETTE: If you don't take your seats pretty soon, I'm going to keep the session until 7 o'clock.

Thank you very much. Our next speaker is Dr. Graham Fogg. Dr. Fogg obtained his B.S. in hydrology from the University of New Hampshire, an M.S. in hydrology and water resource from the University of Arizona--that's quite a switch from New Hampshire to Arizona, as a matter of fact, especially this year with the weather we've had--and a Ph.D. in geology from the University of Texas at Austin, where he also worked in the Bureau of Economic Geology.

He has over 25 years of experience in researching and teaching about flow and transport processes, modelling of heterogeneous subsurface systems and groundwater analysis pursuant to problems such as groundwater contamination, groundwater resource sustainability, high-level nuclear waste isolation, coal mining, and petroleum reservoir characterization and recovery.

This afternoon, he's going to speak to us about the influence of paleosols on fluid flow and solute transport.

FOGG: Thank you, David. Thank you for having me here. I'm grateful to the Board and to DOE for inviting me. I haven't thought much about high-level nuclear waste isolation for at least 15 to 18 years, since I was working in Texas on

DOE projects dealing with salt.

But, since that time, they're doing a lot of work on alluvial fans, and I was invited here to give some perspective on recent work that we've done on alluvial stratigraphy or alluvial complexity, and I also understand there is some interest or concern here about possible occurrence and influence of paleosols on fluid and transport, and we've done some work on that, and I'm going to talk about it.

I'll hit the high points of a number of projects, and I'll show examples of heterogeneity of alluvial environments, how we characterize it, and the consequence of this heterogeneity, which in some cases are a little surprising, and some cases are the way we thought it would be.

It's a body of work that's supported by a number of different agencies who are listed here over about a twelve year period. Most of the work I'm going to show is the work of students, in particular, Gary Weissmann, on paleosols, which will come near the second half of the talk, also Steve Carle on geostatistical modelling of systems, who is now at Lawrence Livermore, and Eric LaBolle on modelling of transport. Those are the main contributors to this work.

And the fans that I'm going to talk about are all in California in this case, and there's one in the Livermore

Valley located here in the coast ranges, and we'll take a look at another one further south in the San Waukeen Valley, the Central Valley of California, a coarse grained alluvial fan in the Kings River Fan System. This is the one that has the paleosols.

And when we look at these systems--also, I'll show you some data from the Salinas Valley that kind of poses the problem. When we take borehole data from these alluvial environments and, say, we have well data, each one of these columns is a well, and the wells can be interpreted from core or cuttings in terms of different textures, muds, muddy sands, and sand, so the aquifers are in red, and by and large the aquitards are the low permeability media are in blue here. These are complex environments. They're not big sand piles or gravel piles. And understanding how fluids will move through these materials entails understanding how these various material types, and sometimes we call them facies, connect up, and what are the three dimensional geometries.

And it's a little bit like this problem you've got two boreholes, one here and one here, and you've got sand and mud, and here mud just means silt and/or clay. So, it's a fine grained environment. Sand, mud, sand, mud, likewise over here. And we tend to want to connect up the dots, consolidate these things, and that's the dashed lines. In reality, it can be much more complicated, such as the stipple, and this sand might not even be connected to that one.

And for the problem of transport and characterization of aquifer heterogeneity, this is crucial. Because in the one case where things connect up, you have very easy flow, preferential flow of fluids and contaminants. In the other case, it's quite a different story. So, the question is how do you deal with that, and I'm not going to go into details of how we do it, but I'll show some generalities.

Basically, we've approached it from a geostatistical approach. It's known as conditional simulation of the geology in this case. And just consider for a moment a reference image here, which could be anything, it could be geologic test or rainfall, and let's say it's reality, we can sample reality at data point locations. We can do geophysics as well. And once we have the data, we have two choices. We can either interpolate the data, or we can do something else.

If we interpolate it, that's akin to Kriging. So, if we interpolate samples from this, we're going to get a smooth representation of reality. For the geology, that's commonly what we do in groundwater, and that sometimes can hurt in the end in terms of the predictions.

What we do is something that's a little bit different. It's called simulation, where we create images that look like that, and honor those data and have a similar degree of complexity. It's a geostatistical method. It's not particularly new. The way we do it is a little bit new. This is a stochastic approach, in that you can generate multiples of these simulations, so you don't just do one.

I'll just show you examples of individual cases and the consequences for the flow and transport in this alluvial framework.

So, the methods that we use are based on something very simple, actually transition probability. We're looking at categories of materials, sand, silts, clays, and so forth, and we looked at the probability, the transition probability, that Material K occurs at one location here, given that Material J occurs there. So, simple spatial transition probability, a very old concept. We've just extended it into the geostatistical realm. You can create transitional probability matrices. You can sample stratigraphy.

And here you've got, you know, three different kinds of materials, but in the general case, you might have more. You can sample that and you can measure the transitional probabilities, and in your handout, these didn't come out, but this is the way it should look. I think it says MOM across here in the handouts. That must have been inserted by Kinkos.

But you can generate transitional probability of matrices that vary as a function of spacing between these points, and actually come up with a very complete one, two or three dimensional description of the spatial variability of the system. That's the kind of heart of the scientific basis of what we do.

And I'll just show you an example of what comes out of it from a case study on one alluvial fan in Livermore Valley. This work was originally funded by Lawrence Livermore National Labs. And you have in Livermore Valley in the area of the labs, an alluvial fan, and we take about a one mile chunk as a study area out of this fan, and I'll show you some of the results. You have borehole data, and the borehole data can be interpreted in terms of textures, mixed textures, clay, silt, silty sand, sands and gravels.

And these different texture types have different characteristic hydraulic conductivities. The sand and gravels are in channel environments. The intermediate hydraulic conductivities, such as debris flows and levies, are in this range here. And then you have a large component in this case of very fine grain floodplain materials, and there's enough data now that the question marks can come off of this, a basically, much lower hydraulic conductivity and much higher volume of materials in the low permeability realm in this system.

And you can come up with transitional probability matrices. This is transitional probability of going from each category to another category. I'm not going to drive you through the details, but just suffice it to say that it's a complete spatial description, in this case, in one dimension, of the spatial geometry and interrelationships between different texture types in the system that have a geologic content.

And we do this same thing in the other two dimensions to come up with a 3-D model of the system complexity that honor as the geologic basics, it honors the data and it honors things that we think are happening out there, but we're not quite sure. So, we can include the hard and soft information.

In this sort of approach, cutting out a lot of steps obviously, results in very detailed models of heterogeneity, but also turn out to be quite realistic. And, so, here is a model of part of that chunk of the Lawrence Livermore Labs that I showed you. The aquifers are in orange here, or yellow, depending on the face you look at. The aquitards are the fine grained materials or the floodplain materials. And if you zoom in on this, you see more detail. In actuality, it's a highly connected network system. This case has no paleosols, the next case will, so we'll talk about that in a moment.

Now, I just wanted to point out what happens, what are the consequences of putting this kind of stuff into your model of fluid flow and transport? Well, first of all to visualize what that aquifer looks like, it is basically a connected network. Envision sand bodies that are encased in a much finer grained system. This is not the ideal pluvial model for this case, but geometrically it fits in terms of how this system operates. If you take a system like that, and I'm going to show a little animation of what happens when you have that complexity in the model, and then when you don't, what's the effect on transport. I'm going to skip ahead because you've seen some of this here.

Each of those material types, we assign a hydraulic conductivity, and there's several years of effort that involves calibration, and actually not much calibration and characterization that I'm skipping over, but the model is quite carefully done. I'm going to show an example of the transport experiment in a part of that box, and there's going to be a homogeneous case and a heterogeneous case. First, we start heterogeneous. The permeability distributions defined by the bases distributions you just says, an instantaneous release of a tracer, an ideal tracer, small dispersivity representing local scale mixing.

And we're looking down on the box. The release is right there, the time is up there. And you get a plume that snakes along these channels and it branches. This will rotate in a moment so you'll see it in 3-D. I'm not sure how well it shows up in this light. But you get a branching plume that flows preferentially along the channels, and it's a 40 year simulation. So, 40 years later, there's still a lot of stuff, solute mass or tracer mass, stuck back here.

Okay, the complexity you see is a consequence of the heterogeneity. The next example is a case where we have the same total flow to the box, but with what we normally do in groundwater modelling, we have effective parameters. We don't have that complexity, and we run the same experiment and with the same total flow, and the results are quite different. And some of you know what's going to happen, but it's still worthwhile to compare.

This is a case with the same total fluid flow through the box, but without the heterogeneity, and it's going to rotate again. It's going to look oblong because there's vertical exaggeration here. The plume is going to look like a cigar moving down here. But, basically, you don't get any of the basic features that we saw before. And this is basically a lot of what we've been doing.

We've been characterizing alluvial heterogeneity, doing these kinds of experiments comparing to field data as well. So, that's just a highlight film on the consequences of the heterogeneity.

The technique that we use can incorporate basically anything you can observe as long as it's somewhat consistent spatially, or stationary. We've used seismic to account for variable dip angles, and so forth. That can be built in.

Here's another example where you have unconformities. You have one package of materials here, another on top, another one down here. This is all quite doable now, and actually very similar things are being done in petroleum reservoir characterization.

Now, for a look at the paleosol case in a more coarse grained alluvial fan, and this is based on Gary Weissmann's work from the Southern San Waukeen Valley, this area right here. It's a fluviallly dominated fan. So, it's a very relief surface as a part of a very flat valley, and this is the outline of the fan system. But, in the field, a lot of people don't recognize that they're standing on a fan.

We have core data from this system, from the USGS core wells. We also have aged dates and other things in this transect that make for an interesting study. The core data, and here's one core revealed to us the presence of paleosols. These red zones in here, there and you see another one there, for example, we'll look at another one close up in a moment, are paleosols. There's a contact between a paleosol and more recent glacial run-off deposited materials.

A paleosol is just an--it's a soil that's developed on a surface due to natural weathering processes. The paleosols in this system tend to be much lower in permeability, and much more laterally continuous than any of the other material types, which makes them potentially important aquitards. So, if you suspect you have these, it's important to look at them and characterize them in the context of the depositional processes that gave rise to them. They're lower in permeability because of petogenesis processes, formation of clays due to weathering, and infiltration of fine grained materials.

But, they are relatively uniformly low in permeability. They're not impermeable, certainly not. They are leaky materials. They are separate. We see them deposited primarily in glacial transitions. The Sierra Nevada, the mountains that are the source area for this fan, were glaciated periodically in the geological past, and during the inter-glacial periods, like now, now we have a paleosol forming on the surface. There's not much deposition of soil, mainly weathering.

During major glacial erosion events and depositional events down in the valley, you had a build-out of fan packages, and basically, it happens like this. Today, it's like this. You have a stream that's incised and paleosols forming on an exposed fan surface. In the geologic past, you had much more rapid deposition. The basin is also subsiding. So, whether you see these things depends a lot on the source area, the rate of subside into the tectonic factors in the area.

But, in this system, the bottom is dropping out on the basin fast enough that during the high depositional events, sediments built out on top of these paleosols and were buried as ancient paleosols. And they are correlatable. In cross-section, you see paleosol zones that can be recognized even on driller's logs, as well as on our cores. They're correlatable because these were, during the depositional and erosion phases, very continuous, and the base was subsiding and they were preserved. And, again, that's why they represent potentially important aquitards in the system.

In this case, we developed a little bit on sequence stratigraphy applied to these kinds of systems, and developed a very detailed model of where these features are. And there are areas, like in this green area right here, where they are not. And that's an area where a channel, a more recent channel due to one of the most recent glacial low stands on sea level resulted in incision to about 30 meters, and then backfilling of very, very coarse grained materials. So, when this happens, you have a potential for vertical fluid flow, and also makes an ideal place for artificial recharge.

So, knowing what's going to happen on a fan like this entails knowing where these are, where these are, and also there's lots of complexity within each of these packages, too, that we work fairly hard to account for. So, in this case, we build a model that included paleosol boundaries between depositional packages. So, there's a paleosol there, there is that incised valley, it has a nice gravel fill on the bottom of it. It is a key feature hydrologically and, frankly, we wouldn't have found it if we hadn't known something about the depositional processes.

Once we understood the sequence stratigraphy, we said these should be out there. We went looking for them, and we found the big one. NSF funded several core holes for us to verify that this was there, that our model worked, and indeed it worked out quite well.

So, using similar geostatistical approaches, we have modelled in this system a multi-scale heterogeneous characterization of an alluvial fan system. There is the incised channel. It has heterogeneity within it, which is modelled there, and it incises through this package here. It's modelled over there. And there's another package down there. So, all these go together to create one, and there's the paleosol surfaces to create a multi-scale model of heterogeneity.

Here, we look at the consequences of this sort of thing probably by looking at groundwater age. Here, we were interested in age mixing in an environment like this. Each different color you see entails a different hydraulic conductivity. The hydraulic conductivities here range over five to six orders of magnitude.

Okay, we take a smaller subset of that, and we've done work on groundwater age and dispersion and transport, and in this transect, we have age dates for each of these wells. So, we've done quite a bit of simulation and comparison, and I'll show you some of the high points of that.

By groundwater age here, I simply mean in the standard thinking, you have a parcel of water enters here, and moves down gradient, and it ages, and it discharges somewhere at an older age.

In our modelling to represent this based on a sample of water at this point, we run our models backwards. So, we do backward tracking along the stream lines. I'll show you results that are backward in time, and that's done specifically so that we can get better information on age. But the transport processes and the mixing, preferential flow, that's basically the same backwards as it is forward.

So, we get things like this. Here's our heterogeneous model. We release particles there from a very small point, and things are spreading out. They're spreading out because of the heterogeneity that we've built in.

There's a paleosol right there. There's a hole through that paleosol that's not in this plane. So, it's not obvious from this movie why things are moving through there, but the particles are moving through a hole that is not in this particular plane in terms of the gray scale characterization.

So, you get a lot of spreading. If you take out the heterogeneity, you get the same kind of thing like what I showed in the second part of that other movie. And, obviously, where these paleosols are and where the holes are and how continuous they are is going to control transport quite a bit in this case.

When we look at the age distributions, we can clock here, when each particle gets to the water table, we have an age for that particle. So, we can calculate with models like this the distribution of ages you would get within a single sample from that well. And here's ten cases and ten different cases of the heterogeneity. But, universally, we see ages, and these are cumulative curves representing groundwater age from individual samples in a model that have also been verified with data.

They basically all look like this where you could have one water sample which has water particles in it that are ten years old, and that same sample, you have water molecules in it that are in excess of 100 years old. So, it's showing tremendous mixing. Some people aren't surprised about this sort of thing, but others are. But, it is an interesting area of research, and it's important for understanding how to interpret groundwater ages.

The mixing doesn't go away. When the water gets very old, it seems to get greater and greater. That's something that we're still looking at. We have compared simulated ages, simulates CFC-11 concentrations, which is an age dating technique that has been used at this site, compared to reported, and we do quite well. There's the one to one line.

However, CFC methods don't tag the old water, the water that's older than 50 years. So, when we compare the actual ages, the actual mean ages from the model to the simulated CFC-11 ages, there's the one to one line. The methodology universally under estimates the actual age, because of preferentially sampling. And it's a problem of the age mixing, and this is kind of an emerging issue with the meaning of groundwater ages.

And what it means basically in a nutshell is if you take a water sample, it consists of not just one age, not even with a narrow error interval, typically, you can expect a wide range of ages. And if it's very old water, the age range will be much greater than that is. But, here I'm talking about young waters, 50 year old waters. Here, you've got some molecules that may have recharged the system in

1923, some came in in 1974, 1969, and if you're concerned about recent contamination, perhaps only a fraction are going to be potentially contaminated. It had broad implications for future changes in groundwater quality.

A bit more on paleosols. Backward tracking results, here, we show the results in terms of year snapshots. At ten years, the particles are there. At 30 years, they're all in the blue. At 50 years, it's where the green are. And at 70 years, it's where the red are. And by 70 years, most of them have exited the system. And, again, the paleosols are these dark regions in here. This is a case with paleosols.

Normally, people don't look for these things hydrogeologically, so they just don't get characterized. So, we did another case where we had the same complexity, but without the paleosols, and did the same experiment, and you get this kind of result. So, here's a statistically identical aquifer to this one. Here's preferential flow and kind of confinement of the transport pathways due to the paleosols, and here's the case without paleosols. So, it can be quite important, and have a big effect on age mixing as well as the initial break-throughs and the mass hold-back due to slow advection and diffusion and the heterogeneity.

So, to summarize, we have a hydrofacies geologic approach that provides an added perspective on hydrogeologic processes from a number of different angles, including remediation, transport, and groundwater age. That technique that I showed you was a transition probability technique where we fit Markov chains to the transitional probabilities. It's in a package that is now available within GMS. It generates heterogeneous models that honor the data and geologic fundamentals.

So, geologically, it's rather rigorous. That approach allows relatively easy incorporation of geologic information. The Kings River fan case shows sequence stratigraphic approaches that include knowledge of potential for paleosol development and the potential consequence are helpful. The paleosols can be mapped regionally in that system, and are aquitard in that system. That won't be the case in every alluvial fan, but it is certainly something to look for.

Typical alluvial heterogeneity leads to significant dispersion of groundwater ages within water samples, even when those samples are collected from short screen interval wells. Significant age dispersion compounds interpretation of estimated or inferred groundwater ages. And conventional, nearly homogeneous models of groundwater flow and transport can be misleading when used to forecast groundwater travel times.

That's all I have.

DUQUETTE: Thank you very much. Questions from the Board?

NELSON: Thanks, Graham. How interesting.

Let me just ask you are you familiar at all with the site, the Yucca Mountain site and flow path?

FOGG: I am an amateur when it comes to the site at this point. I've read things in the newspapers, but I have not kept up with really what has been done. You know, I hear talks here and there at AGU, and so forth.

NELSON: Okay. Let me just ask you do you think that you know enough about how to model heterogeneity of alluvium that--I mean, you've got a tremendous density of boreholes in many of your models that you put together, and in other cases, you're not going to have that density. Do you think that you know enough fundamentally about how such alluvium is put together that models could be obtained and are prepared and verified using smaller numbers of boreholes with some sense of certainty? Is that a good question?

FOGG: Could you get by without more data? I have to say I don't know. I would be skeptical about it. There is--you know, these systems, we actually do a lot with a little data, but I think at the transport scales that I think you're concerned about surrounding Yucca Mountain, you would be talking about doing this at a much larger scale, and moving out into areas where maybe it's one borehole per 20 square kilometers, or even less dense than that.

And, there, it can be--there would be a problem, I would say. You're going to be getting into many different environments. At the minimum, it's worthwhile, considering what we show here, to temper your predictions with sound engineering sense. But, there are things you can do. I know there are folks with the USGS who have been interested in applying these approaches, but on a regional scale, to modelling the flow between Yucca Mountain and destinations south in the system.

So, you can do a lot, but, again, if you don't have much data, it's going to be more speculative, but maybe it will help you put some bounds on your answers.

NELSON: Thanks.

DUQUETTE: Any other questions from the Board? Dick?

PARIZEK: Yeah, Parizek, Board.

Clearly, if there were paleosols present, it would have a consequence compared if you assume you didn't have them and just started the fan as a heterogeneous environment, as a high porosity. It's surely good for transport compared to the welded tuffs, you know, in terms of travel time, and so on. So, you can say, well, I think I have paleosols, you show it in the sense of concentrating your tracer basically. So, in a sense, you can actually have higher concentrations in selected water sampling locations than if you didn't have them present.

So, right away, if you didn't know you had them, you'd end up under estimating how bad the case could be, I guess is one way to put it.

FOGG: Well, higher concentrations in the sense that the paleosols in that example funnel a bit more of the tracer through a smaller area.

PARIZEK: Right. Or if in a part of a fan you found paleosols, there's reason to believe, therefore, that system ought to have them buried in the subset somewhere, right, as you did in your fan studies you had where you found them. You had one, you're bound to have several others.

FOGG: Within those packages in between the main paleosols, there are smaller ones that don't seem to go anywhere. So, you may have, you know, periods of a little bit of uplift and down cutting. So, you have some topography developing in paleosols, and then that all gets filled up, and you have remnants of paleosols sitting around. But those would not be as hydrologically important. You know, an isolated paleosol in this case isn't going to be anymore hydrogeologically important than a floodplain mud. In fact, the floodplain muds might be more continuous.

So, it's the continuity and the permeability that are important. And I would say it could be important for the reasons that you mentioned, and it's also important for understanding how to monitor the system, because when the monitoring programs are set up, or if they do get set up, you have to know something about the fabric, as to where to put your wells.

PARIZEK: Some of the program building was designed to get into bedrock, you know, in kind of a business-like manner, and the overburden alluvium isn't too well characterized, as it would be if you intentionally went drilling for that purpose.

So, in the cores, you showed some views looking for alluvium, alluvium stratigraphy in detail, and then you found it. But, if you rip through that with rapid drilling methods, you may or may not know about it. The color itself would have given you a clue, because your soil profiles give you color changes that are brief, but abrupt, and quite noticeable.

So, if you're watching for it, you might pick it up; right?

FOGG: Yes. And, in fact, the four of these cores were taken by the USGS in the early to mid Nineties. There were people saying oh, it's just a big sand pile. And I don't think so. But, you know, really the data weren't very good to pin that down, and lo and behold, the cores showed tremendous complexity, a lot of sand, but half of it's not sand or gravel. It's fine grained stuff. But that's a coarse grained sand for you.

And the paleosols sat there unrecognized by us for over a year before, you know, the research process began to piece together what was going on.

PARIZEK: Thank you.

DUQUETTE: Any other questions from the Board or from the Staff?

(No response.)

DUQUETTE: Thank you very much, Graham. Appreciate it.

The last formal presentation this afternoon will be by Peter Swift. Dr. Swift has an interesting background, I think. He has degrees from both Yale and the University of Wyoming at the bachelor's level, and a Ph.D. from the University of Arizona in geosciences. He's responsible for development of performance assessment strategy and defining the supporting technical analysis in the Bechtel SAIC Performance Assessment project.

Previous responsibilities with the Yucca Mountain Performance Assessment include Lead Analysis for Igneous Consequence Modelling, and Lead Analysis for Identification and Screening of Features of Processes to be included in the Performance Assessment. Work prior to joining the Yucca Mountain project included nine years of experience in performance assessment for the WIPP project, where he was a lead author for the DOE's 1996 Compliance Certification application to the Environmental Protection Agency.

His presentation this afternoon will describe plan analyses of the capabilities of the barriers of the Yucca Mountain repository.

Peter?

SWIFT: I'd like to start out by crediting the co-workers with me on this presentation, starting by crediting Bob Andrews, who's been the manager of the Performance Assessment, and now combined Performance Assessment and Science Programs for this project for quite a few years. And you'll see I'm listed here as the manager of something called the Performance Assessment Strategy and Scope. It's actually a sub-project in Bob Andrews' group.

I want to thank Ron Howard, who's sitting over here, who is the Deputy Manager of this group, who actually does all the work, and I make the presentation. I want to thank Jerry McNeish, who is the Manager of the Total System Performance Assessment team that really does work, and Pat Lee. Is Pat Lee here? Pat Lee should be listed as a co-author on this, but we tend to just list the speaker. Pat generated the figures, told me what they meant, worked with me in putting this together. And also Dave Sevougian, who's back there somewhere, who is the lead for developing TSPA models, who will answer the hard questions.

What I'm here trying to present is a follow on to a talk I made to this Board in September, which was a series of what we called one arm analyses. And the reason I'm here I think is to explain there are other techniques to getting--I'll come back and work the one arm in a minute--but there are other techniques. And, in particular, there are also--there are regulatory drivers, NRC regulation, Part 63, that lead us to use a variety of techniques to demonstrate the barrier capabilities in the License Application. And what I'm going to go through here are some draft examples of how we may choose to do that in the license application.

Topics here, a few remarks on what's in the NRC regulation about barrier importance, barrier capability, a very brief summary of the analysis techniques that are available to us, and the analyses I'm going to show. I'm going to show a bunch of results here. They are representative examples based on past work. They're all examples, they're all draft, they all happen to be for nominal performance only, which is where the NRC wants us to focus our emphasis on the barrier performance, barrier capability. Everything here is a draft. If there are any quantitative results here that look promising and interesting, they will be updated for license application in two years.

And the last point here, everything I've shown is a mean result, and that's for simplicity in this illustration.

I recognize fully, we recognize that there is uncertainty in those mean results. They're averages in the Monte Carlo modelling system of many, many outcomes, hundreds of them. And we will need to give you a more full discussion of what the uncertainty in the results is and where it comes from.

Also, one more point on that uncertainty. I show results here typically in terms of those that involve radionuclide fluxes. In terms of all radionuclides, they're broken down by integral species, where is the Technetium going, where is the plutonium going, and so on.

I'm not going to read these. These are quotes out of the NRC regulation. I hope they're correct. The important thing here, important to waste isolation is a defined term, that key phrase here, reasonable expectation that disposal will not exceed the requirements of Part 63.113. That's the 15 millirem limit there.

What is a barrier? It's something that reduces the rate of two things, either movement of water or movement of radionuclides, or a third thing, released radionuclides from the waste, which is basically a sub-category of movement of radionuclides. So, barriers have a role of water flux or radioactivity flux.

I had hoped actually that the NRC would be presenting before me on this same subject, and they're not here. They would have read this instead of me. The regulatory requirement here, the applicant must describe the capability of barriers to isolate waste, taking into account uncertainty. And they must provide a technical basis for that description, and it must be based on and be consistent with the entire performance assessment. And that's what I'm going to try and show here in draft examples from past analyses.

Types of techniques we've used to get at behavior of components within our full system model. We start with system-level regression analyses. Anybody who does Monte Carlo modelling, it's a very standard technique. You perform regressing analysis to see what length of parameters contribute the most to the spread in your model results. It's great for providing insights into contributors uncertainty and total system performance, but it's pretty limited in the insights it will give you on component performance. Therefore, we've gone to other approaches.

One-on analyses, what you saw in September where we simply added--we started out with bare waste from the land surface hypothetically, and added in different barriers one at a time. And we showed you what the hypothetical total doses would be, very hypothetical. It's a good display of relative contributions of barriers. It is, however, unfortunately strongly dependent on the order in which we added those barriers. It's also not physically realistic.

Waste is not going to be emplaced without the barriers.

One-off neutralization techniques is where you remove barriers one at a time, and then rerun the analysis. They are in definitive order. Redundant capabilities are difficult to characterize. In other words, you have two barriers that are basically capable of doing the same thing. Let's say the unsaturated zone and the saturated zone are both very good at absorbing plutonium, which they are, if you take out the saturated zone, it will look like nothing happened, because all the plutonium you put into the system got stopped in the unsaturated zone anyway, and the saturated zone didn't make much of a difference there. And there again, they're not physically realistic.

Another technique, just look at the intermediate results from the full TSPA. That shows individual barrier contribution in the context of a full system. But, again, it simply doesn't get you through this redundant capabilities problem. If all radionuclides are stopped at the waste package, which a large number of them are certainly, you have very little way to assess the capability of the downstream barriers.

However, it does let us look at different types of results, water flux, radionuclide flux, for different barriers, and we don't have to use total dose as the primary metric.

I may get through this whole presentation without showing you a single dose plot, which is something that TSPA rarely does, I think. But there are no doses anywhere in this, and I think it's useful to realize that you can use the TSPA modelling tools for things other than dose.

So, what we propose to do here for the LA is look at intermediate metrics, i.e. water flux, radionuclide flux, using component analyses, full system analyses, one-off/one-on techniques, whatever techniques are appropriate to get us through, to get a good look at what each of those components can do or could do with respect to water or radionuclides.

This is a complicated picture here, but it's a nice picture. This is the thing you want to focus on here. What are the barriers? There are nine barriers that the DOE listed in the Site Recommendation, and plans to use also in License Application as the significant barriers that make the system work the way it does.

Surface soils, topography. This is the infiltration barrier. The unsaturated zone, this is above the repository. It's the percolation flux, you have seepage and drift effects, the drip shield, the waste package cladding, waste form, invert, UZ below, saturated zone. I listed them here not the way we did them in the one-on analyses. Here they're listed in the way the water moves. So, the water would contact each of these essentially in that order.

From here on, the whole talk is draft results from here on that I hope give some insight into what these barriers may be able to do for us. And this first one, let's look at the surficial soils topography. This is the infiltration barrier. What's of interest to us? What's compared? Precipitation, that's the upper curve here. That's, you know, the rain, the snow that falls on the mountain, compared to what enters the percolation flux in the unsaturated zone.

This is a very crude coarse spatial average. If you look at a detailed map, precipitation and infiltration that's developed by the USGS model that provides input to the Berkeley laboratory unsaturated model, a tremendous amount of spatial variability. We've just coarsely taken that and averaged it here. These steps are climate changes in the precipitation. They show up down there. Bottom line. As currently modelled, and all these results here are as modelled, and I'm just trying to explain what our models are doing, I'm not trying to justify them or explain why they do what they do, just what's happening here, about a factor of 16 reduction in water flux just at the soil level and the unexposed rock level, earth land surface. Those are from transpiration, run-off, evapotranspiration, the big ones. All right, so keep this one in mind.

Now, what does the unsaturated zone do? This is the question of, well, how much of that water that gets into the top of the unsaturated zone actually enters the drifts? And there are two different plots here, and there's a good story here. The upper one shows--one of those curves is the same red curve from the previous page, now shown in black. It's down here also. This is the spatially averaged infiltration into the model above the mountain. All these other curves up here, that, by the way, is right up here at the top of that one, basically underlies that green curve, if you can see it, all the rest of those show spatial variability in seepage into the drifts. And these are waste package bins. We group the waste packages into different groupings based on how much infiltration is in that portion of the model.

So, over most of the repository, the packages are seeing less water flux than the average infiltration. Some are actually seeing more, full purposing effects. The spatial average down here over all packages and over the whole repository, it's about, out here 10,000 years, about a factor of 4 reduction. How much of that water is going down through the UZ in our model is diverted around the drifts and how much gets into them and lands on the drip shield? About a factor of 4.

The drip shield. This is clearly a barrier designed to reduce the movement of water, in the NRC's language. So, what is it doing? Well, the upper curve is the same curve you saw on the previous page. If you'll back up one, that's this one here. They look very different because you've gone from a log scale to a linear scale. And this is seepage flux into the drift, and this actually came up in Mark Peters' talk. What do we actually think modelled seepage flux is, and he gave an estimate on the order of millimeters. Well, that's in fact what this model is showing, spatially averaged.

The bottom line here is actually not modelled. That's simply drawn, because in our current model, the waste packages are not failing within 20,000 years, water flow through them is zero. So, the drip shield is completely effective as modelled at preventing water from getting through.

The waste package, what kind of barrier is it to water flow? And in order to get at this, we have to take the drip shield out. It doesn't make any sense to compare zero to zero. So, we take the drip shield out and rerun it. The upper curve here is that same seepage flux, and this is--the notes down here, there should be enough of a note there to tell you where I got the numbers from. This one is created by scaling that one to the area of the waste package failure, the breach, in the small fraction of packages that the model is showing are failing in the first 20,000 years.

So, this is for the packages that have early failures, it's less than 1 millimeter per year getting into those packages. For all the rest of the packages, which is the 11,999 of them in the repository, it's zero. The waste package is completely effective at preventing water from getting in in the first 20,000 years.

All right, now moving to barriers whose function isn't to limit water movement, but to reduce the rate of radionuclide movement, in the NRC definition. And that curve should be a smooth line. I'm not sure why it came out with little steps in it there. I think it's smooth in the handouts. The upper curve here is simply the total radioactivity in the inventory of the repository through time. That's just the radioactivity to K curve, the K in-growth. There's no transport, no nothing in that. Imagine all the stuff is just sitting there in waste packages going nowhere, or sitting there on a pad, wherever you want to put them, that's what it would look like if you left it the reactor sites, too, through time.

The bottom curve here is the calculated release rate. The red axis here is a little different from the black one. I'm comparing apples and oranges here. I'll explain that. This is total activity in curies. This is curies per year leaving the waste packages that have early failures in them, and this is leaving--I've removed the drip shield here, so this is leaving by both advective transport and diffusion. However, there is one waste package out of nearly 12,000 in each realization. That's a large factor in that reduction.

All right, what's my number there, about a one in 10 billion, it's less than one in 10 billion total inventory. Granted, this is a comparison of total inventory to what's being removed through time, and you could argue that, well, this accumulates through time, comparing the rate to a single barrier. But I'll come back to that. I'm going to show the same thought in a different way in just a minute.

The cladding, what can the cladding do? Here again, I'm comparing an activity flux, and in this case, I've chosen to compare the activity leaving waste packages with and without cladding present. Now, all the other barriers here are working as expected. This is basically the lower curve here is as we expect the system to work. But the upper curve just removes the cladding, and if we take the cladding out, and the cladding only affects the commercial spent nuclear fuel, the cladding alone then is creating about a factor of 40 reduction in the actual flux.

This one again, this looks at the activity flux out of the waste form. This actually comes from the same one-on analyses I showed back in September. This is the hypothetical case of the bare waste form with the cladding removed, the drip shield, the waste package, no engineered barriers, the waste form exposed directly to precipitation flux. So, it's the annual precipitation flux, that main curve I showed back on the first one of these results that is controlling dissolution here, solubility limits determined by that body of water.

And, again, the upper curve is simply the inventory of the whole system through time, same curve I showed before. This is what could get out of the waste form itself if it were fully exposed to water. You see a big spike right away. This is highly soluble stuff, it comes out very quickly, technetium, and short-lived stuff, cesium and strontium. The stuff that is solubility limited in that precipitation flux comes out more gradually through time. But the waste form itself is a very effective way of reduced total activity getting out of the system, except for that initial spike.

Because this initial spike is dominated, it's primarily cesium and strontium total activity there, that stuff is short lived and would not be around later on here at 10,000 years. Therefore, I think it is appropriate to think in terms of comparing total activity to the release rate at 10,000 years. The first time step release here is, if we simply took that and integrated it out over time, you'd get a lot larger release. But it doesn't exist over time. It decays.

The invert. Again, here we've removed some of the other barriers in order to get a better look at the invert, removed the waste package and the drip shield, and then just looked at activity flux into and out of the invert. So, the upper curve here is what gets out of the waste package and enters the invert. The lower curve is what comes out of the invert. And, again, the early spike here is the cesium and strontium.

I can field questions on these later. We'll keep going here. Next one?

We've gotten through the invert, and now we're looking at the unsaturated zone below the repository. This is the transport barrier. It's not a water flux barrier. What we're interested in now is how much radioactivity that enters the unsaturated zone can get out. And there are two ways to look at this. We've got examples with the next two slides, one is to look at the break-through curves, which these are actual break-through curves that we calculated and used in TSPA-SR a couple years ago.

The break-through curve is a form of a complete neutralization. You're assuming that you've got a unit of stuff released directly in the top of the UZ at time zero. It's assuming it was bare waste form sitting there in the UZ. Well, what mass fraction of that gets out through time? So, what we see in the UZ is that, for example, the green curve here, these are the irreversible plutonium colloids, the ones that move quite quickly, and about half of them are getting out around, what, 300 years, I think. That's fairly fast movement for those colloids.

The dissolved plutonium, or the reversible colloids that attach and detach and then tend to get sorbed while they're detached from the colloids, they don't come out, you know, maybe 30 per cent of them don't come out after 100,000 years. So, what this sort of plot tells you is that the UZ is a very effective barrier for the strongly sorbing actinides. And, you know, it's not a very effective barrier for the fast moving colloids, and a moderately good barrier for intermediate species like neptunium.

This is the actual model behavior through time. This is not an instantaneous time zero release into the system. This is the release through time. In order to get a good look at it, we took a lot of the other barriers out, took out the drip shield, the waste package, seepage effects, the invert, this came out of those one-on reports, one-on analyses I showed back in September. The reason I'm showing this in part is it gives a very good comparison against the saturated zone here in just a minute.

You see a tremendous reduction in the linear time scale, that's 1000 years there. The first, you know, 500, 600 years, there's a tremendous reduction here, while we still have large quantities of cesium, strontium and Americium 241 in the system, that are both sorbed in the UZ very effectively, and they decay away. So, that's the difference between there and there. At later times out here, we're looking at the effectiveness in stopping neptunium, technetium, which are the main contributors to the total activity flux.

Saturated zone. Here again are some calculated mean saturated zone break-through curves. These are means from 100 realization. You'd have to see the plot of all the four realizations to understand what I mean by that. But each of these is a--well, the first curve here are the non-reactive species, technetium, iodine, and for the purposes of this model, we assume carbon is non-reactive, but it's not a major player in the system, and that was a simplified assumption rather than trying to build a reactive transport model for carbon.

The green here is--I'm having trouble reading that. I think I see why. Yellow is neptunium, plutonium, the brown, is the irreversible colloids, cesium and strontium are in there. The median break-through times, how long it takes for half the mass you put into the saturated zone are shown down here, and what we see here is that the saturated zone is not an effective barrier as modelled to technetium. At 18 kilometers here, we're still getting half of it through in about 300 years. It's not as effective as we would have thought for neptunium, and it is quite effective for the other species.

And this is the activity flux into and out of the saturated zone, out of the unsaturated zone, upstream of it. So, we're avoiding that problem where you can't see the effect because things were already sorbed in the unsaturated zone. This should be directly comparable to the plot I showed on Number 17. What we see is a big benefit at early times where we're picking up the cesium, strontium, Americium 241. We see an odd effect here where it looks like we're actually briefly getting more out of the saturated zone than we put in. That's probably a real--it's potentially a real effect from the sorption of Americium 241, which then decays, neptunium 237, and then remobilizes, so it could actually be possible to have a system that effectively trapped Americium 241 back here, and as it decayed, let it get back out again out there.

This system here is of benefit about 1000 years in here by a factor of 7, much less out here at later times. Basically, the difference here is the effectiveness of the UZ at stopping technetium and neptunium, which are the largest contributors to the total activity flux, and it is not that effective at stopping those two. It's a very effective barrier on cesium, strontium, Americium 241, plutonium, but the largest contributors, the total activity at later times are neptunium and technetium.

I've got a three-page table that simply summarizes what was in the last bullets in each of those slides that I went through, and I'm not going to bother to go through it again.

The conclusions. The first point is that the draft work to date, that's what you just saw, confirms that each of the nine barriers does have a capability to reduce movement of water or radionuclides. There's no question about that. The capabilities can be quantified using component models, TSPA results. What I showed here was simply means and total activity fluxes. We can break that down into full uncertainty analyses. We can break it down by species. We can show you, for example, where the Americium went, where the cesium went.

And the last point, doing this kind of analysis invariably teaches us something more about how our model is working. So, aside from the fact that it's a regulatory requirement, it's a good thing for us to do.

And I'll take questions.

DUQUETTE: Thank you very much, Peter.

Board questions? Mark?

ABKOWITZ: Abkowitz, Board.

Maybe I'm over simplifying all this, but it seems to me that the $64 question has been of these nine different barriers, what are their individual and collective components in terms of, you know, the adequate protection that they provide to some type of hazardous release? I'd seen the one-offs and the one-ons and some of these other things, and my sense is that we're kind of dancing around the perimeter of the issue.

My question to you is why can't we create an analysis where the dependent variable is some measure of risk much like TSPA is looking at it, and the independent variables are the nine different barriers? Why can't we perform a regression analysis at that scale? Why can't we do a correlation analysis between them so we can see what type of redundancy they contribute to one another? And, therefore, I think we'd have a much clearer understanding of all this. Am I over simplifying it?

SWIFT: Well, the issue there is that we've done this with a full system model, and done a regression analysis. But what you see is that uncertainty in total performance is driven by the barrier of next largest contribution, and you simply don't get past that to look at the rest of them by looking at the full system. You really do have to take them apart piece by piece and ask the question. Essentially, the question is if the other barriers weren't there, what would this one by itself do? The other barriers will be there.

There's no question but that all nine barriers will be there and will perform some function. So, it's a difficult modelling problem.

ABKOWITZ: Maybe we're talking at two different levels here, because to my way of thinking, any time you're looking at a dependent variable that's a function of independent variables, and maybe it's a different modelling formulation, but it strikes me that each of these barriers is an independent variable. I mean, that's the way the problem is being defined. Are we or are we not going to have certain barriers included in the process? Obviously, the natural barriers are there. I'm having difficulty understanding why one cannot run an analysis where those are the independent variables, and you have the ability in your model to remove what each of those represent and be able to, you know, re-compute the final results, and then you can use that information to perform another regression at a higher level and get at the issue, as far as I can tell.

SWIFT: You're suggesting basically to rank the barriers by their contribution to total dose, and then remove them in that order one at a time and rerun the regression analysis?

ABKOWITZ: Right, and get to the point where you can run a correlation matrix between the different barriers so you can see how highly correlated some of them are, or even if some of them are negatively correlated. And that really provides a better understanding of how they play off against one another. And in some cases if the correlation is high, then you can accomplish two things. One is you can recognize that there is that interrelationship, but at the same time, it provides you with certain quantification for the defense-in-depth that you have with your system.

SWIFT: The answer to that one is that we've actually started down this path, and the effectiveness of the waste package to begin with, we design a robust waste package, and that overwhelms the importance of the rest of the system if you go straight to contributions to total system performance. I mean, one way around that would be to design a best robust waste package. That seems like not the purpose of the repository. So, I'm not sure I'm answering your question. I'm sorry.

ABKOWITZ: That's okay.

DUQUETTE: Dan Bullen?

BULLEN: Bullen, Board.

Could we go to Figure 12 first? First off, let me acknowledge that I understand why you're doing the type of analyses you do. You want to look at the barrier importance because it's a requirement of the regulations. Unfortunately, maybe you've even skewed the data a little bit more here because you've got a one package failure out of then to the fourth, so, you know, when you show a ten to the tenth reduction, it's really only like a ten to the sixth reduction, because of the fact that you're only failing one package.

What I'm trying to make a comparison to is the one-on analysis that you gave us last time. And the one-on analysis started with sort of like a ten to the seventeenth dose, and I know you're not going to compare dose, but it sort of broke down that there was, you know, significant contribution of the natural system, which is kind of lost here when you have those curves laying on top of each other. It looks like in this case, you have, you know, almost everything relying on the waste package.

And I understand why you have to do the analysis, but it's a little bit more confusing when you do it in this way, because as you do the one-on analysis, you can really see sort of the contribution of each barrier. You know, if there's a ten to the seventh reduction because of the natural system, and a ten to the seventh or eighth reduction because of the engineered system, it looks like there's an even contribution. So, I just wanted to point out that, you know, this is even maybe a little more artificially emphasizing the performance of the waste package. But that's just a statement. You don't have to comment on that one, unless you want to.

SWIFT: I do want to comment on it. First, part of our purpose here was to stay as close to that business about showing the capabilities consistent with the total performance assessment models, and our current performance assessment models, consistent with the information we're getting from waste package design and performance, suggests one package failure out of--in the system, in each realization, one early package, due to a bad weld basically, so that's where the one comes from. I mean, you're right, you could just multiply that by the number of packages and move that curve up.

Can I go to the Figure 17? The question of why did the one-on analyses in September show such a big performance from the natural system, and this one apparently does not? It's actually, you know, I want to be careful with how plots are presented, a lot of that here is the conversion from a log time scale to a linear time scale.

The big benefit that we saw occurred in the first 1000 years in those plots in September, except the first 1000 years was out to here in September, and now it's just out to there on your linear time scale. It's still there. And the peak coming out of the UZ in September was out here at 38,000 years, which is not shown here. So, it's actually the same information with the scale adjusted.

BULLEN: Can we go back to Figure 13, please? Now that I've complimented you and said maybe you should, you know, show us more credit for the natural system, now I have to come back to cladding credit again and ask the relative question. You have a 40x reduction. That's great. I guess the question that I have, and maybe you've already worked this out with the NRC, is the data that you used to justify the 40x reduction, particularly in light of the fact that a majority of the cladding that you're trying to take credit for hasn't even been fabricated yet, and so when you look at sort of the failure rate of original cladding, and you look at the fact that they're going to go to higher burnup, they're going to get thicker oxide films, they're going to have hydrides that may reorient, particularly with the higher temperatures that may be in dry storage, it's really hard for me to see that you take credit for cladding in the engineered system. We're not doing a great deal of analyses of the spent fuel that's out there to tell me what's the condition of it. And, so, I know what the as fabricated condition is. I know how many leakers you have coming out of the core. But to take credit for a 40x reduction for cladding is just stretching it for me. I guess I'd like to hear your comments on that.

SWIFT: Sure. My comment on that is I view my purpose here to try to portray as fairly as I can what our models are doing, and it's not my role here to defend this model. I just explained to you what it's doing, and I appreciate your comment, and I hope our cladding people have heard that.

BULLEN: Bullen, Board. Just one last issue that was raised by a member of the public, asking what happens during the 300 years, what's the exposure that you expect in the releases to the atmosphere and worker exposure, but I predicate that on saying that these are all just examples of a failed waste package, or do you expect to have worker consequences identified in the types of doses that you calculate?

SWIFT: I see Tom Doering is here who can respond to the cladding comment. But I will field that one first.

No, these are all aimed at the post-closure performance, and there are no doses shown here. But, the dose calculations, dose results that we show were from total system performance assessment all begin after closure. The calculation of the worker dose and the dose to the general public during the operational period is done separately, and that's part of basically the preclosure safety assessment.

BULLEN: Thanks, Peter. I'll acknowledge Tom, but I'll ask him do you want me to give you an answer first, or should I just let you give it? Go ahead, Tom, you can tell us.

DOERING: Tom Doering, the person responsible for the cladding people.

What we've been doing to gather that information is actually we have been working with the industry, with EPRI and DOE and the NRC, because all those organizations are very interested in long-term performance of cladding these days. In fact, there was some work done just wrapped up cooperatively between DOE and NRC, the Ratan facility, pulling out rods out of the old TN facility there and just looking at them all and doing some dissecting. And, so, we have data that goes back to looking at that. That was over ten years of dry storage data. And we also have worked with the industry to get that cladding model input from their data that they've seen.

Clearly, this is a Zirc 2, Zirc 4 cladding. If they go to niobium and things of that nature, we're working with EPRI and also with the NRC to be comparable with our models in that sense. So, if some vendor comes up with a difference between Zirc and niobium, then we'll have to demonstrate that it has a similar performance, and we'll take that into consideration. So, we are sensitive to that. And being the first to use the design cladding, it's good material.

BULLEN: Bullen, Board. I agree wholeheartedly. And the test data is actually very good. I guess I would just strongly encourage you that if you're going to take this credit, you have to continue to work throughout all of the repository development, because as I mentioned, a lot of that fuel hasn't even been built yet.

DOERING: Correct. And we realize the different corrosion rates. Again, for niobium, we anticipate a different oxidation rate, a different build-up rate also on that. So, that should be taken into consideration.

BULLEN: Thanks, Tom.

DUQUETTE: Priscilla?

NELSON: Nelson, Board. I knew we weren't through with cladding. I knew it. As I understand this, this is a reworking of analyses that have already been done.

SWIFT: Yes, every one of these has already been done.

NELSON: So, I'd like to verify that first, that it's recast in a different form by focusing on release rate.

SWIFT: Or water flux.

NELSON: Or water flux. But not a new interpretation.

What I'd like to ask you, though, is you're doing this exercise to find the way that you're going to present this issue in LA?

SWIFT: Yes, to meet a very specific regulatory requirement back on Page 3 and 4.

NELSON: Right. So, how happy are you with this? I mean, is this the way you think the project is going to go right now? Are there other attempts afoot to--

SWIFT: There will be some version of this, because the regulation asks for capability in terms of water movement or radionuclide movement, rather than in terms of total dose.

So, we would probably not want to go the more intuitively attractive to many people approach of those one-on analyses where you see the impact on total dose. The rule does define capability as something other than total dose.

NELSON: Nelson, Board.

This is the strategy that you think will be the backbone of making the case, using release rates or water flux, this is satisfying? I'm just trying to see--

SWIFT: Yes. My hope is that this will satisfy the NRC requirements in Part 63, 115.

NELSON: Thanks.

DUQUETTE: David?

DIODATO: Diodato, Staff.

Peter, turn to your Slide 18. I had a question. This is the saturated zone break-through curve slide. First, I'd like to say that I like this kind of figure because for one thing, it's comprehensible and you can understand its meaning quite easily.

So, on the bottom, the footnote there says saturated zone break-through curves for Carbon 14, Iodine-129, Technetium 99 are identical. So, to me that means that you probably treated them as conservative species in this case?

SWIFT: Yes. It's this one here?

DIODATO: Right.

SWIFT: Yes. They were just overlaying each other. They were all treated, I believe, as non-reactive species for this.

DIODATO: Okay. So, they faithfully represent the advective loss via the groundwater in the system. They travel with the water system faithfully.

SWIFT: That would be my interpretation. If we had one of the hydrologists from the saturated zone here, I'd like him to field that question. But, yes, that would be my interpretation.

DIODATO: Okay. So, then I look at the 10 per cent mass fraction break-through at 20 years, so that means that water must have been moving on average roughly a kilometer a year. And then I wonder if there's any field evidence whatsoever that would support that kind of a realization, if you've ever seen any--

SWIFT: I have not.

DIODATO: --isotopes in the saturated zone?

SWIFT: I have not. Do we have--Ernie, would you like to field that one? I'm not a saturated zone hydrologist, but I appreciate the point you're making.

DIODATO: Or even the 300 year, for example, median break-through time.

HARDIN: This is Ernie Hardin with BSC. To answer that question is a long answer. We have run many cases where we've studied groundwater travel time in the saturated zone, and in the unsaturated zone. We have published reports on those cases. We've tried to capture our uncertainty and the understanding of the medium and the flow paths and the chemical transport processes. These curves include--these are mean curves.

SWIFT: Yeah, these are mean break-throughs from--each one of those is underlaid by a family of 100 break-through curves that typically have steeper slopes and a broader range of times and the means are exposed through the middle of that family.

HARDIN: Right. And, so, the representation of unretarded transport there is based on the site scale saturated zone model, which includes matrix diffusion, and it includes several different media types and a hydraulic structure which we think adequately represents the site.

DIODATO: I mean, no matter how you got there, there ought to be some naturally occurring or, you know, bomb produced radioisotopes that would go with the system that would give you some clue, and if you observe those in that time frame or not. That's the question. So, I guess the answer is no, you can't think of any that you've seen.

HARDIN: Well, if I could elaborate a little bit, I think, you know, you're probably aware of the natural isotope evidence that we have for groundwater travel time in the saturated zone. But, there are complicating issues to interpreting that, you know, doing a blanket interpretation that the groundwater has to be 10,000 years old.

DIODATO: Or 20.

HARDIN: There are issues of mixing source, and so on like that. I mean, there are promising lines of inquiry. We have looked down most of--many of those. But there are also uncertainties as to the hydraulic structure, and we know what the head gradients are there, and these curves reflect what happens to travel times when you incorporate those uncertainties into the model.

DIODATO: Thanks. So, that's the one question. The other one, maybe this is all pre-existing studies, and so maybe the other question--I mean, Jerry McNeish said that there's about 80 or so AMRs that are coming on line. They're supposed to be done around May to feed into the LA predictions performance. And earlier, there was a presentation that Jeff Williams gave that showed the new repository layout, and what I'm wondering is there's some parts of this new repository layout that didn't exist at the time of the TSPA-SR. So, the question in my mind is how many process models have been rerun to reflect this new layout that would be sensitive in terms of the thermal hydrology and radionuclide transport in the unsaturated zone flow and, you know, loading and all these different things that might be relevant to prediction of radionuclide transport, and how many of those do you expect?

SWIFT: The relevant ones are being rerun right now, the unsaturated zone, that's the thermal hydrology model. The release points in the saturated zone will be updated. The question has come up as to what is the cutoff time for the new information to get into the TSPA-LA model. Depending on what it is, we're sliding through it right now. It's over the course of the spring. So, we are building the TSPA-LA model as information becomes available from these other AMRs. We expect to have that TSPA-LA model built over the course of the summer and have it running by fall.

DIODATO: The data density question, we asked that before. Is there going to be increased uncertainty with these predictions because of the sparsity of data toward the north more relative to the data collected from the northern part of the ESF and south?

SWIFT: With respect to the site recommendation vintage analyses and the old footprint? I don't know the answer to that one. That would be up to, primarily up to the UZ flow and transport modelers, I would say. I don't know the answer to that one.

DIODATO: Thank you.

DUQUETTE: Thank you, Peter. Any other questions of the Board or of the Staff?

(No response.)

DUQUETTE: I'd like to close the formal part of this afternoon's meeting then. I thank all of the speakers, both for the quality of their presentations, as well as their keeping us right on time. And I'm going to turn the chair over to Dr. Corradini for the public part of the Board's meeting this afternoon.

CORRADINI: Thank you, David.

We have seven individuals that want to make public--I should say oral comments. First, Mr. David Swanson.

SWANSON: I have no comments to make.

CORRADINI: You have none? Okay. And given that we're at the end of the time and we have about 20 minutes, I'd like to hold them to less than five minutes apiece. Mr. Jerry Szymanski?

SZYMANSKI: My name is Jerry Szymanski, and I'm the president of--I engage in oil exploration. I have also been a persistent critic for the last 20 years of the DOE program.

I've also written a book which will be published quite soon on Yucca Mountain. At my initiative, I have encouraged my--the Russian scientist--to attend this meeting. I do have two basic points to make. The first one is we view the Board as an ultimate body scientifically, which advises the Secretary, the President and the Congress.

The second point which is of particular interest to us is a letter which was provided to DOE with reference to the issues we are concerned with, or I personally have been concerned for the last 20 years. Now, that letter had the effect of basically closing the issue of so-called optimum--

CORRADINI: I'm sorry, could you repeat what you said? Close the issue, the so-called issue of?

SZYMANSKI: Optimum coring. And the third point is it reviews this letter as being extremely instrumental to the Secretary's decision to recommend Yucca Mountain, the President's decision to approve the recommendation, and ultimately the decision to over-ride the State of Nevada's letter, which that letter was very, very ill-advised. therefore, prior to publishing our book, which we will be doing prior to licensing, we would like to offer to the Board a preliminary look at the material which we have gathered together, with the hope that the Board will revisit the issue and advise the highest level of the government accordingly. I would like to pass two disks for a review of the book, if I may.

Thank you for your attention.

CORRADINI: Thank you. I would suggest you give it to the Staff. Okay, thank you.

Ms. Sally Devlin, Mrs., excuse me.

DEVLIN: Again, welcome to Nevada and thank you for this lovely meeting. It's always fun to talk at the end, because I always tell you jokes. And I'm really disappointed in Peter's presentation, because he didn't talk about Abe and me playing gin rummy for 300 years on top of both repositories. You know, you only start once it's closed. That's not fair.

The second thing is I think the USGS should do as we saw in Pahrump, take satellite pictures where they found the five mile lake, the length of Pahrump, but on the other side of the mountain in California two and a half miles down. And all I hear again is modelling of water, water, water, water. So, take satellite pictures, the USGS does a wonderful job, and many on the Board have my report, and I hope they'll share it with all the rest of the members, and especially the new ones, bring everybody up to date. Good orientation, you know, that's my middle name.

The other thing is about the--that I'm really going to bring up and I have to talk to you about, and that is in 1995, we formed the NRAMP group at the university, the Nuclear Risk Assessment Management Program, and one of our speakers at that group, we did radiation and water at the test site, and I include Yucca Mountain, which was not included then. Now, we're talking 1370 square miles. Yucca Mountain is 25 square miles with some of the Tonopah test range. And this was on the clean-up, and the first thing with Al Alms and Leo Devers, 95 per cent of the test site could not be cleaned up. So, we're talking about 1320 square miles exception, the 25 square miles for Yucca Mountain.

Now, my feeling is, and this is the wonderful thing about this Board, and especially for you new people, as I understand it, you are finally recognizing that Yucca Mountain is on the test site, and all the dirty water and the colloids and the bugs, and God knows what all, will flow into Yucca Mountain, including the EPA stuff that I told you about with all the cows and all that. So, I brought with me a 1996, March of 1996, FFACO report. And does anybody know what this report is? Has anybody ever seen it? Okay, this is the State of Nevada, Department of Conservation and Natural Resources Division of Environmental Protection, and the United States Department of Energy in the matter of federal facilities compliance act consent order.

And what this was, and it has been updated, it's current at the University of Nevada, and it should be everywhere because it talks all about the compliance with the test site, and my feeling is that's what you guys are going to get at Yucca Mountain. And it should be reviewed, and there should be some kind of an agreement for the 25 square miles of Yucca Mountain with this sort of thing with the State of Nevada, and I don't know if there ever has been. So, that is the FFACO agreement that is updated, FFACO.

And last, but not least, since you're going to ignore Abe and me sitting there playing gin rummy on the two Yucca Mountains, because there is no stewardship funding, and it really bothers me because I certainly don't want to get irradiated doing our job, so I do hope that we, the public, will be considered, because the first 300 years are the most important. And at the DOE/NRC meeting in November, we were promised that there were 295 things that had to be done, and 50-something had been done. And you promised to get reports to us. We have never heard or seen or smelled anybody or anything, and we would certainly like to. This is what the public wants to know, is the information flow.

And, with that, I leave you, and thank you for coming.

CORRADINI: Thank you. Our fourth public comment is by Ms. Judy Treichel.

TREICHEL: Judy Treichel, Nevada Nuclear Waste Task Force. I just have sort of a laundry list here, because we have new people here that are not regulars over decades like the rest of us have, and I just wanted to mention a few of the things that were said today.

I really liked Priscilla Nelson's question about science and technology, because I think it's very appropriate. This thing is being treated as though there's some sort of a crisis, and there is the need for a repository. There's no crisis of need and there's no crisis of time, and it's almost as though it's a race, and you've got the science trying to keep up with the technological fixes.

And Margaret Chu mentioned that we will increase public confidence with short-term wins. Well, that kind of goes along with that race scenario as well. And I probably can tell you with a lot of confidence that you won't increase public confidence with short-term wins, because we've been here for the long-term, and we're not seeing that.

There was also another good comment by Mark Abkowitz about relooking at worst case scenarios now that we've endured 9/11, and we sort of did that together. That happened during a Technical Review Board meeting. And during one of the, I think it was Joe Farmer that was talking in his presentation about the fact that you need to look at the plausible instead of the possible. Well, we all learned a lesson on 9/11. There weren't any of the people that got onto any of those airplanes that thought they were either going to go into a building or have to take the plane down.

But now that terrorism is here, one of the things that it's built on, and I'm not a big person that goes around being afraid of terrorism at all times, but I do think in something that's got the potential, like nuclear waste travelling around the country, or in fact being consolidated in one place, that you do have a potential if in fact there are terrorists out there, but I do think you have to give some more thought to the possible.

We before were not even supposed to be using the term worst case scenario. We were always supposed to use the reasonable case. And we found out that reasonable stuff doesn't always hold the day when we had the terrorism thing.

But in seeing some of the presentations from the Department of Energy, this is almost like the President trying to make the case for the war. You know, tonight, now tonight is the night he's really going to make a case for the war. And we keep going through these things, so somebody keeps trying to make the case for Yucca Mountain. And there was a question from one of the Board members about as far as metal failure was concerned, that it wasn't clear that that would happen in the repository. Well, that's not the question. The question is is it clear that it won't happen.

And the thing that keeps getting lost is that it's not the job of the Department or the Board or any other, the NRC, any of the other agencies, to assume that Yucca Mountain is good, and to leave it up to Nevada, or any other group to prove that it's not. Real science, real investigation assumes that there's something wrong, and you have to prove to yourself that there's not. So, it's always been sort of a backwards case. And everyone, especially the Board, who we have depended upon for so long, has to be able to accept the fact that maybe there is an insurmountable problem, or a problem that it doesn't make sense to try and put a fix on. And there may be one of those problems, and you have to have the courage to say so.

And that would be the end of my comments. Thank you.

CORRADINI: Thank you. Our next comment is by a Ms. Kalynda Tilges.

TILGES: Kalynda Tilges, Shundahai Network.

A lot of people in this room, members of the Board, already know me and my feelings on this, and I'm not going to--I've got a few comments and a few questions, and just to let you know, I did pare it down quite a bit.

I'm very glad to see you all here. It's good to see familiar faces, and I'm hoping in the future I'll consider it good to see the new faces as well. I'm glad Priscilla is still with us, but I sure would have liked to have seen a little more gender equity.

And I've said this before, and I realize site recommendation has already happened, but I cannot get past the point that every time I come to these meetings, there seems to be talk and talk and talk and more talk about water. And I think this is kind of interesting for a site that's supposed to have been picked for its incredibly dry location. Just a little irony.

Some questions I had on, and this is kind of a problem, Mr. Chairman, about waiting until the end to speak, because a lot of times if you have questions to ask about presentations, at the very end, a lot of the people that have done the presentations are gone and there's no one to answer the questions. But, I'm going to ask them anyway.

In Jeff Williams, I believe it was, presentation, he was talking about waste acceptance planning is very difficult. And I was wondering if it's so difficult, why doesn't the DOE just change the rules like it did with the Nuclear Waste Policy Act on siting guidelines? It's only a contract. I mean, those were laws, and they got changed. Contracts should be easy enough.

Also, since the Nuclear Regulatory Commission has abdicated responsibility on including terrorism scenarios for cask testing, I believe the DOE has within its capabilities and its responsibilities to require it. Certainly, for an agency who is promising safety, that would be the least they could do.

Also, Mark Peters mentioned about sampling resident wells in Amargosa Valley. I'm curious to know whose and where. There is a farmer out there by the name of Ed Goodhart who runs the largest dairy in the State of Nevada, a lot of organic dairy products and meat that feeds over 30 million people on the West Coast. His closest field is five miles away from Yucca Mountain, and his water, and his family lives right there, all the workers of the farm live right there, so I can consider it a residential area. And the last time I talked to Ed, he still had absolutely no contact whatsoever with the Department of Energy from the very beginning of the project. Certainly he's seen them driving up and down his roads taking pictures, but they've never stopped to talk to him. So, if you're sampling, I'd like to know if you're sampling those wells, or exactly where. As far as I know, you're not sampling his.

PETERS: Can I find out?

TILGES: Sure, I would love answers to these questions. I don't always expect them, but I'd love to get them.

PETERS: Mark Peters, BSC. I can find out very easily by talking to the PI. I will get you an answer. Gary Patterson is the one who's doing that work, and I can get you the names and addresses of the folks that he's referring to.

TILGES: Okay.

PETERS: I don't have a specific answer, but I can get that for you.

TILGES: Great. I'll get you my contact information. Thank you.

Okay, the last thing I'd have to say, and forgive the flippancy of my remarks, but I tend to deal with stressful situations by humor. This whole Yucca Mountain issue is extremely personal to me, not only in my job, but I am a very long-time, I've lived in Las Vegas for over 20 years, and up in Northern Nevada before that. I was in California before this, which is probably my favorite place to live, but Nevada is the place that has blessed me the most. Just a quick bit of information you probably don't need. I was diagnosed as absolutely sterile, never able to have children. I came to Nevada, and I have three, and a grandchild now. So much for fertility tests. Hopefully, they weren't using modelling.

So, this is very personal to me. I care very much about this state. This is my home. This is my children's home. This is my grandchildren's home. And as executive director of Shundahai and a long-time anti-nuclear activist, I consider this issue--I consider myself an advocate and that it's important to me to help protect and stand up for the citizens of Nevada. And, so, my remark to you is if you really expect that, then I believe that you should develop an office of wishing upon a star, because I believe that's the only way you're going to be able to get this dream passed through. Thank you very much.

CORRADINI: Thank you. Our next public comment is by Mr. John Gervis. Did I mispronounce it?

GERVIS: Gervis.

CORRADINI: Gervis. Excuse me.

GERVIS: I'm John Gervis with Latei Engineering Consultants, and I work with Clark County and Inyo County and White Pine County. I don't have any fertility stories to tell. I'm sorry.

But I did see the Board's note and the letter in September about believing that the DOE's commitment to jump-starting transportation planning and activities is imperative. And I saw the Department's response to that. And I think the concern that I wanted to express is that given the potential short-fall in funding in fiscal year 2003, and the considerable pressures that the Congress has placed on the Department of Energy for completing the license application by the end of 2004, there is the potential here for a squeeze on transportation. And I hope that the Board will continue to emphasize the importance of continuing with the transportation planning part of the program, despite the constraints that DOE will be seeing probably in this budget that's about to come out this week.

So, I guess that's basically all I have to say.

CORRADINI: Thank you very much. And, finally, our last comment is by Mr. Joe Payer.

PAYER: Thank you, Mr. Chairman. I'm Joe Payer. I'm Professor of Materials Science and Engineering at Case Western Reserve. For somebody that's followed the waste package performance and that over the last several years pretty closely, I'd just like to sort of end today maybe with a little of my personal perspective on it. And it goes back to a couple of pretty simplistic relationships.

The corrosion performance, which in this case is a perforation of the package, is going to be determined by the material resistance of the alloy selected, and the corrosivity of the environment. And the alloy has been selected now, and I think there's general agreement that the nickel, chrome, molybdenum alloys, alloys of that ilk, represent the most corrosion resistant materials that we have for oxidizing, acidic oxidizing environments that represent Yucca Mountain.

The big question mark comes into them that will determine perforations and when they'll occur is this description of the environment. And we heard a couple at least very eloquent presentations today that addressed that issue. There's been a lot of work done on that issue. But I come back to something that Priscilla Nelson mentioned early on here, and that is tell the story, the full story.

And I think what's really necessary in this is it's been shown that the pore waters and the fracture waters out there in the unperturbed state are benign. I mean, they're just not a problem from corrosivity. It's also been shown very clearly, though, that as those concentrates, or as those transform, in the terminology that Roger had us using, that they can become, under special circumstances, they can become quite corrosive. Ron Latanision mentioned that any material is susceptible to corrosion in some place. Okay?

So, I think that the next step is with all the data that we currently have, and the additional data that we're going to be getting, is what's the likelihood of those special corrosive environments to occur, and also considering the temporal range. Over what time period would they occur? How would they persist? And if it can be shown that there's a high likelihood that they would form, persist, or reform for long periods of time, then I think they will penetrate Alloy 22.

If it's shown that that's really an unlikely scenario, that the environment is modulated as the corrosion proceeds, or as the deposits proceed, then it's not so likely. So, it gets back to this key issue again of where are the realistic boundaries. And I think there's been some very good work done for that. I think it's time perhaps for more of a chemical engineering approach of how long would they occur, under what conditions would they occur.

Thank you.

CORRADINI: Thank you very much. And I think that ends the public comments. I wanted to thank the staff for preparing for this, and the Department of Energy, the State of Nevada presenters, all those in the public part of the meeting. And, we're adjourned.

(Whereupon, the meeting was adjourned.)