UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER BOARD MEETING

January 16, 2008

Marriott Suites Convention Center
325 Convention Center Drive
Las Vegas, Nevada 89109

NWTRB BOARD MEMBERS PRESENT

Dr. Mark Abkowitz
Dr. William Howard Arnold
Thure Cerling
Dr. David Duquette
Dr. B. John Garrick, Chair, NWTRB
Dr. George Hornberger
Dr. Andrew Kadak
Dr. Ronald Latanision
Dr. Ali Mosleh
Dr. William Murphy
Dr. Henry Petroski

SENIOR PROFESSIONAL STAFF

Dr. David Diodato
Dr. Carl Di Bella
Dr. Bruce Kirstein
Dr. Daniel Metlay
Dr. John Pye
Dr. Gene Rowe

NWTRB STAFF

Dr. William Barnard, Executive Director
Karyn Severson, Director External Affairs
Joyce Dory, Director of Administration
Linda Coultry, Program Support Specialist
Davonya Barnes, Staff Assistant

I N D E X

P R O C E E D I N G S

8:00 a.m.

GARRICK: Good morning. Welcome to the first Nuclear Waste Technical Review Board meeting of 2008.

My name is John Garrick, and I am the Chairman of the Board. When I am not working for the Board, I spend my time in my consulting practice in the risk field, and nuclear science and engineering applications. I have a Board assignment in addition to Chairing, and that is to be the technical lead on radiation dose calculations.

I want to introduce the rest of the Board. I'll ask them to raise their hands as their name is called.

Mark Abkowitz. Mark is Professor of Civil Engineering and Management Technology at Vanderbilt University, and Director of the Vanderbilt Center for Environmental Management Services. Mark chairs the Board's Panel on System Integration, and is the Board's technical lead on transportation.

Howard Arnold. Howard is a consultant to the nuclear industry, having previously served in a number of executive positions, including vice-president of the Westinghouse Hanford Company, and president of Louisiana Energy Services. Howard chairs the Board's Panel on Preclosure Operations.

Thure Cerling. Thure is a Distinguished Professor of Geology and Biology at the University of Utah. He is a geochemist, with particular expertise in applying geochemistry to a wide range of geological, climatological, and anthropological studies. Working with Panel Co-Chairman George Hornberger, Thure is our technical lead on the Natural System.

David Duquette. David is the John Tod Horton Professor of Materials Engineering at Rensselaer Polytechnic Institute. We want to congratulate him for that recent appointment. His areas of expertise include physical, chemical, and mechanical properties of metals and alloys, with special emphasis on environmental interactions. Working with Panel Co-Chairman Ron Latanision, David is the Board’s technical lead on Corrosion.

George Hornberger. George is the Ernest H. Ern Professor of Environmental Sciences at the University of Virginia. His research interests include catchment hydrology, hydrochemistry, and transportation of colloids in geological units and media. George co-chairs the Board’s Panel on Postclosure Repository Performance.

Andy Kadak. Andy is Professor of the Practice in the Nuclear Engineering Department of the Massachusetts Institute of Technology. His research includes the development of advanced reactors, space nuclear power systems, and improved licensing standards for advanced reactors. Andy is the Board’s technical lead on Thermal Management.

Ron Latanision. Ron is an Emeritus Professor at MIT, and a principal and Director of Mechanics and Materials with the engineering and scientific consulting firm, Exponent. His areas of expertise include materials processing and corrosion of metals and other materials in different aqueous environments. Ron co-chairs the Board’s Panel on Postclosure Repository Performance.

Ali Mosleh. Ali is the Nicole J. Kim Professor of Engineering and Director of the Center for Risk and Reliability at the University of Maryland. He has done risk and safety assessments on a number of facilities, reliability analyses, and decision analyses for the nuclear, chemical, and aerospace industries. Ali is the Board’s technical lead on Performance Assessment.

William Murphy. Bill is a Professor in the Department of Geological and Environmental Sciences at California State University-Chico. His areas of expertise are geology, hydrogeology, and geochemistry. Bill is the Board’s technical lead on the Source Term.

Henry Petroski. Henry is the Aleksandar S. Vesic Professor of Civil Engineering and Professor of History at Duke University. His current research interests are in the areas of failure analysis and design theory. Henry is the

Board's technical lead on the design of Surface Facilities.

Before we get started, I'd like to say a word about how the Board works, and how we conduct our meetings. I'm sure you all are aware that the Board is a technical and scientific peer review body. It is an independent federal agency in the Executive Branch. Our mandate is to evaluate the technical and scientific validity of DOE's activities relative to the implementation of the Nuclear Waste Policy Act.

The Board is not constrained by having to judge the adequacy of compliance requirements. That's left to the Nuclear Regulatory Commission. An example of Board product is the recent report that the Board completed on infiltration. And, I just want to make a comment or two about that.

That report is now on the website. The hard copy has not been distributed yet, but I believe we're making executive summaries of that report available for you in the back of the room, at least that was the plan. Has that been done? Yes.

That report kind of epitomizes what this Board is all about, because sometimes there's confusion between what we do relative to DOE, or NRC. We are not approving licenses. We're here to satisfy ourselves that the work that is being done is on solid technical and scientific basis.

And, the principal driver for our evaluations is a realistic assessment of radiation doses at the accessible boundary. There are other issues that we look at as well, such as through-put performance of the repository. We certainly look at issues having to do with security, and we look at operations issues. But, the driver, the fundamental objective is to establish and satisfy ourselves that there is a scientific and technical basis for the overarching objective of whatever the radiation doses are at the accessible boundary.

A comment on the infiltration report, because it is an important milestone in many respects. The report came about as a result of a Congressional hearing. The hearing was many months ago, had to do with some of the considered deficiencies of the Quality Assurance Program existing in the Department in relation to the Yucca Mountain Project, and we were called in to testify from our perspective. And, while we are not evaluators or reviewers of Quality Assurance, they nevertheless were interested in our opinions about some of the technical issues associated with the project.

And, one of the issues they were very interested in was what was going on with the infiltration rates and their validation. And, our commitment at that hearing was to get back to Congress with a report, and the report that I'm referring to is response to that hearing question.

This report involves the Board looking at what work was done in infiltration, both by USGS and by the Department. And, it's an interesting example, because the Board concluded that the technical work was of high quality in both instances, but the boundary conditions in both instances were very different. In the case of the USGS where there was cited some deficiencies with respect to Quality Assurance, they nevertheless anchored their infiltration analyses to site data. This is something the Board is very interested in and concerned about, and that is that the analyses, to the maximum extent that they can be, are anchored to site specific data.

And, so, in spite of the deficiencies, whatever they were, and as I say we did not evaluate those in Quality Assurance, the USGS infiltration rates were, we considered, as very sound science. And, interestingly, those results were about a third of the infiltration rates that were later developed by the Department of Energy, and in interest to separate themselves from what was envisioned as an effort that was not considered under the auspices of an acceptable Quality Assurance Program.

The work that was performed by the Department, therefore, did not have the access to the site characterization data, or did not incorporate the use of that data, and tried to arrive at it by infiltration rates by entirely different approach. The approach, given the conditions that they chose, was very sound. In fact, the work that they did from the standpoint of characterizing the uncertainties, was outstanding. But, still, given the position of looking at two sets of information, two situations, the Board came down more favorable with respect to the USGS results, which were much lower rates, on the basis that those analyses were more scientifically based by the fact that they had site data.

The other thing that this report points out is that it is possible to have sound science in an environment of a deficient Quality Assurance Program, and it is possible to have poor science in the environment of a competent Quality Assurance Program, the point being that these are not equated necessarily.

So, this is a very valuable effort, in our judgment, as to what the Board can do, and that our focus is strong with respect to the technical basis, the scientific basis, but sometimes, the issues having to do with prescriptive requirements are not as relevant, from the science basis, as they might be. And, we thought it was important to make that comment.

Now, I want to move on and say one other thing about how the Board works, and that is that in these meetings, we express quite freely our views, our opinions, and we want to be able to continue to do that. So, when we speak as Board members, we are speaking as individuals, not on behalf of the Board. We will try our best to make it clear when we’re speaking for the Board, and when we’re speaking our own views.

Now, let’s come to the meeting. During the past year, 2007, the Board has expressed interest in the technical areas of transportation, corrosion, and thermal management. These topics will be explored today.

We will start off the presentation with Ward Sproat, who is the Director of the Office of Civilian Radioactive Waste Management, and will give a Program and Project Overview, and we look forward to hearing about the progress on submitting a License Application in the near future.

On the subject of transportation, Gary Lanthrum will provide an update. What appears to have happened over the past year, last year, is the publication of the draft Supplemental Environmental Impact Statement describing the rail routes, and with more recent information, an apparent default selection of the Caliente route. The Board is interested in learning more about the transportation issue and in such details as the anticipated distribution of waste shipments between truck and rail. A realistic thermal strategy has been of interest to the Board for quite some time. Thus, the Board looks forward to a presentation on Thermal Management by Jack Bailey and Ernie Hardin. We’re specifically interested in the evolution of the temperature limits and how a thermal loading other than that specifically used by the Total System Performance Assessment can be accommodated.

The subject of corrosion was, of course, of major interest to the Board during the past year. It still is. We communicated our interests and concerns to the Director of the Office of Civilian Radioactive Waste Management on a couple of occasions, January 12th and July 10th. The response we received from the Director on November 20 addressing these interests and concerns has been reviewed by the Board, but it appears that additional clarification will be necessary to satisfy the Board’s interests. Thus, we look forward to the presentation today by Paige Russell and Doug Wall just before the lunch break on the Plans for Long-Term Corrosion Testing and some Recent Results.

Following the lunch break, we will hear from Zell Peterman about the Effects of Temperature on the Composition of Soluble Salts in Dust. And, then, we will finish the presentations with Pat Brady describing a Waste Form Degradation Alternative Analysis which may affect the source term strength of transporting radionuclides. And, of course, anything to do with the source term is of great interest to me, as I firmly believe that the credibility of a source term determines the credibility of the entire performance assessment.

As is our practice in these meetings, following the presentations, we have scheduled time for public comment. If you would like to make a comment at that time, please enter your name on the sheet at the table in the back of the room. And, of course, written copies of any extended remarks can be submitted, and will be made part of the meeting record.

Some of you have asked about questioning during the course of the presentations. Our preference is really for you to write down your questions and submit them to either Davonya or Linda in the back of the room, and we will cover as many as we can, time permitting.

And, finally, to minimize interruptions, we would like to ask all of you to put your cell phones on the silent mode. And, it’s important to also remind the Board members that when they speak, they speak into the microphone, so that we can make sure that our record is well established.

Okay, I think with that, we will move on with our first presentation, and I’d like to welcome Ward Sproat to give us an overview of the Project. Ward?

SPROAT: Well, good morning. And, thank you for the invitation to address the Board.

What I’m going to do this morning is to give you an overview of what the Program has accomplished over the last 18 months, compared to what we said we were going to do, and what we told you we were going to do, about 18 months ago. And, then, talk a little bit about the future going forward.

Now, I joined the program in June of 2006, and at that time, I went in front of Congress in July of that year and laid out a multi-year major milestones schedule for the program. And, we have passed some of the dates that I laid out in that initial milestones schedule, so I'd like to talk about what the program has accomplished relative to that schedule that we set 18 months ago.

The first milestone that I set was that we would complete the design of the repository needed to support as input into the License Application by the end of November of 2007. At the time we laid that milestone out, we took a look at the various engineering work products that were needed to support that, and we ended up with slightly over 1,000 separate engineering work products that were needed, drawings, specifications, calculations, analyses, and a lot of people said that's a lot of work, we don't know if we can actually get that done.

I'm happy to report that on December 1, 2007, the last of those engineering work products was completed. So, that milestone was met.

The next milestone I laid out was that we would certify the licensing support network by December of 2007. And, I think most of the Board is familiar with the issue of the licensing support network and what it is. Essentially, no other NRC licensing proceeding has had something like this before, where essentially we are producing documents for discovery prior to License Application submittal, and making those available on the web for anybody who may want to intervene in the licensing proceeding to look at them ahead of time.

There were approximately 3.5 million documents, including e-mails, that we needed to include in the licensing support network, and certify to the NRC that processes and procedures in place to keep the licensing support network up to date on a going forward basis, that all the documents we currently have completed in our possession are on the LSN, and, so, we set the date of December of 2007 to get that done.

As you are probably aware, DOE tried to certify the LSN back in 2004, and failed miserably, just really didn't do the job that was needed to make that happen. We certified the LSN early, on October 19, 2007. The State of Nevada challenged that certification, as we expected they would, on the basis that all the documents that we'll ever have aren't done yet. No kidding. And, that, therefore, it shouldn't be certified. The pre-application presiding officer board who reviews the pre-application activities for the NRC rejected Nevada's challenge to that certification. So, therefore, the certification stands. Nevada yesterday have appealed that decision to the Commission itself. We'll see how that goes.

But, the bottom line is Nevada and all the other potential intervenors in the proceeding need to certify by tomorrow. They need to certify that their processes and procedures and training are in place, that their LSN processes and collections are up to date, and all of their documents are loaded on the LSN. They need to certify that by tomorrow also.

So, the certification process and the LSN are moving forward, in accordance with the schedule that the NRC regulations require, and we completed that major milestone ahead of schedule.

The next milestone I laid out was that the Supplemental Environmental Impact Statement for the repository would be completed by the end of May of 2008. The drafts were issued back in October. We conducted nine hearings, seven in Nevada, one in Washington and one in California. The public comment period, we had a 90 day public comment period on the Supplemental EIS's. That closed last week, so we are now in the process of collecting all the comments, grouping them, and beginning the comment resolution process. We are on schedule to get that completed in late

May of this year.

But, the bottom line is is that we produced, the organization produced actually three different Environmental Impact Statement documents in draft form on schedule in October, held the public comment sessions on schedule, and we are still on schedule a year and a half later after setting these dates to make those key documents complete, in preparation for submittal of the License Application.

I set the very public date of completing the License Application and submitting it by June 30th of this year. As of January 1st, we were on schedule to do that. I think as everybody knows, when Congress passed the fiscal year 08 appropriations bill for the Energy Department, there was $108 million reduction from the President's request for the Program. And last week, on Thursday, I completed the allocation of that remaining budget to all the Program offices and management teams. They are currently going through the numbers that I have given them to determine what they can produce, what the impact is on their business plans for the year, and re-programming their business plans.

So, exactly what the total impact on the Program is in terms of License Application completion and submittal, don't know yet. I think anybody who has been in the private sector knows if you get your budget cut by 20-some percent a quarter of the way through the fiscal year, you've got to do some major rejuggling and resource reallocation, and that doesn’t happen in a couple weeks.

So, that’s where we are. I am “cautiously optimistic.” That’s the way I was quoted in the paper, and that’s an accurate quote. I’m “cautiously optimistic” we’ll be able to get a license application in to the NRC sometime in calendar year 2008. I can’t stand behind 100 percent the June 30, 2008 date right now until we finish our re-evaluation on an office by office basis of what the impact of that budget cut is. But, it’s possible that we will get a license application in, and by the way, I’m going to say one thing very clearly here for the record.

This isn’t about getting a license application in to the NRC this year, regardless of what it says. I’m the person who has to sign the license application out to the NRC, and I’m the person who has to basically certify that all the material statements in the license application are correct. And, we are going through, as part of this license application, a preparation process. We are going through and identifying literally thousands of material statements in this license application, and going back and tracing them back to ground zero where they came from to make sure that every material statement is accurate and traceable back to original calculations and analyses. We will not submit, I will not sign out a license application just for the sake of meeting an artificial date, unless it is accurate, correct and high quality. So, that's where I've put a stake in the sand, and we'll see how it goes.

But, as I said, I am "cautiously optimistic" we'll be able to get a license application in sometime during calendar year 2008. But, it's not a certainty or a done deal at this stage of the game.

Next slide? The next milestone I laid out was that we would start the Nevada Rail construction by October 2009. That's not going to happen. It's a minimum of a two year delay from that. I'm going to come back and I'm going to talk about money in general on the Program, because the Board needs to understand the impact of the funding mechanism on the Program's schedule, and it flows through the rest of these milestones on a going forward basis.

Obviously, the transportation piece of this Program is as we laid out our new milestone schedule back last year for the whole program, and we did our integrated planning and scheduling. The transportation piece, particularly Nevada Rail, was off critical path. Critical path goes through license application, license construction authorization receipt and detailed design completion. And, Nevada Rail was off critical path. So, when we received this budget cut, I made decisions about reallocation of resources, and basically took money away from Nevada Rail and transportation, and Gary will talk about that later.

But, the bottom line is we're putting the money on the critical path activities, which for us right now is completion of design and completion of the license application. So, a minimum of a two year delay on Nevada Rail construction for sure, based on the funding, and I'll talk a little bit more about the impact on the rest of the program in a few minutes.

The next milestone, getting construction authorization in 2011. I said that's the best achievable date. That is still a best achievable date, but I just want to make sure you understand what that is predicated on. That's predicated on submitting the license application by June 30th of this year, and the NRC completing its review and making a decision in 36 months after docketing. And, that 36 months is based on what the Nuclear Waste Policy Act requires, but it does give the NRC an out for an additional twelve months if they come back and tell Congress they need it. I highly suspect they will.

But, again, in terms of that 2011 date, that's still a best achievable date if we get a license application in by the end of June of this year.

Operating license submittal in March of 2013. Again, strictly predicated on adequate funding and when we are able to start construction.

The rail line operational in 2014. Again, a minimum of a two year slip, but again, only if adequate funding is provided. We're getting a theme here about funding for the program.

And, then, finally, the March 2017 best achievable date. That date is not achievable at this stage of the game. In fiscal year '07, received $100 million less than the President's request. In this fiscal year '08, $108 million less than the President's request. You cannot keep cutting budget requests by 20 and 30 percent a year and maintain the schedule. It's just not realistic.

So, what we are currently doing is we are re-baselining the program based on the funding we actually got, and, so, sometime later this spring, we will come out again with a revised program baseline, revised program required cash flow, and a revised best achievable opening date. But, 2017 isn't it. DOE and the Program have had a history of year after year, I remember the 2010 date, which seemed like stayed frozen in time forever, and everybody realized it wasn't realistic, except maybe DOE, I don't play that game. 2017 ain't gonna to be made (sic.), and I think everybody needs to understand that.

Go to the next slide. So, I want to talk a little bit about what you're going to see coming out of this Program and what you've already seen coming out of this Program recently, to kind of give you a sense of the progress that we’re making.

I talked to the Board last year about the three independent assessments that I commissioned when I first got here. I talked about the need to do an independent assessment of the Quality Assurance Program, its design and implementation, and an independent assessment of the Engineering Program and the processes and procedures across the program, and I talked about the need to do an independent assessment of the old draft License Application to see what weaknesses and holes it had in it, so that we can learn from that and incorporate those lessons learned into the License Application we’re currently developing.

All of those independent assessments are done. The independent assessment on the old draft License Application is the only one we are not releasing publicly at this time. It will be released publicly later. And, the reason it’s not being released publicly at this time is because we consider it privileged under deliberative process. In other words, the output of that is being used to develop the new License Application, and under deliberative process pre-decisional privilege, we are holding that information closed to help us finalize the actual License Application. Once we submit that, then that independent assessment will be released also.

But, the other assessments, the one on Engineering and the one on Quality Assurance, have been released. They have been posted to our website. As part of the Quality Assurance Program independent review, they also had a Quality Assurance management assessment, looking at the overall management of the program associated with quality. So, all of those assessments are on the website.

I was very pleased with the outcomes. I was not pleased with how long it took to get these done. I really wanted them done probably within six months after getting on board so that I could take the output of those and put them into our improvement plans, and drive improvements to fix the issues that they raised. Unfortunately, they didn't get done until this past fall, for slow-downs in the procurement process.

However, I guess the good side of having them come out later was that when they were done, a number of improvements that we had done in both the Quality Assurance Program, the management processes were already in place, so when the assessment teams came in from the outside and took a look at them, they saw a significantly improved program in both Engineering, Quality Assurance, and across the board. So, when you read those reports, I think what you will see is a very positive picture of the improvements that have been made in the program, and a markedly different quality focus culture in the program than existed maybe two years ago, or earlier than that.

So, these were actually, I think, very good news for the Program, gave us some very good additional areas for further continuous improvement, but in terms of being confirmatory of the improvements we made in the Program, found it very reassuring that the outside experts that we brought in with a lot of senior nuclear management experience, the changes in the Program were positive changes in the Program, were very visible to that team.

The TSLCC, the total system life cycle cost estimate, which I thought I was going to get out sometime in November, until we went through inter-agency reviews, O&B required us to, we had that in constant dollars, they wanted to see it in current dollars also. So, we had to go back and redo all the calculations. That has now been done. It’s going through final inter-agency reviews. I hope to have that out sometime in the next month or two, and that will take a look at, again, the total system life cycle cost of the entire Program through repository closure.

The upcoming, also, will be the Final SEIS’s for the repository and the rail, and I’ve already spoken about those, but they are on schedule.

The license application. Again, I’ve already spoken about, and, as I said, I am “cautiously optimistic” we will get it in sometime this year. I just don’t know yet whether or not June 30th is achievable.

The Fee Adequacy Determination. This is a requirement of the Nuclear Waste Policy Act that the Secretary determine the adequacy of the waste fund fee of one mil per kilowatt hour. I do intend to issue a fee adequacy report sometime probably mid-year after the TSLCC is out, because that's an input into that calculation.

I have spoken to the Board briefly previously about the report to Congress on the need for a second repository. The Nuclear Waste Policy Act requires the Secretary to issue such a report by January of 2010. We're going to do that this year, because the 70,000 metric ton administrative limit for the repository will be fully allocated by 2010, and we know that now from the existing fleet, operating fleet of nuclear plants. So, we intend to basically document the analysis of that allocation of the 70,000 metric tons between civilian and military, and then talk about options. What are the options to either building a second repository or alternatives to needing a second repository. So, that report is currently being developed, and I hope to have that out sometime probably in the summertime.

The last item on there is Interim Storage Report, and this is kind of interesting, because in the fiscal year '08 appropriations bill, Congress inserted some language basically directing us to develop a report on the pros and cons and feasibility of centralized government interim storage. So, I'm putting that up there to kind of just put a marker that we've been asked to do that. I'm having a little trouble tracking down exactly who put those words in the appropriation bill, and I need to talk to that person to find out what they want. But, that will go forward, and in terms of the timing of that and exactly how broad it will be and how detailed it will be, don't know yet. I need to find out what Congress is looking for from that. But that is something that's in the '08 appropriations bill that you ought to be aware of that's being asked of us.

I think from one of the themes you're obviously getting, I think, from this discussion is the funding issue, is a major impact on this program. And, I have talked about this at the last Board meeting, and I want to reiterate it here, because it is, while I know this Board is very technically focused, as it should be, the ability of the Program to move forward on a schedule that those of us in the private sector are familiar with is strictly dependent on resource allocation, resource availability. And, when the Nuclear Waste Policy Act was passed, its framers clearly wanted to make sure this program was not subject to the annual appropriations battles in the Congress, and have this program compete against all of the other federal priorities. And, that's why the Nuclear Waste Fund fee was established, why the Nuclear Waste Fund was established, and that that funding mechanism was intended to be able to fund this Program on a schedule to get it done.

As I think I explained at the last meeting, that is not currently the case. The Gramm-Hollings-Rudman Act in the early to mid nineties essentially broke that funding mechanism so that the $750 million a year that comes into the federal government from the Nuclear Waste Fund, from the fees from the nuclear generators doesn't get credited to this Program and doesn't get offset at the appropriations, because they are characterized as mandatory fees, this program is characterized as a discretionary program. You can't use mandatory fees to pay for discretionary programs. Therefore, you have a disconnect in the funding mechanism, and the Program ends up competing against every other federal priority for increasingly limited resources, which is exactly what the framers of the Nuclear Waste Policy Act were trying to avoid.

As a result, you have a situation like developed this year in the Congress, where during the normal appropriations process, the President asked for $495 million, and the House appropriated $495 million, the Senate, $50 million less than that. And when the whole budget reconciliation process was completed, because of the budget caps that both the House and the Senate Appropriations

Committees were given to meet a bottom line number, the program was cut by $108 million.

And, I would like to point out that it was just not this Program. Almost every program in the Department of Energy was significantly cut back as a result of that. And, from my discussions with people up on the hill, the direct quote was nobody liked the end result, and everybody's ox got gored through this process.

So, I encourage you to, you know, my perspective on this is that this final bottom line number that we got for fiscal year '08 is not necessarily a reflection of dissatisfaction in the Congress with the Program. It's more of a reflection of the reality of the appropriations process that the Congressional approach committees are operating under given the constraints we have. And, quite frankly, the only way this program is going to be able to move forward on any kind of an accountable schedule is to get that funding mechanism fixed, so that the Nuclear Waste Fund fees and the Fund itself can be used for their intended purpose to fund this Program.

So, with that, I'll be happy to answer any questions the Board has.

GARRICK: Yes, Ron?

LATANISION: Latanision, Board.

Just to follow up on that last point, do you have a champion in the Congress who is likely to follow up on making that fix?

SPROAT: Yes, we do. We have a number of them. We started in the House, and we talked to the House Budget Committee staff. The two key committees to address this issue are the House Budget Committee and the Senate Budget Committee. We went and talked to the House Budget Committee chaired by John Spratt from South Carolina. They held a hearing on this issue last November, within four weeks after us approaching them. That committee is very concerned about this issue, and Congressman Spratt is very concerned about this issue, particularly being from South Carolina where there is, obviously, a lot of nuclear waste bound for Yucca in his state. So, there was a lot of interest in this.

And, while there is clearly interest in the Senate among certain players, the question will be, you know, can we actually make something happen on this issue in the Senate this year. All I'll say is I have not given up on that, and there are funny things happen during election years, and lame duck last years of administrations in Congress. And, we'll see what happens.

LATANISION: Who is the point person in the Secretary's office for that legislation?

SPROAT: I am at this point.

GARRICK: Andy?

KADAK: Kadak, Board.

Ward, could you go to Slide 3? I'm trying to understand once you get your construction authorization, let's just say it's September, if that's the case, it means that you can start construction?

SPROAT: Yes.

KADAK: And, the operating license submittal means that the construction is not yet complete?

SPROAT: Right.

KADAK: But, you're ready to resume operations once it is complete, which according to this schedule, could be, say, 2017. I'm trying to figure out what is the construction time to build all the facilities and the subsurface facilities, based on your chart?

SPROAT: The schedule we've laid out here is that the critical path for construction, with certain assumptions about site work, preparation work in terms of roads, infrastructure, that type of thing, and we've made certain assumptions about being able to do work outside the growth, in terms of getting infrastructure that we need, so that when we get that construction authorization, we hit the ground running in construction, that still remains to be seen how successful we are at that.

But, on this schedule, we're assuming that essentially there is a six year construction period of the what we call the initial operations configuration, so that we can start to receive Naval spent nuclear fuel and the first shipments of commercial spent nuclear fuel, but there are construction activities of major facilities still proceeding on site after that. So, this is the shortest, best achievable schedule.

The operating license submittal of 2013 was a date that we picked based on making certain assumptions about how long it would take the Commission to make that decision, with some float in it, quite frankly.

KADAK: The other question is relative to the 25 percent budget cut, what does that practically mean to the people who work here in Nevada in terms of jobs?

SPROAT: Well, the thing about this is, first of all, people have to recognize that when we laid out our very detailed business plans, execution plans for fiscal year ’08 in June and July of ’07, so we’ve had a very detailed spend plan and work plan geared to getting the License Application done, and all the engineering done since mid last year.

That spend plan in fiscal year ’08 is front-end loaded. This is not a levelized spending plan through fiscal year ’08. It’s a front-end loaded process that drives us to get the engineering complete, drives us to get the science work completed and integrated, and then get the License Application drafted, checked and finalized. So, it was a very heavy front-end loaded schedule.

As a result of that, you know, as a result of getting this cut a quarter of the way through the year, what it does is, you know, we were spending at a certain rate during the first quarter, now that means we've got that much less in the remaining three-quarters of the fiscal year than what we had planned on. So, while there was always intended to be a reduction in staffing on the Program during the last three-quarters of the year, this cut will make those staffing cuts even significantly higher and larger.

So, in terms of which specific people, from which specific organizations are affected, that's being worked on now. That's exactly where we are now in terms of the management teams figuring out what they can do with the money, with the dollars that we've given them, who has to stay, who has to go, when do people go, that's where it stands right now.

KADAK: Is there any opportunity for Congress to come back and restore some of this funding?

SPROAT: I think that's a low probability of that.

GARRICK: Ward, I want to ask a question, and I don't quite know how to ask it. But, you have cited a number of constraints on the Project, the most important one being funding. But, at the same time, you will hear people, especially in international circles, saying that they wish they had your budgets to get this job done, implying that they are much more efficient.

Are there constraints that keep you from organizing the Human Resources, for example, in an optimum manner to get the job done? You have an infrastructure that it seems--that you seem to be somewhat at the mercy of that's very complex, somewhat disjointed at times, so from a project management standpoint, it looks like a very difficult thing from a Human Resources standpoint.

Can you comment at all about that? If you had a free ticket to organize and attack this project in as optimum a fashion as possible, do you not think that the budgets you're getting are adequate?

SPROAT: There are probably multiple questions in that.

GARRICK: There are multiple questions.

SPROAT: First of all, let me say this.

GARRICK: I told you I didn't know quite how to ask the question.

SPROAT: Let me say this. First of all, I do have, as director, I have a lot of freedom and capability to re-allocate resources across the Program, and to make organizational and programmatic changes. So, I have that authority. This Program has got a 20 plus year history, as the Board knows. So, in a way, when I came in, I'm playing with the cards I've been dealt.

However, what I'll say is we've made a lot of changes in the way this Program is being managed, the way it's organized, and we're continuing to make changes. I feel pretty confident in saying if we hadn't made the changes we made over the last 18 months, and we got hit with this kind of a budget cut now, the probability of getting a License Application in 2008 would have been zero. That's not the case now. And, it's because of the changes we made, and the downsizing of the Program, and the refocusing of the Program, and the way we've changed the management structure in the Program, that we have a possibility of making that happen.

Now, don't get me wrong. $390 million budget is a lot of money. Okay? There are 20 plus or minus, about 2400 people working on this Program right now, and you may say well, what are they all doing. Well, they're all focused right now on getting this License Application done. That's what the vast majority of them are doing. And, while the $300-some million or $400-some million is certainly enough to keep a core staff together, and to defend that License Application if and when we get it in this year, it's not enough to do this Program in its entirety.

If you take a look at this Program in its entirety in terms of what's still out there to be done, detailed design of the repository, construction of the repository, buying the casks and the TADs, buying the transportation hardware, building the Nevada Rail, operating the Nevada Rail, and developing the national transportation plan infrastructure and staffing, $300 to $400 million a year isn't going to get you any of that, and that's the reality.

I mean, this is an expensive Program to do it the way it needs to be done, and some people think well, you know, yeah, you put this new Program baseline together that shows the 2017 best achievable date, and you came up with--we assumed an unconstrained cash flow to minimize critical path, and to minimize costs, and it comes out to a budget requirement of between $1.5 and $1.9 billion a year. And, some people say well, if you get less than that, it will just take longer. Wrong. At some point in time, you can't build an integrated multi-project program on $300 to $400 million a year of this scope. It just can't be done.

GARRICK: One other question. One of the issues that keeps coming up with respect to this Project is building the public confidence. I understand that among the casualties of the budget cut is a lot of what one might call the reach out programs that are directly related to building public confidence, and what have you. Can you give us an indication of which of these Programs are no longer, because I'm thinking of the closing of the site, no tours, I'm thinking of the information centers, et cetera. What's happening on the public outreach front with respect to these cuts?

SPROAT: As you pointed out, when you get a bunch of cuts, you need to rethink your allocation of resources, and you need to think about how you do things more efficiently and effectively. It really bothers me to have to stop public tours out there, because they were pretty well attended, and we got very positive feedback from the people who went. But, the reality is in terms of the big picture, critical items of what we need to do in the program now, it doesn't meet my critical need criterion, at least for this year.

However, it is pretty clear to me that if we get this License Application in, this is a very complex, technically complex and detailed analysis that goes into the safety evaluation report, and that, number one, the average person isn't going to take the time to understand it, and probably doesn't really want to understand it. They want to get the simple version. And, so, we are preparing, as part of the License Application process, another document that I'm calling the Qualitative Safety Analysis that is a layman's version of the License Application that we're going to have ready and submit that and make that public at the same time we get a License Application done. And, we'll probably have what I call the professional version that you folks would like to read, before you get into the License Application itself, and we'll probably have the layman's version which somebody like I would like to read, so I can read 10 or 15 pages and understand what the basis is of the licensing argument for Yucca Mountain. So, we're working on that right now, and that is going to be one of our key what I call public education deliverables that come out of this process.

GARRICK: Now, is that going to be based on an abstraction of things like the Performance Assessment, or is this going to be more along the lines of something the Board has been interested in for a long time, some sort of representation of what the experts really believe?

SPROAT: I think more so the latter. We're going to put together a discussion that is representative of what I call the non-regulatory focused arguments, as well as it talks about the conservatisms and actual analogs, and why there's a high competence level on a broad level around this whole system is the way we're designing it.

GARRICK: Okay. Yes, Andy?

KADAK: Ward, you mentioned the 1.9 billion, or so, in out years to actually build this, and what you described as a current process of year by year appropriations. It doesn't seem like that's ever going to happen unless something major changes.

SPROAT: Yes.

KADAK: So, your proposal apparently is this off-budget treatment of the waste fund as it was originally intended. Is that the strategy, to try to see if you can get serious money and Congressional support to build this repository?

SPROAT: I think it's the only way of going after this at this stage of the game. There are other ideas in terms of formulation of a--I know there are a number of people in industry that are promoting the idea of some sort of a government owned corporation that manages the back end of the fuel cycle, so that the corporation has access to the waste fund, and that manages, on a commercial basis, whether the back end, so that if fuel is going to be recycled, they handle recycling, what's going to be disposed of in a geologic repository, goes there. They build a repository. All of those are potential options that are out there. But, for the near term, the issue of using the Nuclear Waste Fund fees for their, not only their intended purpose, but their legal purposes, is where the near-term focus is.

GARRICK: Any other questions from the Board?

(No response.)

GARRICK: From the staff?

(No response.)

GARRICK: Well, we thank you very much. Thank you.

Gary, you're on.

LANTHRUM: Hello, everyone. I'm Gary Lanthrum. I'm the Office of Logistics Management Director, and responsible for doing the transportation planning for the OCRWM Program.

First slide, please? These are the topics I'm going to be covering. Very quickly, we'll go over the current approach the transportation planning is taking, what our focal points are, talk about some of the things that have happened over the past year, and what the funding requirements are, the status of our system development.

I'm going to present some of the opportunities and options for interested organizations to be engaged in development of the transportation system. That's not going to be a separate discussion, but I'll integrate that into the discussion about where we are on the projects and processes for developing the operations plan.

And, I'll talk about some of the concerns that have been raised over the perception of spent fuel shipment. It was part of a range of issues that were raised by the National Academy of Sciences in their report on the safety of spent fuel shipment, and that was found in the topic of Social Risk that they raised. Perceptions, public perceptions of risk are a big part of what plays into the social risk metric, and I'll be talking about that a little bit.

Next slide? The current transportation development approach is really focused on shipments in canistered form. In the original planning view, we were looking at mostly bare fuel shipments in large rail casks. We're looking now at mostly canistered shipments. And, canisters have always been part of the transportation system. The Naval spent fuel has always been planned to be shipped in canisters. The DOE spent fuel has always expected to be canistered, and the DOE high-level waste had always expected to be canistered.

What we’re looking at now is a shift to include most of the commercial spent fuel also being shipped in transportation, aging and disposal canisters. There’s still the option, though, of shipping some waste in bare fuel casks. Most of that would be in legal weight or overweight truck casks. All of the canistered shipments right now are presumed to be shipped on rail, and, so, rail is a very critical part of being able to develop an operating transportation system that serves the current design of the facilities at Yucca Mountain and the operating plan for Yucca Mountain itself.

The approach with this mostly canistered system, since the fuel is in welded cans that go into the transport overpacks, the transport overpacks used to transport canisters are going to be much cleaner, much easier to maintain. And, so, we’ve got a bifurcated plan on how we would manage and maintain the transportation casks. For the casks that are shipped canistered content, we’ll build a facility close to the repository to maintain those casks. For the small number of casks that would ship bare fuel, where there’s the potential for more complicated maintenance processes, we’re going to buy that as a service from the private sector, and, again, we’re expecting most of the bare fuel shipments to be in legal weight or overweight trucks, where transporting those casks to a maintenance facility somewhere more distant, would be less problematic.

We are also looking at a system that’s designed-- the original plan was to have all of the equipment needed for transportation available on day one of operations, so whatever people wanted to ship, whatever came out of the contract, the standard contracts with the utilities and the commitments to DOE, and other work, we could support anything that came up. The current expectation is that we will have a fairly constrained capability, much like the repository, we’ll be able to transport roughly 400 metric tons of heavy metal in the first year, building up over roughly a five year period, with the capability of transporting 3,000 tons a year. And, so, the costs for initial operations are reduced somewhat. That reduces somewhat our cash flow requirements.

This slide gets back to the structure at least for our planning and management, and this is basically very similar to what I’ve talked to you about before. We have broken down the management into the project areas and the operations and institutional planning areas as being separate activities.

On the Project side, we have the National

Transportation Project, which includes the casks, the rail cars, the special rail cars required to ship these large heavy canistered contents. The canisters for transporting TADs, combining with the overpack, the weight of the TAD itself, the impact limiters, and the equipment to secure it to the rail car, we're talking about roughly a 280 ton package. So, these are very large, very heavy packages.

And, then, we have the facilities required to manage the actual transportation operations themselves, to track the shipments, to maintain communications, to do the pre-notifications to states, to the governors' designees, and then also the facility for doing the maintenance of the rail cask that ship canistered contents.

There's a rail project, and the activities associated with it. We are currently engaged in the Environmental Impact Statement, and we hope in the not too distant future, to be engaged in the preliminary design. We have done conceptual design work to support the EIS, but there is an additional increment of design necessary before we can go up for final design and construction project bids. And, we are hoping that that will proceed in the future. And, then, final design and construction will be the last phase, the Nevada Rail project, before we get to operations.

And, there's a lot of activities over on the institutional and operations planning side. We've got requirements under the Nuclear Waste Policy Act for Section 180(c), which is the requirement to provide funding for training and emergency preparedness, and for technical assistance to states and tribes along transportation corridors.

We have a national transportation plan, the first draft was provided, and we got comments from our key stakeholders. We are preparing an updated draft that will go out to a broader distribution, and engage a larger cross-section of the public that's interested in what our overall transportation planning approach is, and how we're expecting to implement.

We are working very diligently on the operations plan. I have a very early version of it that will take what we put together in terms of a concept of operations, and take it one step further to have more details about how we would actually expect to implement that concept. And, we are looking at the routing issues.

Next slide? This says graphically what Ward said with words. I'm an engineer, and I think in terms of graphs. I love spreadsheets, and my whole presentation would be spreadsheets if I were allowed to do that. In fact, I would sit here and just do graphs if that were possible.

This first curve is the original planning flow of funding, showing cumulative investments over time to achieve a 2017 date for transportation. And, it comes in at just about $4 billion. It's a lot of money, but that gets to the issues that Ward said. You cannot build this capability on the cheap.

Dr. Garrick, you mentioned the fact that some other countries have indicated that they can do a repository system for far less, but most other countries don't have the vast breadth and depth of just geographic expanse that we have. In fact, our railroad is bigger than some countries. This is a very large undertaking. We've got a 340 mile long railroad we're looking at. The ability to transport the volume of material that we're talking about, which is again greater than most other countries are having to deal with, requires significant infrastructure, a lot of rail cars ultimately, a lot of casks. And, the individual unit costs for those items is fairly high. And, so, we've got a very major investment that's required.

This lower line, the three triangles, is the funding that we've actually received so far. And, actually, when this was prepared, we had 18 ½ million for 2008 in transportation. Ward talked about the challenges we've been going through with the actual allocation or appropriation of funds. When the bill that was signed on December 26th, it was, again, as Ward indicated, $108 million less than we thought we had, less than the spending profile. And, so, I'm part of this discussion about how we're re-allocating funds. This number has gone down significantly, and we're looking at what the impacts are. So, this triangle is going to drop down the next time you see the graph.

The other cute little symbols on here are basically the planned and current status of some of our major milestones for developing the projects. The diamond here was when we were planning to publish the rail EIS and the supplemental rail quarter Environmental Impact Statement, and the actuals stayed in line with that. The funding impacts that we have had so far have not driven any schedule slip in the EIS development efforts.

The procurement of an escort car, there are three types of rail cars that we're developing that meet the new requirements under the Association of American Railroads for shipment of spent nuclear fuel. One of them is an escort car. We will have armed escorts that travel with these shipments. Even though our shipments aren't going to happen for a very long time, the Department has other people that are shipping spent nuclear fuel. One of them is the U.S. Navy, the Naval Nuclear Propulsion Program. They have shipments that are going on currently, and they've got a fairly significant work load to defuel operating Naval vessels, ship the spent fuel to Idaho for interim storage until it goes to Yucca Mountain. And, those shipments are going to be going on for a long time before Yucca actually opens. Because of their requirements, they currently have a need for developing the escort car as a key early development, and since it’s the same escort car that we’re going to need, we’re working collaboratively with them on that.

Now, there’s been a slight slip in the schedule there. Again, it’s a shared funding project right now. The design of it is not as expensive as building the actual prototypes and doing the testing of the rail car as it’s going to be, and the big portion of the slip in the schedule right here has not been funding driven at this point, but it’s been more driven by the fact that that procurement has become a classified procurement as opposed to an open source procurement. So, management as a classified procurement has added additional steps in the process, which has kicked the schedule out a little bit.

The circles here, the solid one and the dotted one, are the schedule for procuring designs for DOE casks, and that’s the casks to ship DOE spent fuel and DOE high-level waste. We had originally expected that to happen in 2009. Based on the current budget profiles, 2010 would be the earliest date that that might happen. And, we’re going through a range of rebaselining options for the program as a whole. It’s not just looking at 2008 and possibly 2009, but a broader look at what the likely funding profiles are going to be, and looking at what those impacts might be. And, so, this bottom line may again change significantly before I present to you next time.

The star represents the start of rail construction, and as Ward has indicated, that's already slipped at least two years. We had expected to start in October of 2009, which is basically the beginning of our fiscal year in 2010. That slipped out until two years later at this point, into the 2011 time frame. We're hoping that we can hold something close to that, but again it's going to depend exactly on what the budget profiles are. There's a one to one correlation. We will get the EIS done. We'll have the foundation for moving forward, but the funding profile is going to depend on when we can actually start this work.

The next circle is the green here. That's the cask and buffer car designs, the other two types of rail cars. That's far enough out in the future that it hasn't slipped yet, but looking at our long-range profile for budget funding, and what we expect to get, that may be impacted if the funding in 2009 and 2010 is not sufficient.

The diamonds represent the procurement of the actual cask fabrication, when we actually buy the construction of casks. That's slipped a little bit right now. And, then, the last date up here that slipped significantly is when the railroad would actually be available, and it slipped from the 2014, as Ward had indicated, to the 2016 time frame.

Next slide? On cask systems, the basic approach, as I indicated, most of the commercial spent fuel shipments will be made in the TAD canisters now. I'm not managing the development process for the TAD canisters. Chris Kouts' office is working on that. They've been doing some great work with the collaborative efforts and a lot of industry interactions. The industry was invited in very early to talk about the process, the requirements, and the big driver for the TAD canister, the part that goes inside the transport overpack, is that that canister is intended to be adequate for both storage, transport, and disposal. Meeting the disposal requirements for the repository is one of the more challenging parts of it. The internal design is something that would have to be considered very significantly.

We got proposals in from the vendor community about how they would move forward, what their expectations would be. Those proposals are being evaluated. The next step will be actual procurement. We're hoping that that may take place in the not too distant future.

Independent of the TAD is the design, or somewhat independent, and the process for designing and procuring the transport overpacks for those TADs. The transport overpacks will be designed by the same organizations, the same vendors that designed the TAD in regulatory space. Since the NRC is going to certify these packages, they will certify the contents, the way those contents are packaged, which is the TAD, and the way those contents are transported, which is the transport overpack. The certificate of compliance will cover all three of those, and there will be a very tight bound on that.

We had originally expected that the procurements of DOE casks would be initiated in 2010. It looks like that’s slipping out to 2013 at this point. And, again, based on what our longer range funding profiles are, that date may be subject to slipping even further. And, also, the procurements of the commercial casks to ship by truck have also slipped out until 2013.

Next slide? Rolling stock. There’s, again, the three types of rail cars that we’ll be buying that have to meet the update of the new standard, the AAR. 2043. We’re working with the Naval Nuclear Propulsion Program. And, again, the efforts on the escort car, which is their key requirement at this point, is driven by their schedule, not by our schedule. And, again, I piloted that fact here.

This is an example of the kind of car that we’re looking at for actually transporting the casks. This is a car that was actually designed for the PFS, the storage facility that was being promoted out in Utah. They never completed the testing of this design. We don’t know if it’s actually functional, but we do know what components were used in it. We’ve done a lot of modeling on the performance of components in terms of their ability to meet the specifications required by AAR. We’re competent that off-the-shelf hardware can do that, but there’s been no funding provided in 2007 or in 2008 for the cask or buffer cars. That work is currently on hold, and we’re waiting until there is more sufficient budget authority in order to proceed.

One thing that we’re looking at in terms of our acquisition approach is the probability and possibility of not buying everything before initial operations, but buying only those components and those assets necessary for initial operations, and then as we hire a logistics contractor to actually conduct the shipments, to make them responsible for building out the balance of the fleet. So, the capital costs would no longer be a DOE responsibility. They’d be a vendor responsibility, and we would pay for it through the negotiated rates for the actual logistics services. That defers some of the capital costs and allows us to get a system ready to go sooner than we might otherwise be available under constrained funding.

Next slide? Support facility developments. The cask maintenance facility is going to be designed to maintain the casks, as I indicated, that ship the canistered fuel. Our current plan is to use commercial facilities to maintain the casks that ship bare fuel, which will be primarily legal weight or overweight truck casks. We are hopeful that we can begin the design of our cask maintenance facility in 2010. And, again, the fact that it will be a facility designed to maintain casks that have only shipped canistered equipment lowers the requirements for that facility, and will make the facility less expensive to build.

One of the reasons we're not right now looking at buying this as a service elsewhere is that the cost of transporting these 250 ton packages is very expensive. Rail space is costly. The cost of, we did one quick run of running a train of empty casks from Yucca Mountain to Pittsburgh, where one of the commercial facilities is located, and it was about $80,000 a train. Now, $80,000 a train, if you're running three trains a week, you're talking about a quarter of a million dollars a week worth of costs associated with just transporting the casks to maintenance. And, that basically took that off the page as a reasonable option for us. And, so, we're looking at something close to the mountain for the maintenance of these large and heavy casks. And, again, we're hoping to be able to begin the design of this facility in meaningful ways in 2010.

Next slide? Status of the Nevada Rail line. Ward did indicate that we completed the draft supplemental corridor analysis, and the draft rail alignment Environment Impact Statements. The comment period ended last week. We got a lot of comments. We're working with Dr. Jane Somerson, who is the NEPA document manager for all of the EIS's at this point on how those comments will be addressed.

My team is primarily focused on technical feeds to that process, and doing the leg work to support her management processes. We did hold eight public hearings. They were scattered around Nevada. There was one hearing that was held in California, one held in Washington, D.C. We are scheduled right now for completion and publication of the documents in the June time frame.

One good thing is that--why don't we go to the next slide. I think it feeds into it better. One of the things that's published as part of the draft rail alignment EIS is that we have a preferred alignment. That preferred alignment says that of all the options that we considered in the two quarters that we looked at, were where the rail line would actually go from most of the way through the Caliente corridor, we have mapped out what the preferred alignment is. Now, there are a couple of areas where we were looking to get additional public input before a preference statement was made, or before any decision was made, and one of those areas is around the town of Caliente itself, how we would connect the main line UP track. There were three options there, and there's different places that we could locate staging yards and rail sidings in that area. We have not made any preference about that. That's one of the things that we still have to look at.

But, for the bulk of the corridor, we have established what our preferred alignment alternative is. What that does is it gives us a basis for more detailed discussions with land owners and land users about how to mitigate the impacts of constructing a railroad. Until you know where you're going to put it, it's very difficult to have meaningful discussions about how to deal with impacts. Now that we have largely an idea about where it would be, meaningful discussions with those communities and with individuals out in rural Nevada that either own or use land out there become much more meaningful, and we're going to be engaged in a much stronger basis on those interactions.

With the little bit of decisions that are yet to be made, there's still some variability about the overall length of the railroad. We can't hammer down the exact costs until we have the final determination about what the alignment is going to be. And, a lot of that depends on these last decisions about preferred alternatives in areas where there are still some selection options.

And, the construction time to build this is dependent both on the length of it after we've made our final decisions, and on the funding stream. We know roughly what the cost is. We have a cost range that we've published before. The cost range is between $1.8 and $2.4 billion. Our target cost at this point is $2.2 billion.

But, one of the things that was implied by Ward's presentation and is really true for us is that if you drag the Project out, the total cost of the Project goes up. The actual construction costs don't change, but the management costs, the indirect costs that are associated with a major project are carried for a longer period of time, and you wind up having a higher total cost at completion than you would if you completed the project in less time.

Next slide? On our institutional and operational activity fronts, we did publish the draft 180(c) Policy back in 2007, based on tribal input and concerns, and the fact that there are some gaps in coverage for how it would be applied to tribes. We extended the comment period until January 22nd of this year. That's coming up soon. We hope to be able to drive to closure on a final policy possibly this year. But, there's lots of work to be done.

We had originally planned to do some pilot projects to see how the draft policy worked. And, again, that's one of the things that was a casualty of the budget scenarios. Now, we continue to work on operational planning activities, separate from the YDC. The transportation operations plan is a build-up from the concept of operations, one that we're continuing to work on.

The other thing that we're looking at, it's partially because of comments we have gotten from the Board, is looking at what the impacts are in our ability to actually get out of some of the utilities. What's the status of the rail infrastructure, the near-site infrastructure, particularly for utilities that are located on short-line railroads that don't have the attention of the main-line track.

Part of my long-range planning is to make sure that we work collaboratively with the Federal Railroad Administration, the Association of American Railroads, and the state regional groups that we work with to do short-line inspections and broader near-site infrastructure inspections of the overall transportation capability. Now, we actually conducted one of those this year. We went up with the Eastern Regional Conference and the Council of State Governments. We worked with the FRA and the states of New Jersey and Pennsylvania, and we inspected the Winchester and Western Shortline Railroad. That was a very successful effort. It was one that we got a lot of good feedback out of. It was one that's used fairly regularly, so there were no infrastructure challenges, but that's the kind of work we're going to want to do more of as we move closer towards transportation and actual activities.

The National Transportation Plan Development. It's one of the four strategic objectives we have for the Program. We did publish an early draft and distributed to our key stakeholder groups. We didn't have a broad distribution. We got their comments. Those are being dealt with. We hope to have those incorporated in a revised draft. I'm trying to get it framed so that the next draft that comes out incorporates the current view of what our budget is and when things might happen, because a big part of the value of that plan is to identify to stakeholder groups when they can be productively engaged in key elements of our planning.

If those schedules are shifting out because of budget profiles, I don't want to get them excited about engagement that's not going to happen as it had been planned previously. And, so, we're trying to incorporate the latest budget changes and views into the next revision so that it's a meaningful guide for interactions with the broader stakeholder communities.

Next slide? On the topic of Social Risk, there are a couple things that drive perceptions of social risk, and one of them is the overall perception of transportation risk. In the National Academies' review of the risks associated and the safety of spent fuel transportation, they concluded that there really were no fundamental technical barriers to conducting these shipments safely, that the U.S. regulations are adequate as they are written, and if you comply with those regulations, you can have good assurance of the shipments being safe. And, that the accident risks of spent fuel shipments are far less than the accident risks associated with other hazardous commodities.

So, what drives perceptions that these shipments are, in fact, more risky? That's one of the things we scratch our heads about frequently.

Next slide? This is an actual graphic out of the National Academies' report. It's looking at consequences and probabilities, and, again, this is all in the accident world. This scale here on the left-hand side is a logarithmic scale, and, so, the changes between each of the divisions on the Y axis, the vertical axis, are factors of 10. And, so, you can see the chances of low consequence accidents for spent fuel shipments, which is this line, are one, two, so, that's 10, that's 100, that's just between 100 and a 1000 times less risky than shipments of methanol, and it's almost 10,000 times less risky than shipments of propane or shipments of chlorine.

And, again, that's based on their analysis of accidents. It's a very, very good story, and yet there is a public perception that somehow the reverse is probably true.

And, the same relationship is perpetuated out even to severe consequences analyses. Again, the risks associated with spent fuel shipments, according to the National Academies' assessment, are far less than the risks associated with other hazardous commodities that are routinely shipped throughout this country, and shipped in much larger numbers than we're looking at.

Next slide? When you look at the overall volume of spent fuel shipments in this country, I've got data from--this is, again, just on rail shipments--data that I got from the Association of American Railroads indicated that there were roughly 250 billion ton miles of hazardous material shipments in 2005, which is the latest dates, that the 125 billion ton miles of that was the material shipped by rail.

The vast majority, over 28 percent of that, was flammable liquids. Again, over 20 percent were gasses. Corrosives, around 18 percent. And, you go all the way around this little pie chart showing what the different classes of hazardous materials are, and Class 7, which is radioactive materials, is less than 1 percent of the total hazardous material shipments in this country by rail. And, spent fuel is less than 1 percent of Class 7. Most of Class 7 is low level waste. It's bulk debris from decontamination and decommissioning projects, from rubble in buildings where contamination work was done.

And, so, you’ve got an extremely small part of the overall hazmat pie is spent fuel. and you’ve got lower risk associated with these shipments. And, so, there’s certainly a need for more extensive engagement, and my current plan for most of that is focused on the emergency response communities. It’s one of the areas that we’re obligated to deal with under Section 180(c) of the Nuclear Waste Policy Act. We have a lot of plans, we’re engaging the emergency response communities in meaningful ways.

One of the ways that we’re looking at is if the funding is provided for the Nuclear Regulatory Commission’s package performance study, it’s an update that they want to do to show that the current series and design of transport casks is truly robust, and they’re working on a test plan that the commissioners have been reviewing with their staff that would basically involve a rail size cask impacted by a locomotive at a crossing. And, they haven’t determined where the test would be done yet, but it will be the full impact of a locomotive going 80 miles an hour, clobbering a spent fuel cask, which is a replica of tests that were done many years ago with previous generation casks, and it was Operation Smash-Hit was done in England, and there were a couple of tests in smaller casks that were done at Sandia. This will be the first of a modern generation rail cask done in this country.

But, following that test, rather than just packing up all the camera gear and data collection equipment, what I’ve proposed to the NRC, and they’ve tentatively accepted, is the idea of turning it into an emergency response exercise. Instead of a staged event where you pretend that there’s been an accident, you’ve got an actual accident scene to respond to. And, you get actual emergency responders out there on the ground, and to use that as a training exercise to both show how they could deal with an accident of that magnitude, and to show the robustness of these casks.

These are the only hazardous material transport packages that are designed to survive severe transportation accidents intact. The rest of the containers out there are basically designed to survive no accidents intact. And, so, there’s a good story to tell. The question is how do we get out, how do we work with the emergency response community to tell that story, and then use them to communicate to their communities because they’ve got a lot more credibility, frankly, than the federal government does.

Next slide? And, that just, again, goes into the discussion about how we need to proceed. One of the discussions I’ve had with the excellent community, we’ve done a lot of benchmarking, both with companies in this country and with programs in this country that ship either spent fuel or other hazardous materials, but also internationally.

Since the last time we met, I visited AREVA in France, looked at their transportation program and what they do.

And, I asked them at the beginning of the meeting what they had done to address public concerns about the safety of these shipments, and they basically said they hadn't done anything, that there was essentially a referendum in France in the Sixties about what France was going to do in terms of generating power. They had used all of the viable hydro that was available in the country. Their coal reserves had run out. They had no reserves of oil or natural gas. And, so, the question was do we import all of our energy and become beholding to others about what the cost of that energy is going to be, or do we develop nuclear. And, the country basically decided that it would rely on nuclear.

They get about 80 percent of their electricity from nuclear power now, and because there is support from nuclear in general, there is not a concern about the consequences or risks associated with transportation. And, so, one of their messages to me is that you can't just go out and talk to people about how low the risks are. If there's not a perceived benefit, then no risk is acceptable in the public's eye.

And, so, there's an ongoing engagement on the benefit side of the equation that we need to work broadly with other programs and other organizations to talk about, in addition to addressing the issue of the fact that the risks themselves are fairly low.

Next slide? And, this is looking at some of the benefits of the repository itself. Talking about that benefit side of the equation, currently spent fuel is stored at 121 sites around the country. Many of those sites are close to major metropolitan areas and population centers. Yucca Mountain is in one of the most remote parts of the continental United States, and the Nevada Test and Training Range and the boundary that we hope to have around our site provide a very significant physical protection boundary, in addition to the remoteness. And, so, there are a lot of benefits with this site beyond its scientific and technical contributions.

And, the last slide? There are a lot of projects that have to proceed in order to develop a successful transportation system. We have not funded significantly the cask development or the rolling stock development, or the facility requirements for the past two years, and we won’t until sufficient funding is made available. It’s a major challenge for us.

We have proceeded effectively I believe with the Nevada Rail EIS, and we’ll have a good document out. We expect, based on the fact that we have a preferred alternative that’s been published, that we can have much more significant engagement with communities, land owners and land users out in rural Nevada.

The technology and expertise to do this work exists. It’s work that’s going on currently. We’re not doing something new. This is not going to be first of a kind, one of a kind efforts.

There are benefits as well as small risks associated with this transportation system. We’re going to be working more significantly with other programs within DOE and companies outside of DOE to start focusing on what those benefits are. So, that’s part of the discussion.

And, the costs of implementing the system are not trivial. It is going to be expensive to do this, and to do it right. But, we believe that overall, the benefits justify the investment of those funds, and we’re hoping that Ward is imminently successful in his discussions about doing the funding things.

And, with that, I’m open to questions.

GARRICK: Mark, we start with our technical lead on transportation.

ABKOWITZ: Thank you, John. Abkowitz, Board.

Gary, first of all, let me thank you for the substance of your presentation, and also wanted to acknowledge that I think your program has done a lot with a little. I’m particularly pleased that the decision has been made to keep the tech. stakeholder group activity going in light of your budget crisis, because that is the most important outreach feedback mechanism that I believe you all have. And, we're also glad that you're taking a look at the short lines, because that is one of the weak links in this system.

LANTHRUM: Understand.

ABKOWITZ: Ward mentioned earlier that budget focus is on issues related to the license application, and I wanted to explore a sequence of three or four considerations with you, not so much that these are decisions that you make, but I'd like to probe the involvement your group has had in weighing in on those decisions.

LANTHRUM: Certainly.

ABKOWITZ: The first thing has to do with the TAD and the railroad, and I think I know this information already, but I'd like you to validate it. Given the weight of the TAD with its overpack and impact limiters, et cetera, and we can guestimate that's about 180 tons, or so.

LANTHRUM: Yes.

ABKOWITZ: And, given that it would require a heavy haul permit and probably block two full lanes of a road if shipped by truck, it's pretty clear that the only option to operate a TAD based repository is to have the rail line available in Nevada. So, would you confirm that there's pretty much only one way to do this, and that requires a 300 plus mile railroad that doesn't exist right now?

LANTHRUM: That is very true. One of the reasons that I went to France to do the benchmarking visit with AREVA was to look at their transportation approach, because as you're aware, they have rail across most of France, and the rail only goes to Velonia. It ends. And, 30 kilometers, roughly 16, 17 miles away, is where the actual reprocessing facility at La Hague is, and, they do heavy haul for the last portion of the trip. It's about 16 miles, though, and that heavy haul trailer travels at 20 miles an hour. Well, that's not bad if you're going less than an hour's trip.

The other thing is that the casks that they use are significantly smaller than the casks that we use. They transport 8 to 12 assemblies at a time, whereas, the TAD that we're looking at for PWR assemblies is currently designed to hold 21 assemblies. So, we've got a heavier, larger package, and if it were only a 16 mile trip, it might be conceivable. But, the fact that the current nearest pass of mainline UP or BN track is 300 plus miles away, at Caliente, or the interchange up at Beowawe on the northern part of the state is even further, that's about 360 miles away, and if you get over towards the Reno side of things up around that northwest corner of the state, the common use BN, UP line goes through these, and you're about 270 miles away, that length of a distance with the very slow passage, and the breadth of one of these heavy haul transporters just makes that a not really a viable option at all for us.

ABKOWITZ: Okay. So, conclusion number one is there is no surface facility to design or a repository to operate if there's no Nevada Rail Line?

LANTHRUM: That's not entirely true, because there has always been an expectation that there will be a capability for taking some truck shipments. There is a wet handling facility at the repository that will be able to take bare fuel out of casks. We're looking at primarily legal weight or slightly overweight truck casks. We're talking 50,000 pound casks that may hold four assemblies. That will always be part of the make-up, but you'll never get the through-put that the repository was designed for without rail. You will be able to do some processing, but it will be very constrained.

ABKOWITZ: Okay. Now, let's move on to the second consideration. The delay in the TAD production process that you referred to implies that over time, utilities will be putting more waste into dry storage, which implies that you may actually have a lower percentage of TADable wastes coming into the facility, which implies that you're going to have to do a lot more handling in the CRCF--I'm sorry--a lot more handling in the wet handling facility. Is that a correct assumption?

LANTHRUM: Yes and no. We’ve always known that the utilities will continue to discharge waste, and much of that waste will go into dry storage in the period between when we submit the License Application and the time when we actually start operations. We are hopeful that since the TAD is designed to support and accommodate, transport storage, and disposal, or aging and disposal, that the utilities would choose to use TADs for their own dry storage.

And, so, as they’re doing their discharges to their pools, and then five years later, taken the fuel out of pools and go into dry storage, we’re hopeful that they will choose to do that in TADs themselves, which will just be--

ABKOWITZ: But, if the production of TADs is being delayed, then it’s not an option until further out in time; is that right?

LANTHRUM: It’s not an option today; that’s correct. And, so, for discharges happening to dry storage at this point, then they’ll still be using the dual purpose casks.

ABKOWITZ: Okay.

LANTHRUM: And, it was always anticipated that we would have the capability of dealing with a range of fuel forms, and that’s one of the reasons the wet handling facility is there, so that we can deal with bare fuel and the possible need to deal with dual purpose casks at some point possible.

ABKOWITZ: Okay, now I want to move on to the waste management system logistics from waste acceptance to receipt at the surface facility.

There is the TSM model that's being used to try to explore the dimensions of that, is my understanding.

LANTHRUM: Yes.

ABKOWITZ: And, I was curious to find out how much input the transportation group has had into that process. For example, and correct me if I'm wrong, but it's my understanding that those models assume that the loading at the utility site will take only one week once the TAD and rolling stock arrive, and that the repository will empty and release casks within one week of receipt for maintenance and reuse. Are those assumptions the ones being used right now? And, were you consulted on that?

LANTHRUM: We were consulted. We were involved in the process. The turnaround time at the utilities is something that transportation is not currently engaged actively in dealing with the utilities. One of the actions that Ward assigned to us was to develop a process for more significant engagement with the shipping sites, and it's not just the utilities, it's also the DOE sites, about what their processes are and what their flows are, what their timing and expectations are. Until we get closer to shipment, it's not really viable to have detailed discussions about that. After the delivery commitment schedules are signed, I think that’s 63 months before the initial shipments actually start, that provides a basis for some more detailed interactions. Until then, it’s not likely that anybody is going to want to have significant engagement with us, or to make commitments.

ABKOWITZ: Although, I understand that from the utility perspective, it might not be unreasonable for it to be a two or three months turnaround, because they just don’t plain shut down to load. They make electricity. And, so, consequently, if this assumption, or these assumptions have not gained the benefit of the transportation group’s experience, or whomever, the cost to maintain the proper fleet to maintain the proper receipt schedule is going to be different, is it not?

LANTHRUM: The total project costs aren’t, because we know the total volume of fuel that’s going to be generated, and can go into Yucca Mountain, and, so, the total number of TADs necessary is not going to change--

ABKOWITZ: But, if you need a lot more rolling stock and a lot more empty overpacks, et cetera, et cetera?

LANTHRUM: The rolling stock shouldn’t be impacted, because we’ll roll up to the gate, and we’ll drop off TADs and casks. The rolling stock will leave. It may require that more TADs will have to be procured in advance and left for longer periods of time, and that is a possibility. But, that is not something that's really constraining the work that we're doing now. And, since the first procurements for us aren't going to happen for many years, there's a lot of time to relook at that before we make serious commitments about what the assets will be required for initial operations.

And, so, we're not at a break point for decisions. We're at a point where we're looking at some proposed costs, and I think the analytical tools and the assumptions made to date are probably okay for where we are in the planning process. Certainly on the repository side, for unloading, the fact that they were focusing on TADs as opposed to bare fuel makes the probability of a one week turnaround much more supportable, because all that's required is to pull a sealed can out of the transport overpack to release the transport cask as opposed to pulling a whole series of individual fuel assemblies, which would have been a much longer process for turnaround.

So, for where we are right now, I think we're okay. There is certainly going to be the need for some more significant refinement as we get much closer to actually making our procurements.

ABKOWITZ: Okay. My final question has to do with work that's been done recently on the throughput analysis. And, this gets specifically to the surface facility design and the preclosure safety analysis, both of which are important components to the License Application.

LANTHRUM: Absolutely.

ABKOWITZ: In my reading of that work, it appears that each facility was analyzed from a throughput perspective independent of the interactions between them, and the interactions with transportation. And, I recall reading in there under each facility, that all inputs, such as loaded transportation casks and empty waste packages, are available on demand, and all outputs, such as empty casks and loaded waste packages, are removed immediately. Were you consulted on those assumptions?

LANTHRUM: We were. There was a fairly short discussion about that at the time, though. There is a lot of refinement that's going to have to be done, and we expect most of that refinement to happen internally through what we had was an interface control document, an IICD, it's an integrated interface control document, and we have an internal portion of that and we have an external portion that resolves interface issues between OCRWM and other programs within DOE. There are a lot of things going to have to be dealt with in terms of operational space.

We've had a lot of discussions about what the actual handoffs are going to be to make sure that there's no disconnects in either terms of capability or process to make sure that that flow can happen. But, the frequency of the flows and the timing for the flows is going to need some more significant evaluation as we get closer.

ABKOWITZ: Well, any logistics operation that I'm familiar with is not able to achieve that type of performance. And, the implications are that you either have less throughput or significant delays in your ability to process. And, in either case, what it basically leads to is the requirement to build additional facilities to replicate the same function in order to achieve the same throughput, or you have issues with regard to where is this waste going to be and for how long. And it seems to me that those are very important preclosure safety analysis questions, because they have to do with exposure, they have to do with number of handlings, and so forth, and that's why I'm raising this point.

LANTHRUM: In terms of the throughput at the repository, I can't speak eloquently about that. I'm not all that informed about the design or process of the equipment and the processes through the facilities.

One of the things that I was consulted on was, as part of the TSM modeling, was looking at the number of casks that we would need. And, with the assumptions that were provided, where the repository itself provided some feedback and some input, whether if those assumptions were okay, what would the number of casks that we would need to procure need to be. Now, if those assumptions wind up having to change because of actions at the repository, it wouldn't necessarily mean that they would have to build additional facilities. It may be that I just need to have additional assets in terms of bare fuel casks, or the overpacks for TADs, because if I don't get the TAD overpack back as quickly as I would like, if I have other overpacks, transport casks available, the transportation operations can continue by drawing from a larger inventory store than we might have had otherwise.

And, so, that asset base can be adjusted on the transportation end without being overly controlled by the throughput capability of any individual's facility. And, I don't know if you looked at the parallel possibility, since some of the waste coming in is going to be thermally in a condition not ready for disposal, I think there's going to be a discussion here in a little bit about thermal issues, but there is a receipt facility, and that facility is only geared towards taking the TAD out of the transport overpack, and putting it into an aging overpack. And, there is an expectation that there's going to be some fairly significant aging requirement for some of the fuel that's received, and certainly the turnaround times there are going to be less complicated than the turnaround times for a facility we're actually trying to load into a waste package.

ABKOWITZ: Okay, thank you.

GARRICK: Thure, David, and then Ron.

CERLING: Cerling, Board.

Could you go to Slide 15? So, my question has to do with your point on this was that a very small proportion of this is hazmat, and of that, a very small proportion was spent nuclear fuel. But, let's imagine what does the size of that .7 percent come to, and what percentage of that is spent nuclear fuel during a period when you're sort of anticipating the maximum amount of material being shipped?

LANTHRUM: There are shipments going on currently. It's not like the spent fuel world is completely silent. There's the Forum Research Reactor Fuel Program that is shipping currently, and there's the domestic research reactor fuel program for universities that have operating reactors, and the Navy makes a number of shipments a year currently. None of those are going to Yucca Mountain. But, those shipments are moving around for consolidation from universities and from ports to DOE storage facilities.

The addition of the shipments that we're looking at, we're talking about I believe it's 130 rail shipments a year was the last analysis that we had done, looking at a total of 175 shipments, including both rail and truck per year. 175 shipments, again, is background noise for the overall volume that we're talking about here. The volume of shipments under Class 7, which is the radioactive material shipments, is much, much larger than that.

And, so, the .7 percent of the overall suite of 125 billion ton miles is not going to change appreciably, even when we get to full capacity. It’s just a very small part of the overall flow of hazmat in this country. And, even when we’re at full operations, we still are not much more than a drop in the bucket.

GARRICK: David?

DUQUETTE: Duquette, Board.

A couple of naïve questions.

LANTHRUM: There are no naïve questions, I’m sure.

HORNBERGER: Well, you haven’t heard from David.

DUQUETTE: Not being an expert on rail transport, I guess the first question would be are these containers outside of the normal range of what railroads are currently hauling on individual cars? And, the second part of that question is are the national rail beds in good enough shape to carry loads of this magnitude?

LANTHRUM: The weight range that we’re looking at is fully within what is done industrially on a regular basis. Now, spent fuel is not done at this size on a regular basis, but industrial loads in the 280 ton range are not uncommon at all. There’s a lot of those that go on. So, for the Class 1 railroads using--this is really confusing--Class 1 railroads are the major railroads, but the best track is Class 5. Class 1 is the worst track, but Class 1 is the best railroad. And, it's just part of the busy, dizzy world. But, the Class 1 railroads, the major lines that run east/west using Class 4 or Class 5 track, not a problem at all.

As Dr. Abkowitz pointed out, some of the short lines that feed remote utilities may be more of a concern, and that's why that's part of the inspection process that we want to engage with the Federal Railroad Administration on well in advance of our shipments, so that the railroads themselves have an opportunity to upgrade their capability to deal with the shipments that we have.

DUQUETTE: Well, that's good news for a change.

LANTHRUM: Yes.

DUQUETTE: The second question is this statistic that you are quoting here, and the National Academy study is based on the total amount of material that's being shipped.

LANTHRUM: That's correct.

DUQUETTE: And, the statistics on the accident rate, or whatever the case might be, are based also on the total. Have there been statistics done on individual shipments relative to the risk for individual shipments?

LANTHRUM: I can't answer that. And, I'm not sure how they would go about doing individual shipments in terms of what the risks are for a particular shipment. All I know is statistically, looking at the volume that's moved, and looking at what the number of incidents are, you can mine that data for some conclusions. But, how you would deal with individual shipments, I'm not aware of any studies that focus specifically on that approach, nor what the validity of that would be.

DUQUETTE: Well, I think the full size test that you're talking about with an actual collision between cars may answer that question.

LANTHRUM: It may.

DUQUETTE: Because that kind of data would be on what happens if I do have an individual shipment at any given time during a 50 year period having an accident.

LANTHRUM: Right. Well, it's interesting, there were a range of things that the NRC looked at for their package performance study before they settled on this severe, but potentially real accident scenario, where you have an engine actually impacting a cask. They looked at a range of what would possibly have been extra-regulatory tests, where they would drop a cask--currently under 10 CFR 71, to certify a cask, you drop it 30 feet onto an unyielding surface, so that all of the energy of the collision is absorbed by the cask and none by what it's colliding with. Whereas, this test of a railroad engine--locomotive engines are pretty heavy, dense massive things, and yet when they slam into a cask, even going 80 miles an hour, a lot of that energy is absorbed by the engine. You're not going to be using that engine again. It's going to be trashed. And, the cask, even though it absorbed some of the energy, a lot of it goes into the kinetic energy of tossing that cask through the air a short distance, a lot of it is absorbed by the ground where the cask hits, and, so, the energy of the collision is spread over a lot of different ways.

In fact, it may be that slamming a locomotive into this cask at 80 miles an hour may be less severe than dropping it 30 feet onto an unyielding surface. I suspect that's the case in terms of g-forces.

DUQUETTE: You understand my concern. There's very little possibility in the near future, I hope, of a nuclear explosion. If one does occur, the consequences are pretty severe.

LANTHRUM: Well, understand that what's being shipped in here really is not explodable.

DUQUETTE: No, I didn't mean to put it in terms of explosion.

LANTHRUM: Okay.

DUQUETTE: I'm just saying that an individual incident could have major consequences, even if the statistics of it happening are very low.

LANTHRUM: Are very low, I understand. The good thing is looking at the study that was done by the National Academy, they said that the consequences for severe accidents are even lower than the consequences for minor. And, the consequences for minor accidents truly are not driven by what the content is. It’s driven by the fact that you’re transporting something in a heavy container, and accidents can kill people.

And, so, I was looking at those very low probability, small consequence incidents are just normal transportation accidents, where somebody gets stuck on a railroad track and a train hits them. And, that would count against the shipments, and yet what the shipment was really has nothing to do with it.

DUQUETTE: My last comment is a comment. I visited the French facility as well, and it’s quite obvious that it can be transported quite safely.

LANTHRUM: Absolutely, and they’ve been doing it. The interesting thing is that these shipments have been going on for over 40 years, both here and abroad. And, in France, they have shipped more waste, in fact, in the world, more waste has been shipped, than is allowed by the statutory limit to go to Yucca Mountain. And, that volume of shipment experience has happened without any impact, no releases to the public or the environment that’s been harmful. It’s a remarkable safety record. It’s something that we all should be taking great, great pride in. We've designed a system that is very, very robust and very safe. It's not OCRWM, it's the way that system has been designed by the regulators, by the IAEA internationally, and by NRC in this country. It's a very, very safe system.

GARRICK: Ron, Howard, and Ali.

LATANISION: Latanision, Board.

Gary, could we go to your Slide 5? I almost called you Greg again.

LANTHRUM: My first name is Joseph.

LATANISION: It is?

LANTHRUM: And, my hotel reservations and car reservations and stuff are made by Joseph, and I always when I check in, I say, "Do you have a reservation for Gary Lanthrum," and they never do, and it always scares me. So, I get called lots of things.

LATANISION: Okay. I knew there was something peculiar about that. At any rate, this graph makes or adds emphasis to the point that we've heard raised a couple times this morning of the importance of a stable and dependable base of funding.

LANTHRUM: Absolutely.

LATANISION: If this Project is to go forward, that seems very clearly a necessity. And, so, the concept of using the Waste Fund, which is paid by the generating utilities, is an interesting concept.

My question is more a point of information. What is the rate, and this may be as much for Wade as for yourself, but what is the rate at which funds accumulate on an annual basis, first of all? And, at what rate are funds paid out, if any, at this stage. And, thirdly, what is the intended use of those funds when, you know, as this process unfolds?

LANTHRUM: Ward spoke to this a little bit in his presentation. The current receipts from the fund, from the rate payers that pay a surcharge on the utility bills by getting collected, generates about $750 million a year in new receipts.

LATANISION: So, how much is in the fund today?

LANTHRUM: I believe it’s a little over $20 billion, $20.6 billion right now is in the fund today.

LATANISION: Okay.

LANTHRUM: And, there really actually is a physical fund, and it’s invested in zero coupon bonds, and other government securities.

LATANISION: Cannot be used to reduce the federal deficit?

LANTHRUM: You know, that is not my area of expertise.

LATANISION: Okay, so let’s suppose there’s $20 billion in this fund, it accumulates at $750 million a year, how is it intended to be paid out?

LANTHRUM: Well, it’s paid out through appropriation right now, and as Ward indicated, the receipts from the fund are classified as mandatory receipts, and yet expenditures in this program are classified as discretionary expenditures, which means that we rely on appropriations to provide funding. And, that’s completely, right now, independent of what the receipts are. It’s whatever the budget process generates for us.

LATANISION: Well, then, finally, what did the drafters of this fund, when it was constructed, what was the intended use of the fund?

LANTHRUM: Well, obviously, the intended use of the fund was to build and operate a repository to dispose of spent nuclear fuel and high-level waste.

LATANISION: Design, build, and operate?

LANTHRUM: Absolutely, and it’s all covered by the Nuclear Waste Policy Act, all of the allowable activities are covered in there. In fact, the funding that I’m obligated to provide to states and tribes for emergency preparedness training, and for technical assistance, that’s provided for in the fund. That’s also part of the expected and obligated expenditures.

LATANISION: Okay, thank you.

GARRICK: Howard?

ARNOLD: Arnold, Board.

Joe, I'd be interested in just a brief status report on the TAD designs, how many vendors you've got, and how you're getting along, to schedule that.

LANTHRUM: We had numerous interactions with the vendor community, and Chris Kouts' organization headed that effort up. There was lots of engagement, lots of involvement, lots of give and take. There was a technical specification that was developed that was put out for comment. There was lots of engagement over that specification, and whether the way that the tech. specs were defined created problems, or if there were ways that we could groom the specifications to make them easier for the vendor community to meet. Those changes were made. A final spec was published, and then vendors were invited to submit proposals for how they would suggest to move forward.

I don't know, since this is in procurement space right now, I don't know how much we can say, but there were--can we talk about the number of vendors that applied?

SPROAT: No.

LANTHRUM: Okay. So, unfortunately, I can tell you that we had lots of interactions, and people were engaged, and we got people that proposed, and we're looking at those proposals. But, since it is in procurement space right now, I guess my lips are sealed.

ARNOLD: But, you haven't awarded any contracts?

LANTHRUM: No contracts have been awarded. Well, there was the original award of funds to provide the initial proposals, and we got lots of interest. We made the award so they could submit a proposal to us. Those proposals are being evaluated, and Chris will take it from there.

KOUTS: Let me help you.

LANTHRUM: Thank you very much.

KOUTS: We provided funding for vendors for proof of concept designs. We did not give them any funding to prepare proposals. That was done on their own nickel, if you will. But, the proof of concept process was done last year. We have those proof of concept reports. The procurement that's underway now is to take--would take the full design and operational demonstration, if you will, of TADs forward. And, we're hopeful that we'll be able to do that in the very near future. This is Chris Kouts, by the way.

ARNOLD: Thanks, Joe.

GARRICK: Ali?

MOSLEH: Mosleh, Board.

On the estimated construction time that you have for four to ten years, depending on the availability of funds, what's the basis of that time estimate?

LANTHRUM: Actually, in the rail alignment Environmental Impact Statement, there is a significant body of technical information about what it will take to build the railroad. And, on the alternative that we listed as the preferred alternative in the draft EIS, we know the amount of cut and fill that's going to be required. We know the number of bridges that are going to be required. And, so, we have a conceptual design that was necessary to be developed for the EIS.

For example, we have 50 million cubic yards of cut and fill. That's a football field a little more than five miles high of dirt that has to be moved around out there. There's seven mountain ranges that we're going to get to cross with this route. Nevada is basin and range country, with the ranges running north/south, and we're going to be crossing seven of those. And, so, there's a very strong basis for the estimate. It's based on engineering data, and it's on unit costs for that engineering requirement. We have a very strong basis right now.

MOSLEH: So, the data includes actual construction time that the highways and railroads--

LANTHRUM: Absolutely. And, in fact, we also had on the rail side, meeting with the vendor community. We invited, we put out a request for information, we invited the private community, private sector to come in and talk to us about their views about how we should design, construct, and operate the proposed railroad. And, we basically said this is our analysis, this is what we've done so far, here's the information that we've got, can you validate whether we're heading in the right direction? Is this achievable with the way that you all do business? And, do you have advice for us? Much as Chris did with the tech. specs on the TAD proposal. And, they all said, basically, that what we've outlined is achievable within the current standards for construction.

There are a number of things that would have to be done to compress that schedule to the shortest time, the four years, and that would be to do multiple sections in parallel, but that's fairly easy to do when you've got a 340 mile long railroad. You can run 12 construction areas in different geographic locations in parallel. And, if we're very tightly constrained on funding, there's going to be more series activity, and it will be less accomplished in a given year, and that drags the schedule out, and the total project cost. But, we've got a very strong basis for our current estimate range right now.

And, as unit costs change, you know, the cost of steel over the past ten years has way outstripped the rate of general inflation. Steel costs have been going through the roof, and we pay very close attention to that, and will before we go out for the final procurement of the final design and final construction.

MOSLEH: So, cost is an important factor?

LANTHRUM: Absolutely. Absolutely. And, we're doing lots of innovative things to try and control those costs, and that was part of the interactions with the vendor community, was what can we do to constrain the costs, and to make it easier to build? Are there things that we can do in terms of our approach that would make this easier? And, we got lots of good feedback on that. It was a very useful set of interactions.

MOSLEH: One more question.

LANTHRUM: Certainly.

MOSLEH: I thought I understood the figure that you showed on 14, on comparative risk.

LANTHRUM: Yes.

MOSLEH: This graph is actually on the same unit of consequence, estimated fatalities, short-term and long-term.

LANTHRUM: Yes.

MOSLEH: So, he is really--

LANTHRUM: Very comparative. But, the reason the consequences and the risks associated with spent fuel shipments are lower is because there's fewer of the shipments, and I think that was the point, because as you increase the number of shipments, will that raise the potential impact because you have more opportunity for incidence? And, it turns out that in the overall scheme of things, it still winds up being far less than anything else, because even at our maximum throughput rate, the number of shipments is still small.

We're talking about two to three trains a week that would be coming off the UP line at the connection point to the Nevada Rail line. There's 20 trains a day that run through there carrying other commodities, and that's a very busy heavily subscribed line that UP uses to get down into California. We're just a very--in fact, the fact that we're such a small shipper makes procurements very difficult. The railroads don't really want to deal with us. Our trains are speed limited. We are congested on their rail lines when we operate.

And it's difficult to get rail car vendors to talk to us because we're talking about 140 rail cars total. Coal companies come in and buy 5,000 cars at a time. How do we compete in a market where we're trying to place an order for 140 and other people are coming in in multiples of a thousand. It's difficult to get attention. We think we're big, and we think that the cost profile, this $4 ½ billion to develop a system is really significant, but it's a drop in the bucket. Transportation, there's a lot of it going on out there. We're really small potatoes.

MOSLEH: Even in this volume--

LANTHRUM: That's background noise. It doesn't change appreciably, it really doesn't.

MOSLEH: Thank you.

GARRICK: Andy, and then Henry.

KADAK: Yes, could you go to Slide Number 4, please? I'd like to ask you a little more detail on some of these boxes.

LANTHRUM: Certainly.

KADAK: For example, the rail car project, how much interaction have you had with private fuel storage people who have already done a lot of this work, and how much of that can you apply to this area?

LANTHRUM: We have not had interactions with private fuel storage, but we had lots of interactions with the car companies that built the cars for private fuel storage. And, there's only a limited number of rail car companies out there. We talked to Colorado Rail, we've talked to, there's been four or five of them, and again we had industry interactions, and it was the people that designed the car for PFS that we've interacted with.

And, there is a transportation technology center out in Pueblo, Colorado that is the big testing center for rail infrastructure, and used to be a DOT facility, now it's an independent contractor that runs the facility for FRA, and they've done a lot of modeling for us, looking at the performance of individual components, like the trucks, which is the suspension system for rail cars, whether or not existing high quality trucks could meet the performance requirements of this new AAR standard. And, the answer is yes, they can.

KADAK: I guess where I'm going is as I understand it, they've actually built a locomotive to these same specifications.

LANTHRUM: These specifications don't apply to locomotives.

KADAK: Well, somewhere there's going to have to be a locomotive; right?

LANTHRUM: Yeah, but any locomotive can work with it. There is one constraint. Part of the specification requires that the braking systems be electro-pneumatic instead of just pneumatic, and, so, you have to have an engine that will communicate with electro-pneumatic braking systems.

KADAK: Well, let me get to my point.

LANTHRUM: Okay.

KADAK: It seems like there's a lot of stuff that they've already done that you could apply directly without having to redesign everything. I'm just asking how much of that, how much have you explored the opportunities to save a few bucks to try to use what they've already got?

LANTHRUM: A lot. We've gone as far as we can. The only car that's been designed to the specs so far, and built as a prototype was a cask car. Nobody has designed a people carrying car, which our escort car will be, which also has to meet the spec. But, we are building on the base of work that's been done by the actual rail car vendors, and they are more than happy to share what they've learned in the process of working for PFS.

KADAK: In terms of the national transportation plan, have you now selected national routes that would take spent fuel from wherever it is to either Caliente or some other location?

LANTHRUM: What we did in the repository EIS, which is where the National Transportation System is analyzed, is we looked at representative routes to capture the impacts, because that's the purpose of an EIS. We have not gone beyond representative routes to looking at a suite of actual routes that we might use. Certainly, part of those routes will have been picked up by the representative routes that we have analyzed. But, as I indicated in the presentation, it will be mid 2009 before we have the suite of routes that we're proposing, at the earliest.

KADAK: Isn't part of your discussions with the regional and local officials that Mark alluded to, part of that process is to select these kinds of routes?

LANTHRUM: Absolutely.

KADAK: Why is that not on the agenda? It seems like that would be noted.

LANTHRUM: Well, one of the big challenges is if the first shipments aren’t going to be until 2017, selecting routes now, ten years before then, or nine years before, as Ward indicated, even 2017 is out, they’re going to change before we actually use them. There’s going to be a lot of changes of infrastructure.

The other thing, and I think at the last Board meeting, Dr. Abkowitz mentioned that there was a notice for proposed ruling, and it would affect the route selection process. And, there’s a draft rule out right now for comment from the Department of Homeland Security, and we’re expecting one from the FRA, from DOT fairly soon, and it may, in fact, shift the responsibility for route selection to the industry, to the railroads, as opposed to us.

Now, the railroads are also part of this discussion, so we’ve been working with UP, with Burlington Northern, with CSX, and the other major carriers, on this route selection process through this tech. conference that Dr. Abkowitz mentioned. And, so, the railroads are involved, the state and regional groups are involved, and what we’re focusing on right now is what are the criteria and methodology that we should use when we ultimately do select routes. But, selecting routes this far in advance of actual shipments is really premature.

KADAK: So, you’re not suggesting that we’re going to change the number of new rail lines that are built in this country, nor the location of existing nuclear power stations, are you?

LANTHRUM: No, but there are lots of upgrades to rail lines that are currently going on. The BN line that runs across the south of the country east/west, they are double tracking that, and changes to infrastructure like that might indicate where the railroads would be more comfortable with us going. And, those changes are going to affect where we would wind up.

Certainly, the railroads have already expressed, because we have had meetings with the Association of American Railroads about this very issue, and one of the routes that we were looking at in our representative routes goes through Nebraska, and they said please, please, please stay away from that route. And, the reason wasn’t because they’re concerned about our shipments. They don’t want to interrupt the coal shipments. They have coal trains going through there like crazy, coal trains running 80 miles an hour. Our trains are speed limited to 50 miles an hour. They don’t want us interrupting that flow, and there’s lots of things like that that will change, even if the track itself doesn’t, as the mix of flow and other commodity moves. And, we have to be cognizant of all of that.

We're a small player, and we have to be in that big pond, and making sure that all of the stakeholders are, all their concerns, all their issues are addressed as we select routes. And, there are going to be a lot of changes between now and the time that we actually do shipments.

KADAK: Okay.

LANTHRUM: Now, what we are prepared to do is to make sure that we have identified the routes more than five years in advance, so we can work on our obligations to the emergency preparedness and responders out there, so that there will be lots of advance, but it's not going to be so far in advance that what we do is going to be moot before we actually ship.

KADAK: In terms of the delay in the Nevada Rail, and I'm trying to figure out what you will do if, say, the repository opens in 2018, '19, however many years delay it will be, and the rail line is not completed, and you're preparing to ship, you can at least ship existing DPCs, even if the TADs aren't exactly ready yet, but if it comes to, say, Caliente, are you looking at alternative heavy haul truck routes to the site that may be from a different location than Caliente?

LANTHRUM: We really aren't. We have looked at the road conditions out there. There would be significant upgrades to existing infrastructure to be able to do any heavy haul in rural Nevada. The state has come out in vehement oppositions to any potential shipments through Las Vegas, and, so, we were looking at rural Nevada. The road infrastructure would have to have major investments. And, realistically, we wouldn't operate a repository absent a canister receipt and handling facility, for example.

And, so, what the Program is looking at is how do you build an operable system, it's not how do you build a repository and think separately, how do you build a transportation system, it's how do you build an operating system, an integrated operating system. Rail is part of that integrated system, and, so, you really, with the current design approach to focus on TADs, you really need the integrated system, which includes rail. And, if it requires a re-allocation of resources so that the repository is delayed another year or so so that the rail line can be completed to make the repository work, the system will be what we focus on as we move the Program forward.

KADAK: So, the rail line is, in fact, a critical path item for receipt of spent fuel?

LANTHRUM: For receipt of the throughput that we want to achieve. There will be some small amount of legal weight truck shipments, but that won't achieve our throughput goals.

KADAK: Last question. A lot of discussion now on interim spent fuel storage facilities, regional and otherwise. Are you involved in any of those discussions, and will those discussions, if there is Congressional support, speed up your acquisition of assets?

LANTHRUM: We're still trying to figure out what our path forward on that is. And, Ward mentioned the fact that there's a requirement in the appropriations bill that was passed on December 26th, but there was no funding provided for that, and how we address that planning activity or over what time frame it's expected to unfold, we haven't waded through that. Transportation will certainly be part of that discussion, and I've got lots of interests and lots of concerns about accelerated timing, how that would be accommodated. And, again, it's all money. If you want to implement a transportation system sooner to some place other than the repository, it requires investment sooner.

GARRICK: Okay, Henry?

PATROSKI: Perhaps I didn't hear you correctly, but I thought you said that if the rail construction project were delayed, that construction costs would not change, but management costs would.

LANTHRUM: They won't change as much. But, the real impact, I believe, although there are going to be some time value of money costs that will be involved, but the big impact is if you're carrying significant G&A costs, overhead costs, indirect costs, that you drag those over a longer period of time. Certainly, the actual construction costs are going to change with annual rates, with unit rates.

PETROSKI: Right.

LANTHRUM: Unit rates are likely to change, but I would expect the bigger impact for a year delay, for example, to be driven more by the indirect costs that would be carried for an additional year, rather than the unit cost changes. But, unit cost changes are part of the equation, though.

PETROSKI: Okay, thank you.

GARRICK: Okay, any other questions? Any questions from the Staff?

(No response.)

GARRICK: Well, we’re pretty close to our schedule. We thank you very much for your presentation. And, we’ll take a break until 10:30.

(Whereupon, a brief recess was taken.)

GARRICK: Let’s get underway. Our next presentation will be handled by Ernest, Ernie Hardin, and he’s going to talk to us about thermal strategy analysis, which is one of the three or four topics that we have been emphasizing a great deal over the past twelve months. Ernie?

HARDIN: Thank you. I’m one of Sandia’s technical leads on the Yucca Mountain Project. My co-authors are shown here. I have the privilege of presenting work to you today that was developed by several teams at Sandia and BSC over the past three or four years.

Next slide, please? This is an outline of my talk. We talk about the objectives for a thermal management strategy, we'll give a very high-level discussion of the characteristics of the waste forms that we talked about, and then by way of background, I'm going to introduce you, or re-introduce you to the postclosure thermal reference case that we use for TSPA. And, then, from that embarkation point, we'll talk about the estimated limiting waste stream, which is a new entity that we developed for the purpose of demonstrating the feasibility of thermal management for the repository. And, then, we'll go to how that waste stream is going to be emplaced, or how it would be emplaced underground.

With that, we start with the discussion of preclosure and postclosure thermal constraints. We identify the mid-pillar temperature as the constraining limit, talk about how we control that and the methodology for determining the emplacement of the waste packages underground. And, then, we'll show one of several simulated emplacement sequences. We'll identify the hottest segment in that for analysis, we'll give some attention to uncertainty and variability in the host rock properties, and how that affects the strategy.

And, we'll talk about hydrogeologic, geomechanical and geochemical responses. This is a part of the analysis that's required by 10 CFR 63. And, then, finally, some comments on the implementation of the strategy in the repository operation.

Now, this topic really has three parts, in my view. There's the operations part, which gets to the surface facility operations at Yucca Mountain, and also the choices in operations that will be conducted at the commercial plant sites. Then, there's the preclosure thermal part, and the postclosure thermal part. My presentation will be emphasizing the postclosure thermal part. We give some attention to the others, and I have my co-authors here available to take your questions on the other parts if you have them.

So, the next slide? The first objective is to maximize the operational flexibility for emplacement of waste underground. This study documents the feasibility of a thermal management approach. In undertaking this study, we had sought to limit the impact to the existing repository design, and we've also undertaken to determine the applicability of the current TSPA models to the anticipated range of thermal loading.

Another objective, an important one, is to ensure that the strategy is consistent with the waste acceptance provisions of the standard contract. And, finally, this approach will establish the transportation thermal limit for commercial fuel from the plants to the repository, among the key criteria for loading of the TAD canisters.

Next slide, please? Okay, this is that high-level summary of characteristics. We’re going to take the waste forms represented in this list, put them in these canisters, and accommodate the variability in the thermal output of the waste forms in this strategy.

So, we’re talking about TADs, of which there will be approximately 6,600. Dual-purpose canisters are included in this analysis, approximately 280 of those. In addition, there will be approximately 14,000 bare fuel assemblies, which will be delivered in truck casks, according to the assumptions that we have used, and they will be packaged in TADs at the repository. And, then, we have the canisterized non-commercial waste forms, high-level waste, defense spent nuclear fuel, and the Naval spent fuel.

Now, these are going to go into approximately 7,400 TADs and 3,700 co-disposal and Naval spent fuel packages. And, of course, the commercial spent fuel accounts for the vast majority of the heat output of the waste stream that is represented by the waste that’s going to be received at Yucca Mountain, and the variability of that thermal output is controlled by initial enrichment, fission burnup, and age out-of-reactor. So, these are some of the key variables of this analysis.

I want to point out that the estimates, the counts on this slide, I give a source here, are estimates, that the counts are subject to uncertainties, for example, assumptions that you might make about how you load co-disposal packages, and whether there are underloads involved in the overall sequence. So, I just wanted to let you know that there’s at least a plus or minus 10 percent uncertainty on these counts.

Next slide, please? Okay, by way of background, the thermal reference case that we use for TSPA is based on a 1999 DOE study of the commercial fuel that would be available for receipt at the repository between 2010 and 2033. The average age out of reactor of that waste stream is 26 years. The average burnup 38 gigawatt days per metric ton uranium. That is the basis for the thermal decay curves that we are currently using in TSPA. We’ll talk about those curves more in this presentation.

Now, that thermal decay curve represents the behavior of a small population of waste packages that we identify as the unit cell that represents all the commercial fuel and co-disposal waste packages that will be emplaced at the mountain.

The maximum waste package power output at emplacement was 11.8 kilowatts. You may remember that number. We bumped that up to 12.6 for the current TSPA to accommodate the TAD concept because the waste package is a little longer. In so doing, we preserved the 1.45 kilowatt per meter line load at emplacement. This is the framework of the TSPA, and how we represent thermal aspects of the waste stream.

The TSPA also assumes that the waste will be emplaced in 2067. This is the so-called instantaneous emplacement assumption. It’s a simplification. It works for TSPA. Followed by 50 years of preclosure ventilation and closure in 2117. So, this postclosure thermal reference case then is used as a boundary condition in two dimensional and three dimensional coupled-process simulations. These are models, such as the TH model and the THC model, with which you may be familiar.

For example, the multi-scale model is a direct feed to TSPA, and it uses the unit cell. It represents eight distinct waste packages, six TADs and two co-disposal types.

Next slide, please? So, why the estimated limiting waste stream? Well, we have a 2002 report that updates the waste stream. It addresses a range of uncertainty on commercial spent fuel age, burnup, and initial enrichment. It also addresses the likelihood of life extensions for the commercial reactor fleet.

In addition, we’ve got the TAD canister operating concept which was added to the mix. It comes with a different schedule, and it involves limited bare fuel handling capability at the repository. For example, there would be no pool there storing large numbers of assemblies for the purpose of picking assemblies for thermal blending to control the thermal output of each individual waste package.

So, the estimated limiting waste stream is a collection of commercial and co-disposal packages. It’s based on what is likely to be received at Yucca Mountain over a period of approximately 25 years, starting in 2017. It is based on the output of the Total System Model, and the Total System Model then is used to represent the various decisions and uncertainties associated with spent fuel selection and transport, receipt, also capabilities of the facilities, and so forth. And, the resulting commercial, plus non-commercial waste forms, packaged in TADs and co-disposal packages, is defined as our ELWS.

A note about the Total System Model. It is an example, it is not the solution, it doesn’t represent a specific projection of what’s going to be done. For us, it represented a representative, a set, of waste packages and waste forms that are going to be received at Yucca Mountain. So, we take some of the output files, we inspect those and qualify them for use in our study.

Next slide, please?

KADAK: Ernie, could I just interrupt for a second?

HARDIN: Yes.

KADAK: Are you saying no blending? You just said no blending?

HARDIN: Limited.

KADAK: Limited blending. And, the TAD heat load is, for loading, limited to 12.6?

HARDIN: I'm saying that the postclosure reference case is fairly narrowly defined, and the maximum package power that's emplaced underground will be 12.6 kilowatts, followed by 50 years of ventilation.

KADAK: So, that's the TSPA assumption for your thermal management strategy?

HARDIN: That's a background. That gives you an idea of what TSPA is doing now, based on what TSPA did for the last assessment.

KADAK: Okay.

HARDIN: Okay? So, the approach that we've used based on the TSM then covers some of the key uncertainties in the waste stream, the selection of commercial fuel at the plants, it's based on projected CSNF inventories, and the current cask handling capabilities at the plants. This would include transportation infrastructure, and so forth.

We make a reasonably conservative assumption for this study, that the youngest fuel will be selected first, so this is reasonably conservative from the point of view of the higher heat output of that fuel that we will have to then manage when it arrives at the repository. And, the minimum five year age out of reactor for commercial fuel as well. These are some of the key assumptions. You'll see those repeated in subsequent slides.

As far as the transportation system, the TSM result that we use for this analysis puts 90 percent of the commercial fuel in TADs before they arrive at Yucca Mountain. 10 percent would arrive uncanistered in truck casks or DPCs. As we discussed earlier this morning, most of the transportation would have to be by rail, and that the maximum thermal output for TADs shipped to Yucca Mountain would be 22 kilowatts, and this is a value that's reasonably high, and consistent with the capabilities of current transportation cask designs.

So, the ELWS then is a collection of spent fuel and co-disposal waste packages. It's based on waste received at Yucca Mountain from 2017 to approximately 2040. There would be some additional shipments beyond 2040, perhaps mostly high-level waste. And, the overall lineal thermal output, if you take those packages and line them up end to end and divide the power output by the length, the average line load is similar to the TSPA reference case.

Next slide, please? This slide shows that. The red curve is the average lineal power output of the ELWS waste stream. It comprises waste that is received at Yucca Mountain starting in 2017 and ending in the 2040 range. After all the waste is received, you see this red curve then begins its logarithmic radioactive decay.

The blue curve is the postclosure thermal reference case that we use. It starts at 2067. I have shifted that curve 17 years to the left here in green just to afford closer comparison with the ELWS. So, the bottom line here is that the waste stream that we expect to receive at Yucca Mountain is similar to and in some respects cooler than the postclosure thermal reference case.

Next slide, please? Okay, onto the postclosure thermal limits. The thermal management strategy needs to honor these limits. The first is the mid-pillar temperature limit of 96 degrees. 96, of course, is the boiling point of water at this elevation. This criterion will promote drainage through the rock pillars between drifts. This has been part of our design basis for the repository for some years.

The drift wall temperature limit of 200 C controls changes in the rock that control the rock’s strength, so, strength in response to seismic and thermal mechanical loading. The drift scale test, for example, was operated to achieve a constant drift wall temperature of 200 C for a few years. So, we have some experience at this level, and this limit will maintain the result within that experience range.

The waste package outer barrier temperature limit of 300 C to be maintained for 500 years, up to 500 years, is set to preclude metallurgical changes, phase separation in Alloy 22. And, the commercial spent fuel cladding temperature limit of 350 C limits degradation to the cladding by pre-rupture process.

I should note that we’re doing this to preserve the capability of cladding as a barrier, even though we don’t take credit for cladding integrity specifically in the TSPA.

So, for all of these, the mid-pillar temperature is the controlling limit, that is, if we meet it, we meet the others. We’ll talk a little bit about how we accomplish that.

Next slide? Okay, what we need is we need an index for each waste package that tells us whether we can load it in a certain location at a certain time, and meet all those limits. And, since the mid-pillar limit is controlling, we choose the mid-pillar as the reference location for a temperature calculation that we call the waste package thermal energy density mid-pillar index.

So, we calculate this number for each waste package, and then we evaluate, as we load each package, we evaluate it against its neighbors, and if the average index is less than some target value for the mid-pillar temperature, then we can put that package in that location.

The index itself is based on a conduction-only superposition solution. One of the insights that we applied here is that we’re using calendar year rather than age out of reactor. It simplifies the concept somewhat. So, the definition of the index here is the peak mid-pillar temperature locally, if the entire repository is loaded with identical packages. And, because of the linearity of the equation, we can sum and average those indices for different waste packages, and get an estimate of the local mid-pillar temperature.

If, of course, requires a parameter for the rock thermal conductivity. This is one of the dominant parameters of the calculation. We use a representative value here. And, in the application of this index, we’re neglecting aging duration, that is, the heat that would be dissipated for some years that a particular waste package may stay on the surface prior to its emplacement underground. The index is calculated from the time of receipt at Yucca Mountain.

And, also, the timing of the peak, because the shape of the thermal decay curve may vary from one waste package to the next, the actual local peak mid-pillar temperature may arrive at a different point in time. It might come at anywhere between let’s say 400 years after closure and a thousand years after closure, but the temperature history at the mid-pillar, because of the geometry and diffusion physics, is very flat. So, we can get away with averaging together the contributions from adjacent waste packages for use of this as an index.

Next slide, please? This histogram shows the value of the mid-pillar temperature index for all of the values for 10,400 waste packages, is the number that we came up with in this particular analysis. That is the number sufficient to handle the legal maximums for the inventory of waste to be disposed of at Yucca Mountain.

You see that the index comes in at greater than 96 degrees for many commercial fuel packages, but because we have these cooler ones, these are mostly high-level waste packages, it's possible to accomplish the blending by your selection of packages as they are emplaced in a sequence underground, and meet the limit.

This figure over here shows--this gets at the flatness of the temperature histories that I was just talking about, and it basically shows the mid-pillar temperature index against calendar year for all the 10,400 waste packages. And, we see that for those packages, they potentially have an index above 96. Of course, that's in the wrong place. But, the point is that the peak temperature for those packages lies in a time window of about 300 years. That's a fairly narrow time window, and the behaviors of all the packages are fairly flat in that window. So, this is why the index concept works.

Next slide, please? So, now we have the waste stream, which is a collection of packages, and we have to address how those are to be emplaced underground while honoring the temperature limits.

Now, I've talked about the postclosure temperature limits. There are also preclosure ones. We require that the maximum power output of any package at emplacement is 18 kilowatts. So, this is compared with the 22 kilowatts, which is the maximum for transportation to Yucca Mountain. So, clearly, there is a bit of aging that might be required for the hottest packages. The purpose of that limit is that in the event of an interruption of preclosure ventilation that lasts up to 30 days, that limit will ensure that the commercial spent fuel cladding does not exceed 350 C.

Similarly, we have a limit that the local average line load as we emplace these packages, any seven adjacent packages should not have a local line load that exceeds 2.0 kilowatts per meter at emplacement, and this serves the purpose of limiting the drift wall temperature to 200 degrees in the event of the same hypothetical interruption in ventilation.

So, the ELWS is based on 22 kilowatts maximum thermal output, youngest fuel first, five year minimum age.

We can honor these. Another detail of the emplacement sequence development is that in concept what's done here is that the commercial spent fuel is received at Yucca Mountain for a period of time, and all of it goes out to surface aging until such time, two years or four years, inventory is accumulated, and then we begin to choose from that inventory waste packages that optimize the mid-pillar index criteria. So, if we choose a mid-pillar level of 96 degrees, we choose a package that, along with the six previous packages, gets us as close as possible to that target. So, that's basically the approach.

With the problem set up the way I've described it, the coolest mid-pillar temperature that we can get to is 85 C. That's an interesting fact. It makes more sense to load to 96 degrees C, and I'll talk a little bit more about that. 96, of course, gives you a little bit hotter mid-pillar temperature, a little bit more efficient use of real estate.

The sequence analysis that was done used a post-processing software tool built on the TSM. And, I suppose that's about all we'll say about it.

Next slide, please? This is an example of the results from the emplacement sequence part of this analysis. And, what you see here is for the YFF5, 96 degree target, two year surface aging inventory, that the packages go in, and as they go in, they are selected such that the local seven package average of this mid-pillar index is between roughly 20 and 96 degrees.

So, we hit 96 as a target in the optimization frequently. Occasionally, there will be a situation where there is no package cool enough or hot enough available to hit that target, so we'll come in lower.

At the end of the sequence, you see that we have HLW packages left over, and that's what this group is. At the beginning of the sequence, we emplaced most of the Naval spent fuel, and it is cooler. That's what this group is.

So, this is basically a projection based on some reasonably conservative assumptions covering everything from selection of the commercial fuel, to transportation, throughput considerations, aging, capacity, and thermal rules for loading the waste packages underground, for emplacing them underground. This is a projection of the thermal output of the sequence as it's emplaced.

Next slide, please? We, the Project, have recently re-analyzed that result using a different software tool called WPLOAD. It has a few more bells and whistles. The thermal modeling is done with a layered stratigraphy. It's a conduction only simulation. There are some other bells and whistles that I won't get into. And, using that, we're able to corroborate the results I have shown you this morning.

We also find it useful with WPLOAD to employ a margin analysis, where we have learned that if you include hydrology in the thermal calculation, you can actually load hotter waste in there and still meet the 96 degree criteria.

Next slide, please? Now, one thing that we have done here that's important is to corroborate the statement that mid-pillar limit assures that you meet the other limits. We've done a search in the emplaced sequence for the ELWS for the hottest locality, and we did that using a drift wall energy density index, that I won't get into here, it's the same concept. It finds the hottest local conditions.

We selected the 13 package segment, where those hottest conditions exist. They actually turn out to be right at 2.0 kilowatts per meter, which is one of our constraints. This is a portion of the waste stream that's received late in the loading period for the repository. So, this is young fuel received late, and ventilated for less time, in this case, 72 years. And, the bottom line here is that we calculated a peak drift wall temperature of 160 C.

This gives us a margin against 200 C. That margin is important for us. It allows us to state that the methodology is robust with respect to variability in thermal properties of the rock, for example.

Next slide? We have four host rock units identified. For each unit, there is an uncertainty distribution on the host rock thermal conductivity. Thermal

K is the key parameter here for determining the mid-pillar temperature. We did a statistical probabilistic Monte Carlo type analysis of the mid-pillar temperature index for all 10,400 packages, based on which unit they might exist in. You can see that the CDFs for the index are very similar for the different units. So, the message here is that no matter which unit we happen to be placing waste packages in, we can hit the mid-pillar temperature limit using the loading rules that I described.

And, over here, we have something that's a little bit different. Here, what we've done is we've gone after the mid-pillar temperature prediction using a thermal hydrology model. We're interested in the effects of hydrology on this calculation. This is a model that has layered stratigraphy, and using a reasonable choice of properties and hydrologic boundary conditions, we first turn off the hydrology and run the mid-pillar calculation. We get a peak out here of about 110 degrees.

If we then turn on the percolation flux, we see that if there's a lot of flux, it quenches the thermal response. But, even if there's a little flux, it's quenched at 96 degrees C. So, the inference here is that if there's any water about, that the tendency is for that water to maintain the mid-pillar temperature at 96, and to maintain it halfway available for drainage.

So, the bottom line here is that we get approximately 20 degrees C of margin from hydrology that we’re able to apply in addition to the mid-pillar temperature calculations that I’ve shown you today.

Next slide, please? Now, we switch gears a little bit, talk about geomechanical, hydrogeologic and geochemical responses to the range of thermal loading. I’m going to focus on the upper limit of the range.

So, going back to that emplaced sequence, we identify the hottest segment. We pulled from that a line load. So, here’s the local thermal loading condition. Its peak is at 2.0 kilowatts per meter. We have 72 years of ventilation.

Analyzing the mechanical response in 2-D, using a distinct element approach, a UDEC code, using typical rock properties, we find that the stability of the drift openings to collapse due to thermal mechanical over stress and exceeding the strength of the rock, is about the same as it is for the reference case.

So, at 95 years after emplacement when the peak drift wall temperature hits, we see that the amount of rock fall here is limited, and is very comparable to what you will find in this source for the reference case. And, the assessment was repeated at a thousand years after emplacement.

So, moving along, we did a similar analysis for the hydrogeologic response, again, required by 10 CFR 63. Here, the analysis is done in 3-D. We're interested in package to package variability, so we pulled the hottest segment out of the emplacement sequence for the ELWS. We look at packages individually. The red curves then are the range of temperatures against time for the individual packages in that segment. And, the blue envelope is that which we use in TSPA for the reference case.

So, the take-home message here is that the peak temperatures are about the same, and this may in fact be due to the hydrologic effect that we just talked about, and that with the anticipated range of thermal loading, we may have a little slower cool-down view that's not in the Y direction, but in the X direction, a little slower cool-down by as much as a couple hundred years during the repository thermal evolution. So, the drift wall temperature and waste package temperature exhibit the same types of behaviors.

The reason we did these simulations in 3-D was we were looking for an effect where the hottest package somehow through evaporation and condensation would mobilize water to cooler packages. So, we were looking for an effect where cooler packages would get hosed, and we did not find that effect in these simulations. Instead, we found that the local resaturation behavior, be it a cold package or a hot one, is really temperature controlled. That is, the local rock begins to resaturate with water when the temperature cools down low enough for that to happen.

Next slide, please? And, if you go to the far field, here, the base of the overlying Paintbrush tuff non-welded or the top of the underlying Calico Hills non-welded units, these are units that have particular significance in the performance assessment in regard to unsaturated zone flow and transport, for example, we find that the effect of the ELWS anticipated range of thermal loading, compared to the reference case, is pretty small. You know, the peak temperatures hit much later in time, and they are on the order of 2 degrees C different from the reference case, which is a small difference, not significant.

Next slide, please? These two slides address the analysis that we've done with the geochemical model. We used the near-field chemistry model, which is a model that we have developed in the last year for use in TSPA. Now, this model includes thermal aspects, so it has finite heat sources representing every single drift in the repository. We chose a drift near the center, where conditions would presumably be hotter. We plugged in representative properties, and used our hottest segment as the thermal load boundary condition in the simulation.

And, what the simulation does is for certain key drifts at certain locations within those drifts, it calculates the water-rock evolution, and the evolution of water that could potentially be seepage into the repository.

So, what you have here are drift wall temperature histories. The blue curves are for the base case. The red and pink curves are for the modified thermal case. This is a conduction only solution, which is why the peak temperatures are a little different, even though they’re not different in the thermal hydrology simulation I just showed you.

Moving on to the next slide, this sort of captures the chemical effects on the potential seepage water composition as a result of heating a little longer, and potentially a little hotter out there in the rock.

You were briefed on this model back in May of last year. It integrates the temperature/time exposure of water to rock, and, thereby, controls the kinetic dissolution of Feldspar in the rock. It actually integrates that over the full period of thermal evolution of the repository host rock.

The results, we find that comparing the two locations selected for the nominal, that’s the base case, and the modified thermal case, you have a very similar response in pH. This is within the uncertainty of the model. The changes in pH are a small fraction of the pH unit.

Moving down here to sodium, because Feldspar dissolves, you get an increase of sodium and potassium in the water, and because of interaction with CO_2, this causes the calcium and magnesium in the water to want to precipitate out as carbonates. So, calcium decreases, and the sodium plot here increases. This is not plotted starting at zero, so this is just a fractional change in the concentration of sodium in the water. So, the bottom line here is we're seeing slight shifts in the chemistry of potential seepage.

Next slide, please? So, how will this be implemented in repository operations? First off, we're going to analyze the thermal conditions for each drift prior to emplacement, as the characteristics of the received waste are known, which of course will be prior to receipt.

This would take the form of an analysis that Jack tells me is similar to a reactor reload analysis. The analysis will use the loading rules that we've described here, and it will verify, and it will verify that the postclosure temperature limits will be met.

Now, one thing that may be added to the analysis is any other additional conditions, such as package to package, end to end spacing that may be used to control the local thermal line load to meet the conditions that we set forth. There are optional control measures available to us in the emplacement of these waste packages underground.

Next slide? So, finally, to summarize this talk, the ELWS has been developed as the estimated limiting waste stream. It’s limiting because it uses fairly conservative assumptions about the type of waste that will be, particularly commercial spent fuel waste that will be received at Yucca Mountain. It will be young, and it will have heat output of up to 22 kilowatts per TAD unit.

We find that the average power output of the overall waste stream accommodating the legal maximums for emplacement of different types of waste at Yucca Mountain is very similar to our postclosure reference case. We have identified loading rules that will ensure that the preclosure and postclosure temperature limits will be met.

We have looked at near field responses to the range of thermal loading, represented by the ELWS. We find that the in-drift peak temperatures are similar. We get a little slower cool-down by hundreds of years. We looked at geomechanical and geochemical responses and found that they are small or insignificant.

And, again, this takes the form of something like a reactor reload analysis, and we’re going to evaluate every drift when the characteristics of the waste to be emplaced there are known.

GARRICK: Thank you, Ernie. I’m going to ask our technical lead on thermal management to lead off with the questions.

KADAK: Well, first of all, I’d like to congratulate you on looking more creatively at how to load the repository optimally, or close to optimally, and having some flexibility in the design.

As you may know, the Board is doing some thermal analyses of our own to try to better understand the sensitivities of what you might receive, and also how those things could be loaded in the repository.

And, just a quick comparison to what you have shown versus what we looked at, it appears that you're a little optimistic on the amount of cool-down advantage you'll get from surface storage, and the need to ventilate for a longer period of time, based on the actual thermal loadings that you're talking about to meet your 96 degree mid-pillar temperatures. Our hope is that shortly, we will have some kind of a report issued to have a meaningful discussion on this with you.

But, let me just question a couple of assumptions. First of all, the 90 percent TAD receipt assumption. At our last meeting, we heard representatives from the industry say that I think the recollection was something like 40 percent by the time you're ready to accept will be in dry cask storage facilities, and I don't see how you convert those numbers into TADs for this analysis in terms of your thermal limits.

HARDIN: Well, I'm going to ask for a little backup on that question. But, I'm going to first say that the, as you know, there's probably twice as much commercial spent fuel waste out there, projected to be out there, in the 2040 time frame, as will ever be received at Yucca Mountain, and, so--under the 70,000 ton total limit--and, so, by us using the youngest fuel first assumption, which is conservative from the thermal point of view, aren't we assuring that we're going to get fuel before it goes out to surface storage at the plants?

KADAK: I'm not sure that's necessarily a good assumption. But, clearly, there appears to be a disjoint between what utilities are putting into their dry cask storage systems at present, not only in terms of heat load, but also in burnup, which is obviously correlated, to what you're modeling here. So, I would suggest that there be a closer link than a 1995, or so, study that you talked about relative to loading. But, maybe Chris can help out here, or somebody can help out.

HARDIN: Thanks, Jack.

BAILEY: I'm Jack Bailey with BSC.

A couple of comments. First, the 99B waste stream, which is what existed in the utility inventory in the 1997-1998 time frame, is the numbers that we used in order to run the TSPA calculations. The ELWS, which is a simulation on what waste is likely to be there, we actually used a later examination of what is likely to be at the utilities. I believe it was a 2002 simulation which was used. That included the fact that waste was, in fact, going to be going to the ISFSI by the utilities for storage. And, it eliminated from the waste stream somewhere between 14 and 20,000 tons of material that was estimated to be on the ISFSI at that point in time.

So, the ELWS was intended to be, and what we've tried to model, was what would reasonably still be in pools for pickup by TADs in the 2017, 2020 time frame. So, we actually tried to look at a hotter waste stream than, much hotter waste stream than what would have been available in 1999.

So, the average age which Ernie didn't put into the presentation is a 48 gigawatt day, 14 year old element, is what we believe is the average, if you will, of TADs coming in under the estimated limited waste stream. So, it is a considerably warmer stream. I think Ernie has a slide here that he may want to talk to that illustrates why the ELWS is really a pretty limited stream.

HARDIN: Yes, Jack, this is a backup slide. It shows the average mass weighted burnup for several different waste streams. A, B, and C came from the 199 study. A prime through D prime from the 2002 study. And, of course, the message here is that TSPA assigned some uncertainty to this.

So, the bracket there shows what we sample on for the performance assessment. So, we cover all these cases, plus the ELWS, which lies right there.

MC CULLUM: Rod McCullum, Nuclear Energy Institute.

Since my previous discussions with the Board reference here, I hope it’s appropriate that I respond.

I believe you have to look at it in more detail, but I do believe what DOE is presenting here is consistent with what we in industry know to be the case. I think one of the key questions here is 10 percent of what. Right now, it’s 10 percent of 70,000. And, when you look at when the repository will open and the rate at which DOE would receive fuel after that, there certainly would be enough fuel still in pools, especially with, I think, the hotter assumptions, for them to get to 70,000 metric tons.

Now, in the EIS, they looked at 130,000 metric tons. They also looked at 10 percent and 25 percent. And, you will note in industry’s comments, which I’d be happy to provide to the Board, in industry’s written comments in response to the EIS, we talked about how these numbers might be achievable with higher repository capacities. Again, a lot depends on, you know, when they deploy TADs and when they open the repository.

So, while I can’t say with any absolute certainty, I can say that I don’t see any inconsistency with this, and that, you know, we still have to talk about how we're going to handle beyond 70,000 metric tons, because we're almost there. I mean, right now, we do have more than 10 percent of the fuel of 70,000 already in dual purpose casks, but that doesn't mean that has to be the first tonnage that arrives as we continue to empty pools. That all has to be worked out.

So, the bottom line is I think this is consistent, and there's a lot of variables that have yet to be defined. But, as those variables evolve, and as we see the second repository report and look at what the ultimate capacity of the repository is, this certainly seems workable within the parameters defined in the Preclosure Safety Analysis.

KADAK: So, let me see if I understand what you said, Rod. You're saying basically that whatever is now going into storage, and will continue to go into storage until a TAD is available, will stay at the site?

MC CULLUM: That's correct.

KADAK: For a long time.

MC CULLUM: Well, now, that's a matter of, you know, certain contractual obligations the Department has.

KADAK: Okay. All right. All right.

MC CULLUM: I'm in no way conceding any of that.

KADAK: I just wanted to clear the air here.

MC CULLUM: I'm just saying that whatever they do with the stuff that's already in DPCs, whether there's interim storage, or what not, I don’t know the Department’s test on that, they have to meet their obligation. But, certainly it is credible that they can get, over the time frames we’re talking about, the amount of material in TADs that they’re purporting to put in TADs.

KADAK: Okay. The second point, the industry is basically saying they will be prepared to ship five year old fuel, once TADs are available in sufficient quality to do this, I’m not sure when that will be, and that will be the waste stream that will be first used to fill Yucca Mountain; is that correct?

MC CULLUM: Yeah, I think almost all utilities are going to want to relieve the congestion in their pools. You know, they’re only going to get so much of an allocation each time DOE shows up, and they’re going to want to relieve the congestion in their pools first anyway. So, provided we certainly can license those for transportation with all the issues inherent there, and I know that’s where you’re going, and we think we can, you know, if we continue to work those issues with NRC, but, yes, that would be true.

KADAK: And, the third point is the TAD limit in terms of thermal loading is 22 kilowatts, as I understand.

MC CULLUM: That’s a DOE question.

KADAK: What is the thermal loading?

HARDIN: That’s what we have introduced in the study, is that the effective transportation limit for the TAD would become a basis, among the criteria for TAD loading.

KADAK: Okay. So, the 22 kilowatts is what you will receive at the repository, at which point you will have to store, according to your analysis, for at least two years before you can emplace, allowing for appropriate decay, and you're planning on a 50 year ventilation period once you continue this waste stream receipt; is that what I understand you to say?

HARDIN: That's accurate. It would be at least 50 years.

KADAK: At least 50 years. Okay, we have some maybe differing results in terms of our analysis. Maybe, Bruce, you'd like to comment at this point, or Gene?

ROWE: Yeah, first of all, I agree with Andy's comment that there's been great progress made in this area, and the work is very, very good.

We've done some independent analyses, and a lot of the results agree very closely, but the one that kind of stands out is the 2kW per meter limit over seven waste packages. We actually received--our results showed a significantly higher allowable linear line load than the 2kW, assuming a 30 day loss of ventilation during the preclosure period. And, even your report, there's a report out that--a project report on the repository twelve waste package segment thermal analysis that the project did. They did a case five, and I understand that case five is only a 1.45 kW per meter analysis. You indicated that the limiting thing was cladding temperature, 350 degree cladding temperature on the loss of ventilation accident, I believe.

HARDIN: Well, that was the basis for the 18 kilowatt maximum package.

ROWE: Okay. What's the basis for the 2?

HARDIN: Drift wall.

ROWE: Okay. Drift wall, okay. According to this analysis, the peak drift wall temperature, again, a low lying load, and I understand that, but still, your peak drift wall temperature is only like 105 degrees, which kind of supports what my calculation said also, that you might be a little bit low on the 2kW.

HARDIN: Well, I have to say that number doesn't sound right.

ROWE: The one in your calculation?

HARDIN: I showed you--I don't want to get out the rulers here, but I showed you a three dimensional on my calculation where we got 160 degrees at the drift wall peak, that's about 20 years after closure.

ROWE: So--after closure?

HARDIN: Yeah, postclosure.

ROWE: Oh, I thought that the line load was during the preclosure period when you lost ventilation.

HARDIN: Oh, I'm sorry, yeah, you're right.

ROWE: Okay. Are we on the same page now?

HARDIN: Well, okay, but I was connecting the dots there. The postclosure was--

ROWE: Postclosure is not an issue.

HARDIN: It's similar to preclosure, I mean, if you shut off ventilation, you essentially begin your postclosure trajectory.

ROWE: Right, but that's a whole different situation. We know, as you indicated, and I agree with you, that during postclosure, the issue is mid-pillar temperature, not drift wall, I agree with that 100 percent.

HARDIN: Okay.

ROWE: But, there just seems to be an inconsistency between the 2 kW and what this report says that you--

HARDIN: Yes, that's interesting. I'd like to perhaps discuss that with you some more. We have ventilation analyses, you know, analysis of the performance of the preclosure ventilation system that show that at 2 kilowatts per meter, you know, your temperatures go well above 100 degrees C, at first, at drift wall for a couple years.

ROWE: I have not seen those calcs. This is the only calc that I have seen, and like I said, this is relatively, reasonably consistent with the results we got independently, which would indicate a high.

But, can I ask one more question? Can I ask a question?

GARRICK: Yes.

ROWE: What is the criteria for closure? You have a criteria for emplacement, but what is your criteria for closure?

HARDIN: It would be a license from the NRC, based on analysis that we provide.

ROWE: I don't understand.

HARDIN: Well, before we emplace the waste underground, there will be an analysis done, detailed projections of temperatures, a demonstration that we meet the limits that are required. Okay? So, the closure term, or the closure conditions will be defined at that point. Those can be modified, but basically, the criteria for closure are known when the waste goes underground.

ROWE: I agree with that. But, it's also dependent on what the characteristics of that waste are, and what your ventilation period is. Do you have a fixed ventilation period, or is your ventilation period variable?

HARDIN: The analysis that you've seen here is based on ventilation until 2117.

ROWE: Which is 76 years, or something?

HARDIN: As much as, or more.

ROWE: But, you're not fixing that? That is still a variable? Gives you some flexibility?

HARDIN: Sure, it's a variable, just like end to end spacing is a variable.

ROWE: Okay.

KADAK: Just one final question. In terms of your criteria, I know the TSPA has now the 12.6 and 1.45.

HARDIN: Correct.

KADAK: But, your real criteria are related to mid-pillar drift temperature, as well as the criteria on the drift wall temperature. And, obviously, the cladding. Is that the criteria for acceptance, or are you not going to load anything higher than 12.6 to keep your TSPA intact?

HARDIN: The former rather than the latter. We will certainly load packages at higher than 12.6 kilowatts per package. We will do it on the strength of analysis that we provide on a drift by drift basis that shows that postclosure temperature limits will be met, so those are the controlling parameters. And, the TSPA uses a reference case, but I think we've shown in this study that the TSPA modeling basis is capable of representing the slight modification represented by the anticipated range of thermal loading.

KADAK: So, to bring this into more licensing terms, your operational tech specs will not be based on the 12.6 or 1.45. It will be based on a different criteria, based on what you might call your reload analysis for each drift. Is that what you're talking about?

HARDIN: That's correct.

KADAK: Okay.

BAILEY: May I make a couple of comments? Yes, the goal there is that the limits are in fact achieving 96 degrees centerline pillar temperature over the entire postclosure period, or less than that. They are staying within the wall temperature and the clad temperature that Dr. Hardin has already identified.

The loading rules of putting in at 18 kW per meter, no more--18 kW package, no more than 2 kW per meter, while the calculation of any seven package sequence meeting this 96 degree thermal energy density is a set of loading rules that meet that postclosure basis. So, those become the operational basis on which to load it in order to meet--

ROWE: That's based on a specific ventilation period?

BAILEY: It's based on a specific ventilation period and a specific spacing. And, if you vary those, you can change those results, and that's why it is an analysis at that time.

I need to make a second comment to something else you said, Dr. Kadak. The TAD specification contains no limit on the thermal requirements for the TAD. That is up to the vendor to determine for his system what maximum thermal limit he chooses to make on the loading of that TAD. We chose, DOE, a simulation at the 22 kW place in order to do this chart that's on the board that says we can receive a fairly aggressive thermal case, and still emplace it, and the cases below it, which Dr. Hardin's analysis show, all also fit within it.

When the vendors come back with what their values are, then we'll be rerunning, the reload analysis, each one of the drifts with what the maximum is, or what is actually shipped at that point in time.

KADAK: I just want to--loading rules versus operational tech specs. Your loading rules are going to be the 18 and 1.45 then, and you don't really need to be more cleaver about what you're actually going to be able to put in if you want to--

HARDIN: Actually, we have three loading rules, 18--12.6 is not a loading rule. 18 kilowatts per package, 2.0 kilowatts per meter, a local average, and the mid-pillar index local average must be less than 96 degrees, plus some margin that we get for hydrology.

KADAK: Those are your loading rules.

HARDIN: Yes.

GARRICK: Okay, we've got a little less than four minutes for Mark, George, Bill and Thure.

ABKOWITZ: Okay, I will take three minutes and 59 seconds. I'm going to follow Andy's line of questioning here to make sure that I have clarity on what's going on. Let's go to Slide 9, please.

Okay, as I understand it, this is the criteria that is in TSPA, and you've made the argument that the controlling limit is the mid-pillar temperature. So, essentially what you've done is you've run all these simulations and you've concluded that lo and behold, we can load at 18 kilowatts per package in a 2.0 line load, and not violate this controlling limit; is that correct?

HARDIN: That's correct, with the proviso that it comes with a schedule.

ABKOWITZ: Right.

HARDIN: And, there is ventilation.

ABKOWITZ: Okay. Now, before I move to the other question of my three minutes and 59 seconds, my understanding is that the 96 degrees limitation was under the previous assumption that's since been disproven, that the water actually has to drip between the pillars, between the drifts. Now, most people seem to think the water is going to go along the drifts. Am I correct in my understanding of that?

HARDIN: I don't think anything has been disproven. If you simulate the system in three dimensions, you can cause much of the water that's in the rock immobilized by heating to exit the end of the drift and condense somewhere else in the drift opening. And, that’s a fact and that gives us margin.

ABKOWITZ: So, you consider this then to be a pretty conservative assumption, based on new knowledge?

HARDIN: Yes.

ABKOWITZ: Okay.

HARDIN: It’s also part of our design basis.

ABKOWITZ: Okay, let me move on. One of the things that I would like to recommend, and I think Dr. Kadak was getting at this, there needs to be a considerable amount of sensitivity analysis done with your simulations with regard to a number of parameters, such as the percentage of TADs and the age of the fuel and the start of repository operations and the length of the ventilation period. Has there been any sensitivity work done beyond what you’ve shown us, or is that planned?

HARDIN: The TSM, which is an engineering study, has evaluated a number of different cases. So, they have used 30 kilowatts as a constraint, they have relaxed the YFF5 constraint. You know, they have done a number of cases. As far as generating an emplacement sequence to demonstrate feasibility of emplacement to meet these limits, we have done a very limited number of cases. So, we focused on the ELWS, which is based on that YFF5, 22 kilowatt case.

ABKOWITZ: Okay, and then the last question is conspicuous by its absence were any charts showing the distribution of the size of the aging pad required. Could you comment on that, please?

HARDIN: I could. For both the two year and the four year cases that I mentioned, it's within the capacity of the current design, which totals out at about 21,000 metric tons. So, with the two year case, of course you need less than with the four years.

ABKOWITZ: Thank you.

GARRICK: George?

HORNBERGER: Two questions, Ernie.

First of all, I just want to make sure I understood what you said about your three dimensional simulation. Can I infer that the only cold traps will be at the end of the drifts? There won't be any, you said there will not be any internal cold traps?

HARDIN: From our simulations, we see no evidence of that happening in the rock. I think in order to really see a significant cold trap process, you're going to have to have, you know, transport of moisture through free air over some distance.

HORNBERGER: And, the second one, you mentioned that your simulated changes in chemistry would indicate, as you said, precipitation of calcite. That has to go somewhere, presumably into the fractures. You mentioned it's a small effect. Is it so small that it won't feed back into hydrology, or have people considered this?

HARDIN: We have whole reports on that question. The dominant precipitates in the host rock from thermal effects are calcite and silica. There are other precipitates, but they're more soluble and dissolve and go away. So, those are the residual effects.

Yes, there are effects on permeability and capillarity of the rock mass. We have exercised simulations that express the limits of our knowledge about these processes, concluded that there are no really significant differences if you account for those effects on capillarity and permeability in the waste seepage would occur in the system, so that our treatment of seepage in TSPA appears to be still conservative. We use the ambient seepage abstraction.

Those simulations also show us that the, you know, that salts, such as gypsum and halite, rapidly redissolve when water returns to the near-field, and that they do affect the composition of water that could potentially seep into the drift. But, it's over with in a short time, and, so, that is not considered significant to TSPA. That's a FEP that we have excluded on the basis of other considerations.

GARRICK: Okay, very quickly, Bill and Thure?

MURPHY: Bill Murphy. Nice talk, Ernie. I have a couple specific questions.

I'm curious in the specific responses of the rock and the water. You talk about your 200 degree drift wall limit being dependent on rock strength. What aspect of rock strength is it? Is it the--how it affects your NUDEC calculations of principal stresses, or are phase changes addressed? What is the rock strength affect that you're concerned about?

HARDIN: Okay, it's really more qualitative. I mean, it's a soft limit, 200 C is also probably a tad conservative, but to my way of thinking, and Chestnut and I wrote a report on this ten years ago, it's mainly this. That if you take cores of intact rock to the laboratory and you measure their thermal expansivity as a function of temperature, you find an uptake in that response at around 200 C. That's been attributed to mineralogical phase, alpha-beta phase change in Crystabolite. That's sort of a weak conjecture. What's really happening is you're getting differential thermal expansion in the polycrystalline mass.

So, essentially what we're doing is limiting the extent of that type of thermal damage to the rock, which could affect the drift opening stability.

MURPHY: I was specifically interested in that alpha-beta transition, and other possible phase transitions, maybe dehydration of smectites, or whatever, that might affect the rock strength.

Secondly, with regard to the geochemical calculations, the water chemistry variations you showed were small and reasonable, but they didn't seem to reflect evaporation. Evaporation could, and you just talked about precipitation of halite and gypsum, evaporation could drive those concentrations to much higher levels potentially. But, the geochemical results you showed didn't seem to--seemed to me to reflect a lot of rock interactions with dilute solutions, and not evaporation; is that correct?

HARDIN: That's more or less correct. You know, as you recall from the presentation of that near-field chemistry model, it's really intended to represent the composition of what could seep after the drift wall cools back down to 100 degrees C, or so, and seepage is possible, and after a pulse of water that might have some salt dissolved in it may or may not enter the drift opening. Okay? And, the significance of that pulse is low because it's going to encounter an intact drip shield, so that FEP, there's a specific FEP for this, and it's excluded. So, consequently, the near-field chemistry model is representative of what is important to performance of the system.

MURPHY: Finally, I'm curious if you've made use of the drift scale heater test results either to compare to your model or to calibrate your model with regard to rock strength, or geochemical effects, or potentially other analogs. But, specifically, I wonder about the drift scale--

HARDIN: All of the above. Yes, the specific reports that deal with thermal hydrology, with thermal chemistry, and with geomechanical, thermo mechanical response all deal with the drift scale test at some level. You know, we have expensometry (phonetic) from the drift scale test. We have samples of water. We have CO2 measurements in the gas phase, temperature, humidity, saturation.

MURPHY: What were the drift wall temperatures in that test?

HARDIN: It was controlled, so that we hit 200 and we stayed there. We backed off a little bit on the power input to the test.

MURPHY: And, was there alpha-beta Crystabolite transition?

HARDIN: I doubt it. That hasn't been investigated. I think in pure crystals, that transition doesn't occur until about 220 degrees C, does it?

MURPHY: Thank you.

GARRICK: And, finally, Thure?

CERLING: Cerling, Board.

In your analysis, you had a ventilation period, a forced ventilation period for 50 years, and then you had other conditions. And, so, I'm just wondering was there still natural ventilation, or no ventilation after 50 years?

HARDIN: In those models, we don't ventilate, we don't allow natural ventilation after closure.

CERLING: So, that would be another conservative?

HARDIN: Yeah, we could. We have other reports that discuss, you know, processes that could push gas, you know, this way and that way in the drift after closure. It's basically a plus for us. If such processes occur, it will tend to cool and dry out the near-field.

CERLING: Thank you.

GARRICK: Okay, thank you very much, Ernie.

Okay, our final before lunch, we're going to hear about the plans for long-term corrosion testing and some recent results. And, Paige Russell I think is going to do this presentation.

RUSSELL: Strategic planning, I think we had just talked about before about having the presentation just before lunch, and I'm going to introduce this presentation. Actually, Dr. Wall is going to come up and walk you through it. He is our lead lab technical manager for the program, and it is appropriate for him to go through the details of what our plans are, and some data that we've gotten to date.

What I wanted to do is explain to you a little bit why we are now looking at a new long-term corrosion test plan, and answer maybe some questions about what this plan is to do, and where it's coming from. Doug will talk to you about how we still want to reduce uncertainty, and improve defensibility in our models. And, that is quite correct.

In addition to that, we have a requirement for performance confirmation in the area of corrosion, and we needed to build test programs and test plans to meet that obligation for performance confirmation.

Additionally, though, as we move forward, we have found that we have gotten questions from engineering. We had some discussions with potential TAD vendors that kind of enlightened us as to the types of questions that we may be receiving as we go to further engineer, fabricate, and then potentially operate the proposed repository.

So, instead of writing PC test plans independently of then looking at how to answer engineering questions, like removing an oxide layer, if that's necessary after stress relieving techniques, or variability in the ACM spec for Alloy 22, we decided, DOE decided to look at this as a whole, and put together a whole testing program that would get us to where we needed to be, that would have elements that would come out in eventual PC test plans, that would have elements that would increase defensibility and reduce uncertainty in our models, and also have elements that would eventually get us to be able to answer some of the questions coming from engineering, and coming from operations and vendors, which I think we all need to do.

In addition, as we get more information through environment, environment information coming in, be in a position that we had a planned program that was uniform and covered those types of bases. So, we put that forward as a report deliverable to our new lead lab. We had closure of the long-term corrosion test lab, which many of you have visited and seen, is now closed at Lawrence Livermore, and we are going to be moving forward now into a new program of materials testing and analysis.

Dr. Doug Wall and Dr. Neil Brown were instrumental in developing the program, and that’s why Dr. Wall is going to come up and walk us through now where we’re going, and hopefully, relate that to what that type of information will give us back.

WALL: All right, thanks Paige for introducing this topic.

I’m going to take a few minutes to go through exactly what Paige mentioned, the plans for long-term corrosion testing, and then also try to spend about half of the time I have allocated to talk about some recent test results related to testing under deliquescent conditions.

So, I guess we’ve already figured out the outline of the material I’m going to go through. Paige has already done a nice job talking about some of the objectives. I’ll fill that in a little bit and talk about approaches to both near and long-term corrosion testing. And, then, I assembled a table that's in the handouts, which is a compilation of 17 tasks that are included in the corrosion test plan, and I certainly have no intention of going through all 17 tasks, times the three columns in the next few minutes. However, if you look through those and you have questions about specific testing elements, I'd be happy to discuss those at the conclusion of this presentation.

After that, I'll talk about a couple recent test results. The first one is behaviors in Alloy 22 coupons exposed to deliquescent conditions. And, then, I'd like to take this opportunity to brief everyone on some new technology, new capability development for a next generation means of looking at high temperature controlled dew point test exposures.

So, once again, this reiterates some of the points that Paige made. The long-term corrosion testing plan itself is to obviously generate plans for the testing that will be carried out in the near and into the further future, with the primary goal of reducing uncertainty in our corrosion models.

At the same time, we want to improve the model defensibility and build consensus in the scientific community. So, this is not only collecting more data, as we might do to reduce the model uncertainty, but developing corroborative information and pursuing fundamental understanding of corrosion processes.

The test plan itself also establishes the information needed to start to lay out the facility requirements for a new test facility in order to meet these testing needs. So, this is just a bottoms up approach of looking at samples and environments, et cetera, and rolling that up into the kinds of equipment we'd need and personnel in order to make this test program work for the long-term.

And, then, finally, the test program I'm describing will intersect with a performance confirmation planning down the road. Performance confirmation can look to this test as an input, take a subset of the experimental plans laid out here, take that as part of the performance confirmation activities. Clearly, this won't restrict performance confirmation planning, but hopefully will provide some guidelines and some underpinnings for that.

So, the test plan itself has put together, breaks the testing down into kind of these time chunks. And, the first one of those is our current fiscal year, FY08, and then we group things as to FY09/10, and then sort of the longer-term testing, which is FY11 and beyond.

And, in FY08, some of the primary activities are, one, to go after reducing uncertainty in some of the current models. And, the approach that we're taking in FY08 is a very simple one, and this is just to reproduce data for select conditions, and that will help reduce the experimental uncertainty with certain experimental values and parameters.

We also have a very specific task of improving confidence in the screening justification for screening out localized corrosion of the waste package outer barrier, Alloy 22, due to deliquescence. Now, we have a fairly extensive analysis report that goes through a five step screening for this process, for us to screen this out. However, we certainly recognize that this just continues to be a point of contention, and we want to continue to do work to build consensus in the community over the conclusions that we're reaching in that particular line of investigation.

And, then, finally, we're also continuing SCC testing, which has been ongoing for a number of years at G.E. Global Research. And, so, those tests are continuing to accumulate hours into FY08.

In FY09 and 10, the mode changes from reproducing data under select conditions, and doing some confidence building, to going out and doing parametric studies, and doing a systematic investigation of some of the corrosion modes and processes. And, there's a number of examples of these in the attached tables. The one I'd point to, though, is the localized corrosion model. In that model, we have a lot of uncertainty as we go to, say, lower temperatures and lower fluoridine concentrations. These are very benign conditions, where one would not expect to have a lot of localized corrosion occurring in this system.

However, due to the paucity of data down at that end, we have a lot of uncertainty. We can go in and in a systematic fashion, look at the relevant variables of fluoride and temperature, pH and inhibiting ion concentration, and build a systematic, fundamental understanding of the behavior under those benign conditions, and that way, lower the uncertainty in that corrosion model.

In the out years, we transition to more of the long-term testing. And, this has some of the same flavor as what has been done in the past at the long-term corrosion test facility at Lawrence Livermore National Labs. Now, we're talking about putting samples into exposure systems for years, or tens of years, versus weeks or months of exposure, in order to build confidence in our understanding of the processes that are occurring.

In this time frame, we'll also take advantage of going after some secondary issues, and there's a number of examples of these as well. The one I'd mention off the top of my head is crevice material, or crevice former. In the localized corrosion model, once again, when we model the crevice repassivation potential, we use a very aggressive physical scheme, where we have a very tight crevice forming out of a very hard material. Well, we can improve our understanding of this process by understanding what difference it makes in terms of the load we have on that crevice forming material, in terms of the crevice area itself. And, so, we can roll these all up into a much more fundamental understanding of the corrosion processes.

So, this is the first page of the table that I mentioned a minute ago, and as I said, I'm not going to go through all these 17 tasks. But, to give you an example, I'll talk about Task 1, which is weight loss of the barrier materials, the Alloy 22 and then the titanium materials.

And, in FY08, the objective is to complete analysis of the long-term exposure samples from the previous long-term corrosion test facility. In the case of the Alloy 22, these are the 9 ½ year exposure samples. We can get these data and use those as corroboration of the data we have from the five year exposure samples, which are the inputs into the model used to predict the general corrosion rate for Alloy 22.

In FY09 and 10, we continue to analyze samples from the long-term corrosion test facility, but this time, looking at analog materials that were exposed alongside the Alloy 22. And, these are materials such as C-4, 825, 625, and G-3, I believe. And, the point here is these materials have different levels of some of the major alloying constituents, just chromium and molybdenum, which impart corrosion resistance.

By looking at these kind of one off materials, we can gain additional understanding on how these alloying elements impact the general corrosion behavior for long-term exposures.

As we move into the longer-term testing, once again, we'll be doing long-term exposure testing of these materials, inundated exposures will be the backbone of this testing approach. But, as I mentioned before, we'll also be looking at sort of these one off type scenarios. And, in the case of Alloy 22, an example is the black anneal oxide that Paige referred to.

And, so, having information about the surface condition and how that impacts corrosion behavior can enable us to make maybe different or better engineering judgments as we move forward on this project.

So, the remainder of this table I think covers a couple more pages, and I think if you flip through it, you will see that it targets some of the major corrosion modes, general corrosion, localized corrosion, temperature dependence considerations, corrosion under deliquescence conditions, hydrogen embrittlement, stress corrosion cracking, microbial influenced corrosion. There's a lot of different modes of corrosion, and it takes a while to put together a plan that does a good job at capturing all of them under the umbrella. But, as I said, if you look through there, I think you will see the major players represented. So, that’s an introduction to the planning that’s being done for moving forward with the corrosion testing program.

I do want to take this time, though, to also talk about some of the recent test results that many of you may not have seen. And, the first thing I’ll talk about is the results of dust deliquescence testing of Alloy 22, and a couple witness materials.

The materials that were exposed in this experiment included Alloy 22, 825, which is a lower molybdenum material, carbon steel, and then a couple stainless steel materials. So, we had kind of a range of susceptibility to corrosion, along with our barrier material, the Alloy 22.

Now, the point to this experiment, before I get into the details, was to take the Alloy 22 material, and expose it to deliquescent conditions under representative conditions, that is, to try to mimic repository conditions to the best of our ability, and in a laboratory setting. And, a lot of work has been done on deliquescence testing to date, but much of that has been done using inundated exposures, et cetera, where we have the goal of reproducing the chemical conditions in the repository, but maybe we missed the boat on representing some of the physical conditions. And, the most important one from the deliquescence perspective is the limitation on the available reactives for the corrosion process.

So, in this experiment, what we have done is taken coupons of the various target materials, and deposited it on them a very thin coating of a salt assemblage. In this case, we've done both a three and a four salt assemblage at their eutectic compositions. The salts are applied by hanging the samples in a vapor mist of the salt assemblage, the target chemistry, and then letting that dry, weighing the sample, and repeating the process until you have a target composition that you're interested in.

The ranges of applied loading, salt loadings in these experiments was about 500 micrograms per square centimeter, to about 1500 micrograms per square centimeter. And, that might not mean much to everyone, other than it's significantly more than what you would have from waste package exposed in the repository, but not too much, hopefully.

So, once you have the deposition of those salt layers, select samples were fitted with crevice, symmetric crevice forming materials. Then, both the uncreviced and the creviced materials were loaded into the exposure chamber, and the conditions maintained in a steam environment, which for 180 degrees C, is about 10 percent RH. So, this environment was then maintained for 50 days, the samples were removed and examined.

Running in parallel with the metal coupon exposures was a device intended to give an indication of the deliquescence state of the salt compositions that were included in the test. And, that’s shown on the far right there, and it’s a very simple arrangement. It’s an insulating polymeric block with two co-planer parallel bottom wires, closely located, such that we can put a salt assemblage on top of those wires, and monitor the resistance in between them. If their resistance drops during the experiment, that’s an indication that deliquescence has occurred, and that we’ve essentially shorted the wires together using bionic conduction path. If the resistance remains high, on the other hand, that would be an indication that we do not have deliquescence of that particular salt assemblage.

So, this slide summarizes the results from that experiment. Let me talk about the salt assemblages first, and then I’ll talk a little bit about the materials.

The three salt assemblage, when we had it loaded onto the resistance measuring device, we did not see a decrease in resistance at any point during the experiment. This would be an indication that deliquescence had not occurred.

However, there was some other physical evidence when the experiment was taken apart showing that it looked as if we had had deliquescence of the salt, followed by some flow, and then dry out. What wasn’t known is whether or not the dry out of this salt assemblage occurred when the experiment was discontinued, or if that happened at some intermediate point during exposure.

For the four salt assemblage, on the other hand, the resistance measurement that we were making immediately dropped to a low value upon exposure to the chamber, and maintained that low value for the duration of the test. This is an indication that we indeed did have deliquescence in the four salt assemblage, and that that was maintained for the full 50 days.

The table in the upper left-hand corner is a summary of the observations made for the various coupons following the exposure. The less corrosion resistant materials, including the carbon steel and the 304 stainless steel, showed signs of corrosion attack, and that’s seen in the top two SEM micrographs on the slide.

The 825 and the Alloy 22, neither of those materials showed signs of localized or general corrosion. Sampling of the SEM from the Alloy 22 sample is shown in the lower micrograph. This image was taken from a region that was below a crevice forming tooth in the assembly. And, while there appear to be stained regions on the sample, upon close inspection, you can see very fine polishing marks that start exterior to the crevice region, and go through the crevice region and in and out of the stained regions, with no evidence of corrosion attack on any of those discernable physical features.

So, that's the summary of the recent experimental data. I'm going to take a couple more minutes to talk to you about some capability development. And, this is just a very simple schematic diagram of the new capability that I wanted to talk about, and that is we assembled a chamber for doing controlled dewpoint fixed temperature exposures. And, without getting into a lot of the details of this, the beauty behind this system is that we're using mass flow controllers to regulate all the inputs into the test chamber, so we can regulate the amount of water that's flowing into the steam generator, and by extension, the flow rate of the steam into the system. We also regulate the amount of air or other gases that we want to control in the exposure chamber, and then we can monitor the gas stream to assure that we have the target dewpoint that we're going after in the experiment.

And, I guess the advantage to this new system, other than having this careful control, is the system capability. We know we can run up to 250 degrees Celsius, we're fairly confident, other than some O-ring materials, we can probably run up above 300 degrees Celsius, and we can run at saturation for any of the temperatures that we care to measure. So, this is a really nice capability for doing future testing under deliquescent conditions. It also provides electrical access to the samples so that we can do in situ monitoring.

This is just a picture of the system. The only point here is that it's primarily constructed of commercial off-the-shelf vacuum equipment type fittings and hoses and chambers.

I wanted to give a quick example, just showing that we actually did build the system, and it does work. We've run some shake down experiments, one of which the objective was to look at the stability of a simple salt. In this case, we used a calcium chloride to water, and just exposed it to some target conditions that our geochemists gave to us of 150 C, and a dewpoint of 91 degrees Celsius. We ran this for a week, noting that we were able to maintain all the target conditions in the test chamber, and results from the chemical stability perspective is that we started off with the calcium chloride to water, as verified, using x-ray refraction.

Following the experiment, we maintained the peaks for that original chemical composition, but also picked up peaks corresponding to calcium chloride hydroxide, showing that we observed partial decomposition during that experiment.

So, the last kind of bit of news I wanted to share today is another technique development activity that's ongoing, and this is to develop an in situ technique for monitoring the extent and rate of corrosion attack on the samples under deliquescent conditions.

And, you might imagine making corrosion measurements at very high temperature and high humidity, but without any electrolyte around is a really difficult technical challenge. Fortunately, folks have been working in the area of atmospheric corrosion for quite some time in other systems, and have developed some techniques for doing in situ monitoring in low aspect ratio water layers.

Basically, the technique that I'm talking about is using what's called a direct-current potential drop to look at resistance of a sample. And, the simple way to think of this is you have a sample, and you start losing material from it, you're going to increase the resistivity of that--or, the resistance of the pathway across that sample. So, if you put a current across it and measure the potential drop, you can calculate a resistance, and that resistance will be proportional to the amount of damage that sample has sustained.

And, I have some example data here that was provided by Rob Sorensen from Sandia National Laboratories. This is, from our perspective, a proof of concept for the

Program. It was data that was input into some decision making process. But, the take-home message from this plot is what we have are samples decorated with different concentrations of contaminant, and then we monitor resistance as a function of time in an atmospheric environment, and we see an initial change resistance followed by a tapering off. And, the level that's attained is consistent with the level of chloric contamination that was on the sample originally.

So, this is demonstrating that this technique is appropriate for going in in an in situ approach to looking at all phases of the corrosion process, from initiation through damage accumulation or propagation, and finally, stifling.

And, that about sums it up. The conclusions from this presentation are just that a plan has been developed for the long-term corrosion testing. We continue to have data on the front of corrosion due to dust deliquescence. The most recent results are confirmatory to our conclusion that Alloy 22 is not susceptible under these conditions. And, then, finally, that we're continuing to develop new capabilities to improve our ability to study these processes.

I'll take any questions.

GARRICK: I yield to David Duquette, our technical lead.

DUQUETTE: Duquette, Board.

As you know, and the audience should know, we also visited you on December 5th, and you presented some of this information to us then. It was a very interesting and productive visit at the time.

I'm going to jump right to your conclusions and go to the second conclusion that said recent test results support the conclusion that Alloy 22 will not undergo localized corrosion under deliquescent conditions in the presence of three and four salt assemblages. Do you really think a 50 day test showed that?

WALL: Excuse me? A 50 day test?

DUQUETTE: Wasn't it a 50 day exposure test?

WALL: Yes, it was.

DUQUETTE: And, you think a 50 day exposure test allows you to make that conclusion unequivocally?

WALL: I think the data are supportive of the conclusion. Clearly, what you're looking for is the conclusive form of that experiment would last for the duration of the repository.

DUQUETTE: No, no, not at all. You saw some staining of the sample.

BROWN: Neil Brown with the lead laboratory.

When I saw that SEM photograph, I got just a little excited, so I got on the airplane and went out to Livermore and looked at it very carefully, three different optical microscopes, polarized light, SEM. There is nothing there. What you're seeing in that SEM photograph, when they clean the sample to get the salt off, they left an organic residue, and you can see that residue moving around on the SEM as you focus in and out. I didn't have time to get it cleaned and get rid of that residue, so the staining you're seeing on that photograph, those are not stains on the sample per se. There is nothing there.

DUQUETTE: Okay, I'm not going to argue about the staining. I just think that the test I think is a right direction to go in. I'm not sure that it--I guess support is a better word than proves, but I have some concerns with the test.

The other concern, major concern I have is with your table, and I had the same concern when we were at Sandia, and it's not something you can address I don't think directly, but I think the Board should be aware of. That test program is certainly going to take a lot more than two principal investigators, unless you're planning on doing it between now and your retirement date. It's a very aggressive program. It's going to be a very expensive program, and I think it's the right program to follow through, but I think there's some concern, I would have some concern about whether it can be accomplished with the resources that you're going to have available to you.

WALL: I think that's a fair statement, certainly that all this work will be contingent upon the appropriate level of resources to carry it out.

DUQUETTE: And, so, the next question that would come up, I would hope that for the next time a presentation is made to the Board, whether it be informally or formally, would be to prioritize those particular experiments as to what you want to do them for, and what you intend to get out of them.

WALL: Fair enough.

GARRICK: Ron, and then Bill.

LATANISION: Latanision, Board.

Let me just follow up on this last point that Dave is making, and go to Slide 5. One of the items, it’s under the ’09-’10 fiscal year, third block down, it says to determine threshold values of chloride and nitrate. Now, you know that there had been a workshop on this issue, I don’t remember whether you were present at that workshop, it may have been before the transition.

WALL: Was it the September ’06?

LATANISION: September ’06.

WALL: I was in the audience, yes.

LATANISION: Oh, you were, okay. And, you will remember there was a lot of discussion of work from Livermore and from San Antonio on thresholds. So, I’m not sure whether you’re going to find anything new in this work, but I just want to make sure that you know there has been a lot of effort put into the issue of threshold, not only concentration, but ratios.

WALL: Sure.

LATANISION: And, one other point, a very important point that emerged from that workshop was that at least from the perspective of Dave Duquette, myself and the staff, we were of the opinion that there were two experiments, that if they were conducted and proved affirmative in terms of their results, would be compelling evidence to us that one could safely screen out localized corrosion and consequence of deliquescence. One was to look at the nitrate/chloride ratio as a function of temperature to demonstrate that we knew what ratios would be required to provide protections over the range of temperatures expected in the thermal transient.

And, the second part of that experiment would be to demonstrate that acid degasification would be such that the nitrate/chloride ratios that were required at those temperatures would be achievable. Now, Doug, I don’t see that anywhere on this list. I mean, I like what you’re doing here, but I’ve got to say that given the time that’s available, and what seemed to be an important consensus of people present at the workshop, that ought to be high on this list of priorities. And, I don’t see it represented, maybe it is and I’m missing it, but that, to me, should be a first order--

WALL: Yes, two points. I mean, not every detail that's in the testing plan is going to appear in this table. This is more of a highlight of what's in that plan. So, it would be better to have a discussion, if we're going to talk about the details of experiments, to do that after everyone has seen the details of the plan.

More to your point, though, about going after sort of a silver bullet to put this issue to rest in the Board's mind, I think all that input is very valuable. I think, though, that there are constraints that I see from an experimentalist point of view in terms of you're proposing a strategy that relies on a lot of knowledge about the environment, about tying it down to fairly specific values of nitrate and chloride. And, I think we could generate the data that you're talking about if we could have a consensus on how long you run the test before you know it's been long enough, et cetera.

But, from the information I've looked at, and I have to bring a personal spin to this, from my experience, I feel a lot more comfortable about going after the phenomenon of seeing this corrosion either scale with the initial contaminant concentration, or just shutting off. And, I know we haven't completely seen eye to eye on that approach, but I think that in the experimentalist world, there can be more than one way to get at, you know, a suitable answer.

LATANISION: Doug, I don’t disagree with that. There are often many ways to get to an answer. My simple point is that we have been discussing this long before there was a transition in lead labs for years, and I think the workshop came to what I consider to be a fruitful position and thought on how to really put this to rest.

You remember there was initially a list of five or six elements that had to be either removed from consideration, or were considered applicable to this issue of localized corrosion, and systematically, they were all picked off in terms of whether or not they would rule out localized corrosion. This is the remaining issue.

And, so, I think from the point of view of coming to a working conclusion, I would just suggest that it ought to be a higher priority.

Let me turn to Slide 10. It addresses the experimental issues. No, I guess I mean Slide 14. Is that the one that shows your chamber? Which one is the chamber? There you go. I’m really impressed with this because I think it has the potential to answer the questions we’ve just been talking about. Now, you’ve done work on calcium chloride in this chamber to this point, but you could just as well use salts, you could put the salts on a glass slide, or better still, on a C-22 surface. You could determine by instrumenting it what gases are coming off as a function of temperature. You could do everything that we’re asking for in a chamber like this.

You know, you take resources or take re-distribution of resources, but, to me, you’ve got the wherewithal to answer the questions that we think would put this issue to bed, and I would just encourage thinking about, you know, what experiments and what priority you’re going to attach as you go forward.

WALL: Sure. And, we have certainly looked at the technical challenges in terms of doing some of the gas analysis for this specific system. And, they’re not insignificant. This is not an easy experiment to do, and I think we’re in agreement that we have the right foundation here to move forward. And, as I said, I think there’s a couple paths we can go down. I still hesitate to rely too much on a definition of the salt composition as being the determining factor. I would rather have a more universal answer to how the deliquescence process occurs, independent of what that salt composition is.

LATANISION: So, just to understand, you’re not convinced the nitrate/chloride ratio is a significant factor?

WALL: No, I believe the nitrate to chloride ratio is a significant factor in terms of determining the corrosivity of the environment. However, I do not believe that knowing that value, or strictly relying on that value is the end all to understanding what the limiting processes may be under a deliquescent environment. I mean, I would suggest that we have chemical, electrochemical, and physical limitations on corrosion under this system that we don't have in a lot of other corrosion processes. And, we're building information and I strongly believe that the analysis that's in place now relies on looking at each of these factors, instead of just looking at a simple chemical factor, which is a very solid way to do something, is to look at a chloride to nitrate ratio. I mean, it's pretty well established for a lot of materials the inhibitor to aggressive anion ratio. But, I think there's other low hanging fruit in this system that we can go after.

LATANISION: I guess I disagree with that, but that's a different discussion. Let me turn to Number 14, last question.

SASSANI: Excuse me. Could I just amplify Doug's response?

GARRICK: Give your name.

SASSANI: David Sassani, Sandia National Laboratories.

Relative to the geochemistry, I wanted to indicate that dust deliquescence AMR, the report, the screening report, does cover those five areas, and it relies on each of the discussions, and there is an acknowledgement of the amount of uncertainty in the geochemical modeling.

One point I'd like to take a little bit of contention with is that there are--none of the other arguments besides the nitrate to chloride ratio that hold up. In fact, one of the major bases in that report relies on some very well constrained mass balance arguments, and those, in fact, are the very strongest part of that, because they are fundamentally bounding and very certain in their classification.

And, in fact, with the amounts of salts that are bounded on the way to package surface in the dust, the amount of fluid is extremely small. It's small enough to the point where it is one to two orders of magnitude less than can support even the residual saturation of the corrosion products that would form for a pit that would penetrate the package. That basis is very strong.

The amount of chloride is extremely small. It's maybe on the order of .2 to .3 micrograms per square centimeter, as compared to the experiments Doug was talking about at 500 to 1500 for total salts. We're looking at more like one to four micrograms per square centimeter of total salts in a bounding analysis. That analysis is bounding by at least an order of magnitude in terms of masses, and that's without taking any mediation effects into account. That's just looking at the system where the largest amount could be. You could easily get one to two orders of magnitude extra margin by doing some sort of reasonably straightforward mediation for dust.

So, I think those mass balance arguments are the fundamental reason why even though we're pursuing these other areas and looking to decrease the uncertainties, it's really difficult in my mind as a geochemist, looking at ore deposits formed by vast amounts of fluids, to get real, real excited about these very, very tiny amounts of fluid on these surfaces, and the tiny amounts of chloride involved, which would translate to incorporation into corrosion products on the order of a couple to 300 parts per million if they were sequestered.

So, I think there are other arguments that have much less uncertainty that are really the basis for that, with the others supporting those, indicating that we don't, given what we can do in terms of the geochemical environment, and the uncertainties involved, we don't really expect it to be a major problem.

LATANISION: Well, let me just say, you know, that's a point of view that I do appreciate, and I think it may be ultimately shown to be correct. But, what I am concerned about is that we have probably ten years of work that's been done on this issue by CNWRA and by Livermore, which led us to a point that I think is also defensible, of looking at nitrate as being an inhibitor, and coming to a conclusion that if we knew the nitrate/chloride ratio that would provide protection, and we could demonstrate that it was present at the conditions of operation in the repository, we could feel confident that localized corrosion could be screened out. That’s my point.

And, I think all I’m saying to you today is that I think you have an opportunity to do that and put it to rest. Without being rhetorical or otherwise, you could do the experiments and put that whole issue to rest.

BROWN: Neil Brown with lead lab again.

I’ve heard you. We’ve heard you. If you look at the detailed test plans that are described in the Sandia report that Doug and I put together with input from Lawrence Livermore, first of all, you will see that we are building upon the knowledge base gathered to date. We also have--we are looking at the nitrate and chloride ratio, not just in inundated environments, but also in deliquescent environments. We also have a large quantity of TBDs to accommodate some flexibility. You know, the plan, of course, we know we’re going to do more experiments than what we’ve identified to date, and as we gather information, the TBDs of that matrix will be filled out. I think you need to look at those detailed plans, and I think you’re going to see what you’re looking for.

LATANISION: Well, I just don’t want to lose ten years of work that I think was valuable.

BROWN: No, I managed for the previous five years the corrosion work before the transition. We haven’t lost that continuity. We still have it.

LATANISION: Good. Okay. Now, Mr. Chairman, one final question, if I may, and this is on--

GARRICK: Yes, go ahead.

LATANISION: --this figure. The corrosion that occurred in this instance, was it localized corrosion?

WALL: I actually don’t know the answer to that. I don’t know if it was localized corrosion--I mean, I know this did not have a crevice forming geometry on it. The nichrome material is much more susceptible than anything we’re talking about. I don’t know. I’m not even sure how you classify it, there was a very thin film material. I think differentiating localized corrosion from rapid general corrosion in the area where the contaminant was might be semantics. I’m not sure, but I don’t have the answer to that.

LATANISION: Once again, I’m just concerned about this transition from, maybe in semantics, because in the previous discussions about stifling, it was typically--that was typically reserved for the notion that localized corrosion might stifle, and there were some experiments done by various people that they believed showed stifling. And, I just wanted to make sure that, you know, if in fact this is stifling of localized corrosion, then you should be able to see some evidence of localized corrosion on the surface. If it's uniform corrosion, a different issue. I think knowing what the corrosion mode is in these experiments--

WALL: Right, and I would urge you not to put too much stock in this, other than a demonstration of the measurement, not an analysis of the result from the experiment.

LATANISION: Okay.

WALL: I'd say we do it on the real stuff. Then, we'll have the discussion again.

GARRICK: All right, Bill and then Ali and then Bruce.

MURPHY: I've been curious about this mass balance issue that Dave Sassani brought up, and also very interested in whether or not there were differences between corrosion effects where there's a decoration of very small amounts of salt on a surface as opposed to an emersion test. And, you mentioned that that was one of the rationales for building your decoration vapor suspension system. Did you see a difference? Did you see a difference between emersion tests and your decorated salt tests?

WALL: I'm not sure what type of difference. Are you talking about whether we observed corrosion versus no corrosion?

MURPHY: You didn't see any corrosion in your decoration test?

WALL: No.

MURPHY: Are there comparable tests under emersion conditions where corrosion did occur?

WALL: We don’t have the exact replicate of the experiment under corrosion conditions. What’s been done under inundated conditions typically, some of the data that Ron refers to, has been cyclic polarization data, looking at repassivation behavior. And, in that experiment, we’ve added in another stressor, and that is potential, and, so, that’s--it’s hard to correlate anything back from that measurement to an exposure where we’re just putting it under the prescribed conditions, and looking to see if something happened. I would say they’re consistent, though, and that we have seen high repassivation potentials in environments that are based on the ratios of salts in the salt assemblages that we use in the deliquescent testing.

MURPHY: A related question is that you precipitated salts on these coupons fairly densely compared to what you might expect in the repository environment, and then you saw a decrease in the resistance between your platinum wires. Did that imply to you that there was a continuous film of water generated due to deliquescence, or was it spotty?

WALL: For these samples?

MURPHY: For these samples.

WALL: My intuition is that there would be a continuous absorbed water layer on these samples. You have to keep in mind, just to clarify, that that resistance measuring device was not decorated in the same fashion as the samples. That was decorated, and I wasn’t there, but I believe with a scoop of the salt assemblage. Plus, there was a macroscopic amount, so that it formed a conductive pathway, it would certainly be conductive enough to detect.

MURPHY: And, final question is in the one test case you showed here where calcium chloride with water decomposed to calcium chloride hydroxide. Do you interpret the results of that experiment to imply that hydrogen chloride vaporized?

WALL: You should not mistake me for a geochemist. But, from what I know, I would assume that was the reaction taking place. Do you want to--

SASSANI: Yes, this is Dave Sassani, the lead lab, Sandia National Laboratories.

Yes, we think those corroborate the thermal gravimetric analyses that were done at Livermore with the evolution of HCL causing the phase transition.

MURPHY: Thank you.

GARRICK: Thank you. Ali?

MOSLEH: My question is triggered with the earlier discussion that we had and the use of the term long-term testing versus short-term testing. If time is a factor, how do we determine that a 50 day testing, or 500 day testing is meaningful, compared to the time scale of the repository?

WALL: I think that’s an issue that obviously is intertwined with the whole subject of predicting corrosion performance for extended periods of time. We cannot hope to make a measurement for 50,000 years or 100,000 years. What we can do is learn about how the system is evolving.

And, so, in the case of general corrosion, which is one of the ones where we do additional longer and longer term experiments to gain more and more confidence in the results, you gain two things from that. One is you gain an understanding of is that rate the same or lower as you go forward in time. And, so, for longer term experiments, you tend to increase your confidence in your previous results.

But, you can do something else with these experiments as well. And, this is something that I’m questioning going forward, is that you look at, at a very fine scale, at the nanostructure and the chemical composition of the oxides and all these materials. So, you learn not only from a quantitative measurement of weight loss, but by doing a complementary investigation of the structure and the chemistry at the surface of that material, what the stability is as you go forward in time.

MOSLEH: So, is there such a thing as, you know, in the field of equipment testing or material testing, in the reliability, we have this concept of accelerated testing; is there such a thing here?

WALL: Well, I think accelerated testing is a whole field of, I mean, used in corrosion science, and I've been involved with a lot of accelerated testing over the years in other areas. But, you run into a real problem when you're talking about acceleration over the course of a million years. If you take a material and you stress it enough to make things happen at a measurable rate, you may have perturbed the system so far from the realistic scenario, that the results you're seeing just aren't meaningful in terms of extrapolation. It's a difficult trade-off. The other option is to increase your measurement sensitivity.

MOSLEH: So, then, in that scale, what's the difference between a two day test and a 50 day test?

WALL: I don't think there's a black and white answer to that question.

BROWN: Doug, let me add to that. Part of that experiment, you know, we had to pick how long we ran it for, and that is one of the limitations that we're trying to overcome with being able to perform real time electrical resistance measurement.

If you look, for instance, recognizing this data is not ours and it's not our material, but if we could get something like this showing that corrosion started, which you can see in the first 15 days or so that corrosion started on this material, and then it slowed down, that’s the sort of information we’re hoping to gather as we go out in time. Because if it starts and then slows down, then we say the experiment has run long enough.

When we’re in a situation where it hasn’t started, there’s always a question well, what if you had gone 50 more days. We also are throwing in less corrosion resistant material, such as Alloy 25 and stainless steel, where you saw on the 50 days, that the stainless steel did undergo localized corrosion, but the 825 and Alloy 22 did not. A fair question, though, would be well, what if you had run it 100 days would the Alloy 25 have started, yes or no. So, that’s a lot of what the matrix is trying to examine, is well, what if we had run longer, what if we had had crevices. So, it’s not an easy question, and that’s why for the inundated environment, we’re fortunate in being able to use the short-term test using the cyclic polarization to gather information about critical potential. But, it just isn’t really applicable for the deliquescent conditions.

GARRICK: Okay, very quickly, Andy and then Bruce.

KADAK: Thank you. I’m just going to try to make some sense about what it is that was presented here, because I’m not a materials person.

Your test that you just showed the results of appears to be more reflective of what you think the Yucca

Mountain environment is; is that correct?

WALL: More reflective than what?

KADAK: Than what previously was done in the corrosion work that was reported in 2006, you know, where they took a very, I think in your word, accelerated way of trying to figure out, get some reaction to go, some corrosion to go. Your study here says this is what we think it is. Temperature is 180 degrees centigrade. The two or three or four species that you implanted on this Alloy 22, are what you actually think might occur at Yucca Mountain.

WALL: Right. I wouldn't take any one test in isolation over the body of work that's been done. I think you have to look at each one, how the environment was chosen, and what the information was intended to be that comes out of that. And, so, I think--I'm not saying this is better necessarily than an inundated experiment that was done using electrochemical techniques, I'm saying it adds another piece to the puzzle. And, in this one, yeah, I think this type of an experiment does a better job of getting the environment correct than the inundated type experiments.

KADAK: And, the fact that you didn't see anything in 15 days versus 100 days, I think the way you characterized it was seemed to support the conclusion that we don't get localized corrosion from deliquescence of Alloy 22.

WALL: Right. If you look at the electrochemical data that's been collected in the past in high temperature brine solutions at different nitrate/chloride ratios, looking at that data, you would predict that you would not see initiation on the Alloy 22 in this experiment. So, when you actually run this experiment, and get that resolved, it's corroborative.

KADAK: And, this information I think is the result of that corrosion that you guys had where you couldn't find something that looked like the repository environment, and this is what your response to that was; is that sort of correct?

LATANISION: Latanision. Just to add a point, Dave asked a relevant question, and that is do you think a 50 day test is representative of enough information to give you confidence that you can screen out deliquescence induced corrosion. Now, the question, real question is what happens over a period of time? Do you have sufficient nitrate to inhibit corrosion over an extended period of time? And, obviously, you're not going to wait 10,000 years, but there's a kinetic issue here, Andy, it isn't just a matter of exposing a sample that's been decorated with salt. I like the experiment, but I think there are a few more steps that you'd have to go forward with in order to really feel that you're doing something that at least is consistent and reproduces the kinds of conditions you would expect over time in a repository environment. That’s the only point.

I agree with what Doug said. This is an isolated point. It hasn’t integrated into a base of information, and I don’t think there’s any disagreement on that.

KADAK: And, the information we had from that other gentleman was the amount of nitrates and chlorides that would be deposited is so low that you probably wouldn’t see even what you decorated the Alloy 22--

WALL: Correct.

KADAK: That’s kind of what I’m trying to put my arms around. But, I feel better.

WALL: Yeah, the actual case would be much more benign than what we’re testing.

GARRICK: But, there continues to be the question of how relevant this is to the actual environment that you would get from more field experiments. Isn’t that correct?

WALL: I think there’s more we can do to get back to, say, a better representation of what a deposited dust layer would look like that has a certain salt component to it. So, thus far, we have an idealized system where we have used pure salt assemblages without the organics from the dust that would be deposited on the waste package surface.

GARRICK: All I’m saying it’s part of Andy’s question was does this really represent reality? Does this represent experiments utilizing, to the best of your ability, field measurements of what the dust really is, under the conditions of the repository?

Anyway, I think there's more to be said about that. I don't think that--

BROWN: I guess I would add to that, though, that--or respond to that. There are plans that we're going to take actual dust collected, put it on Alloy 22, and let it run in this equipment.

GARRICK: I see.

BROWN: Now, before we do that, we want to walk before we run. We've started with calcium chloride, we're almost done with the shake out on this equipment. Then, we're going to go to some salts assemblages. And, once we know that we've got the parameters set right, then we'll go do real dust samples.

GARRICK: Okay, Bruce, have you got a quick question?

KIRSTEIN: Yes. It was regarding Slide 13, which is the degassing of calcium chloride. Doesn't this imply then that the chemical environment may change with respect to time, not only with regard to the loss of chloride, but the loss of nitrate? And, therefore, this testing that you're looking at is evolving very rapidly if this is only a one week test. So, what would you have after a year on a waste package? Are you giving some thought to how to predict what that environment will be over the long-term?

WALL: I’m not in the business of predicting the environments. I’m more in the business of being fed that information and assessing the materials. But, I would say, though, that another advantage of the type of experiment that we looked at in these slides of using a thin deposition of a salt assemblage, and doing exposure, is that condition does have the opportunity to evolve. And, if that chemical environment is going to evolve in a short period of time, say in the orders of days or a week, and you’ve run a 50 day or 100 day, et cetera, experiment, now what you’ve done is you’ve seeded the environment with the initial composition of the salt assemblages that might be floating around in the air out in the desert, and you’ve allowed them to undergo the transitions that they would experience due to that thermal profile. So, I think that experiment captures that as well.

Now, we don’t know what the end result of that was. We haven’t analyzed the salt at the back end of that experiment, but it certainly had the opportunity to transform.

GARRICK: Okay.

SASSANI: This is David Sassani from the lead laboratory, just to augment what Doug was putting out there to address the question a little bit, and I know Doug is making references to being fed, so he’s getting hungry for lunch, I’ll see if I can serve something up from the geochemistry side.

GARRICK: Not too much.

SASSANI: Not too much. Yes, the system was expected to evolve. Thermal evolution of the system, the brines and the salts have been evaluated in the dust deliquescence AMR from a number of standpoints. I think you’re going to hear some other discussion today of other possible evolution, all of which contributes to the uncertainty of our understanding of exactly what will be there. But, a couple of things that you can take away from these is what we do see is for salt phases that are the potential high temperature brine formers, we have good evidence that the chloride will evolve. I think you’re going to see some other evidence that the nitrate will evolve, and possibly evolve at a different rate than we may have expected before, by consideration of the full suite of materials in the dusts, possibly organic reactions.

But, again, all of these things evolving, the chloride and the nitrate out of these dusts over time, and looking at it over the periods of time that are relevant, may lead to a dust that has very little of these high temperature brine forming deliquescent salts in them, and it would make all of that consideration less of an issue. And, in fact, these experiments that are being done are looking at what appear to be the most extreme types of environments.

So, if these environments aren’t what we’re worried about, everything else is less extreme, much more benign.

GARRICK: Okay. Yes, Ron?

LATANISION: Just a final point to wrap up. I agree with your comment. If, in fact, this experiment is done next, and I'm talking about the experiment in the chamber to look at acid degasification, I think that would be a great experiment, if you use the ternary or quaternary salt and you looked at the nitrate, nitric acid, hydrochloric acid evolution, so that you could look at the question of what rates are they coming off, and what are the concentrations, this chamber is really a wonderful piece of equipment. In fact, I would recommend you build several of them because there's a lot of work that you can do that would be very meaningful with this chamber. So, as I said, I'm very impressed with it. I would just like to see it used with something other than calcium chloride. And, I think the ternary salts would be a good start.

SASSANI: I agree with that. I think they did a great job.

GARRICK: Paige, did you want to make a comment?

RUSSELL: Yes.

GARRICK: A final comment?

RUSSELL: Final comment. And, I think this is an important final comment. It goes back to Dr. Duquette's comment about this is plans and a very large and a very expensive program, and what’s the chances of us moving forward and getting this information.

I don’t know that I made it clear in the beginning. This plan was done not because Dr. Wall woke up one morning and said I want to write down what I’d like to do for the next ten years, but because DOE directed the lead lab to do this plan. It was done under direction. It was also not done just as an internal report, but it was done and required that it be submitted to the DOE for review and acceptance.

So, there was investment of the DOE not only in saying write down what we need to do, but I’m going to review it, and I’m going to accept it. And, one of the criteria that that plan was reviewed against was not that I can know possibly what happens next week or next year as far as resources availability, but I certainly have the past ten years to look at at a profile of resources and availability and support. So, that plan has got to meet what the DOE can see the future as being able to be done.

So, what you really do see in that plan is investment as an organization of the DOE in what we need to do to go forward. It also is, to some extent, a prioritization of it wasn’t everything you would ever want to do, Dr. Wall and Dr. Brown, but what do you believe are things that need to be done and can be done, and where can they be done, and when do they need to be done.

So, I think when you go through that plan and look at it as a whole, it had to be doable to be accepted by the DOE. It had to be done, it had to be delivered, it had to be reviewed. So, I do think you’re going to see some of that prioritization in the fact that it is in that plan, and it was reviewed in that framework, in that need.

GARRICK: Thank you. This has been a very good discussion, I believe, and very helpful. And, we’re about ten minutes behind our schedule, so I’m going to take the liberty to say that we’re recessed until 1:55.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

GARRICK: Zell Peterman is now going to talk to us about the effects of temperature on the composition of soluble salts in dust, a subject that we have been very interested in for quite some time, and have heard a great deal about already today.

So, Zell, straighten it all out for us.

PETERMAN: All right, thanks, John, and I thank the Board for this opportunity to present our results, which have been generated over the last seven years, sort of Phase 1 of the USGS dust studies. There was another dust study that started in 1984 funded by the Yucca Mountain Project, but the objectives were quite different. At that time, the work was done to understand the genesis of soils and the flux of carbonate that produced these thick caliches, and all that.

Our work, the second phase started in early 2001, following this directive from the person who headed up the Engineered Barrier System work, and that’s John Pye, who was I think with TRW at that time. And, the instructions here were fairly explicit. It was sample dust, have a sufficient number of samples to be statistically valid.

And, then accompanying this was another document by Ernie Hardin that was a little more specific on some of the methods that we might take.

What I’d like to do this afternoon rather quickly is I know the interest is in the recent results, where we see loss of nitrate on heating, but I'd like to give sort of a historical chronology of the dust studies, and I'll do that, give a few slides on how we collect the dust, the methodologies we've used to analyze the dust samples, and then talk about underground dust, and then go to the surface, talk about some of the surface dust work, and then talk about the drift scale test dust, and then some experiments that were completed in the last six months, or so, on laboratory heating of some of the existing dust samples that we have.

The first dust is, you know, there's a lot of people the world over who are very much interested in the dust flux at all different scales. You know, dust isn't just generated at Yucca Mountain. There's a global flux that sometimes circles the earth. Largely, we get our dust from dust storms in Asia. There was one several years ago that was really--really maintained a coherence from huge storms in the Gobi Desert.

There's a regional flux in the arid southwest here, and that's the subject of this USGS work that began in 1984, and then was transitioned to funding by the USGS in about 1990. And, then, there's the local flux, which is the result of activities, both surface and subsurface.

The objectives, in addition to those outlined in John's document, our objectives are to characterize the major and trace element composition of underground and surface dust that may accumulate on the waste canisters. And, to do that, we have a two-fold approach. We will characterize the bulk dust samples, that is, just whole dust samples, total dissolution, and we'll look at the soluble salts, which are obtained by leaching the dust samples.

We identify three major sources of dust, and these are rather strange terms, but they're kind of useful, geogenic, and those are the natural components of rock and soil, technogenic, anthropogenic materials, and biogenic. And, I think we see all those both in the surface and underground.

Our approach is for the underground work, we had several sampling campaigns of underground dust, from tunnel walls, flat surfaces, pipes, electrical cabinets, and so forth. At the surface of Yucca Mountain, anything that sticks up above the surface a bit is going to have a little dust dune on the leeward side. And, there are also rock depressions that collect dust.

And, then, the most recent approach is BSC installed a cyclone dust collector a couple years ago, and that's been running pretty much ever since then.

Our analytical approach is just standard rock chemistry, nothing terribly innovative. The ICPMS revolutionalized trace element analyses 20 years ago, or so, and other methods, some of which, you know, ferrous iron determinations, that technology is probably more than 100 years old. We’ve just started the lead isotope study to try to better characterize the component of atmospheric dust that may get underground. And, it looks like that could be a sensitive method to do that.

For the soluble fraction, we follow a procedure, a USGS procedure that was developed to look at the soluble fraction of salts in soil, and basically, it involves--the key part of it is there’s 20 to one ratio of leachate to solid, and the reason it’s so large is you want to try to minimize the possibility of dissolving anything that may be at the solubility limit in the leachate. And, basically, it’s, you know, put the sample in, measure the liquid and the solid, shake it up for a minute and let it stand for an hour, and decant the leachate.

Then, we can do this soluble anions and cations by isotope, iron chromatography, trace metals by ICPMS, and alkalinity by IC and titration.

To try to identify the salts has been much more difficult, the salt minerals. And, we know from other studies that atmospheric dust, a lot of times the soluble salt component appears as coatings on mineral grains. So, we’ve tried the SEM. Folks at Berkeley and Argonne, I’ve got samples to try to use the synchrotron, which is very focused x-ray beams, and that wasn't successful. And, we've also evaporated some leachates to dryness, and then done XRD on the leachates.

In terms of collecting samples underground, we went to--they didn't want us to use underground electrical power, so we had to go to a large battery operated vacuum cleaner that's got two 12 volt batteries in it, and it's interfaced with a stainless steel cyclone here that was developed by EPA for collecting dust samples to look at lead concentrations, and it worked very well. We were able to vacuum, you know, one or more square meters, and get a couple hundred, or so, grams of dust per sample site.

This is Dr. Leonard Nemark (phonetic) here showing his expertise in vacuuming tunnel walls.

Another technique, this is Dr. Brian Marshall using the brush and dust pan method off the canisters in the drift scale test. It was too tight to get the vacuum cleaner in there, so we had to resort to these more traditional methods of collecting. And, then, finally, BSC installed a cyclone collector, and it yields about 1 ½ grams of dust per sample per month. And, it was deployed at the south portal for quite a while, and now it's been moved over to the old batch plant, and I guess eventually we'd like to see it--we haven't collected very many samples here yet, but eventually, we'd like to see it move somewhere else. The problem is it takes four 40 volts of power, so you’re limited where you can put it.

The chronology of the dust work, I’ll try to run through this quickly, Los Alamos, the folks there were the first to study dust, and they were following behind the TBM in the cross drift, and their work was focused mostly on two things. One was a health issue, what sort of minerals were liberated by the TBM, and also there were various tests in the cross drift, this was a wet headed TBM, and they were looking at various types of surfactant for suppressing dust. So, they just did mineral identification, and mostly, as you might expect, it was silica, polymers, Feldspars, and they detected a little bit of zeolite, a little bit of clay. That was pretty much it. They did not find any areonite in the cross drift samples.

Our first collection was in February 2001. We did a quick run through the ESF and collected 27 samples. These were multigram samples, and we reported our initial results in September 2001. We decided we needed larger samples. We wanted to focus on the finer fractions, the silt size and clay size fraction, the less than 60 or 70 micron fraction. So, we needed larger samples for that so we could do a size classification, and then do analyses of the various sizes. And, I think there’s a supplementary slide in there that shows the variation with regard to grain size. So, this is where we got the big vacuum cleaner, the steel cyclone. We used that same procedure in 2003 to collect from the ECRB.

We went topside then and collected surface samples from these natural accumulations, and we were surprised that the dust samples that have accumulated at the surface are pretty depleted in soluble salts, and apparently there, the salts are washed out. The dust accumulates and the salts are washed out, probably down into the fractures in the rock.

And, so, then with BSC, we had a small campaign here. We tried to find areas where dust was protected from precipitation. We couldn't find a whole lot, but the attic of the SMF, and we found an old trailer and a missile silo liner, and the variability was really very large. Then, in April of 2006, Brian Marshall collected samples from the canisters in the drift scale test.

And, then, the cyclone was started in 2005, and we've done quite a few samples. There was an effort to keep a fair fraction of the samples at SMF for other studies, and other people have asked for those samples. I believe the state has asked for them, and the NRC has asked for some, so we have been getting 200 to 300 milligrams, that's just enough to do the soluble fraction. We can't do any bulk dust.

We tried to identify salt minerals. I mentioned that before. SEM, we found halite, sylvite, gypsum, natroalunite, dolomite and somehow or other I left calcite off this list, a few grains of pyrite, molybdenite, native sulfur, and metallic zinc. Unfortunately, you can't use that technique to identify nitrates.

We took some leachates, dried them, and then did XRD on the dried leachates, and the important thing here is this was the first indication we had that we probably do have ammonium salts.

So, let's look briefly at the underground dust. One important point is the underground dust is 90 to 95 percent ground up rock. And, at the same time, there's, on average, there's only about a half percent soluble salts. This is ESF dust. There's things that have been added to the dust that's not rock. Ferrous iron, which we think mostly was introduced as metallic iron, CO2 from calcite veins, organic and elemental carbon from a variety of sources, including abrasion of the conveyor belt, chloride from pore water, and other elements from pore water.

Trace elements that have been enriched over the rock are the metal elements here. So, again, nothing surprising there.

And, what I've tried to do is just summarize some of the key chemical parameters, and then for some, more detail is given in the supplemental slide. But, this is--I tried to calculate the amount of technogenic and biogenic material in a kilogram of average dust, and that’s based on taking the rock composition, which we know pretty well, and then just comparing it to the dust composition.

So, two columns here, ESF dust and ECRB dust, so we’ve added a lot of iron, this is grams per kilogram, or parts per thousand. Magnesium and calcium, which is coming from grinding up the calcite fracture fillings, they are more grindable than the rock itself. P_2O_5, we don’t know where that comes from. Chloride, fluoride, there’s fluoride in fracture minerals, calcium fluoride, CO_2 is in calcite, and organic carbon, a considerable amount of organic carbon has been added to the ground up rock to form the dust, and surprisingly, bound water, and the bound water is water that’s expelled at 900 degrees C, and then it’s collected, and the amount of water is determined. That’s just a standard method of doing bound water. All we can think of is there’s a component of clay and zeolites and maybe hydrated volcanic glass in the dust that’s yielding that water.

The next slide shows you’re average percent salt contents, and these things have, I didn’t put on the plus or minuses, but probably on the average of maybe a 20 to 30 percent coefficient of variation. The ESF has about half a percent salt and a fairly high nitrate to chloride ratio. ECRB has much less salt, and I think we attribute that to the fact that there was probably much less activity in the cross drift over time.

The surface samples are also low in salt. The protected samples, four samples, a wide range, and a wide range of nitrate to chloride ratio. The cyclone samples typically have 2.3 to 5.5 percent soluble salts, and consistently nitrate to chloride ratio of about 10. And, then, the regional study that the USGS has done under the direction of Merritt Reheis for the collectors, the traps around Yucca Mountain, they run about 13 percent soluble salts.

So, then, we moved, let's go to the heated dust, and the drift scale thermal test, you know, ran for about eight years or so. For a couple years, it was at a temperature of about 200 degrees C. There was a moderate amount of dust that collected over that time.

Next slide, please? This is the temperature profile from the temperature record from two canisters in the heater. It got up to 200 degrees for a couple of years.

So, the geochemistry relative to the average ESF dust, the bulk DST dust enriched in iron, both ferrous and ferric iron, magnesium, calcium, titanium, manganese, chloride, fluoride and CO_2. Enrichment in the calcium oxide and CO_2 has to be from the limestone aggregate that was used in the concrete liner that lined part of the alcove. And, we thought maybe there was carbonation under these conditions, but we cut thin sections of the concrete, the concrete cylinders that were in the test, and we saw no sign of carbonation. The leachates are enriched in those elements, and that's shown here.

On the next slide, what we've done is we analyzed the concrete with the aggregate, and then we have a bulk analyses of the dust. And, if you take the elements have to be mostly geogenic, and the average, you can make a mixture, which averages about .62 rhyolite and .38 concrete. And, then, you can use that to normalize the average DST dust. So, the dust is enriched in ferrous and ferric iron, and then these other elements by, you know, up to almost an order of magnitude and a half. And, again, these reflect the materials that were used in the drift. A lot of these metals are coming from steel. There's a lot of electrical cables, probably that contributed chlorine and fluorine in the dust.

This is just another way to show the same data. This way, here, the drift scale test dust is normalized against the average ESF dust. And, you can see some of those same enrichments, especially in iron, magnesium, and so on and so forth, CO2.

Now, we then leached the drift scale test dust off the heater canister, and this is what--well, I have to say surprised us. We saw this, we normalized this to average ESF dust, soluble fraction, average ESF dust. So, we see very significant differences. The drift scale test dust is depleted in magnesium and nitrate, enriched in some of the other elements. So, this was a puzzle, and this led us into taking some of our existing samples then from the underground, and also from the surface, and just conducting some scoping experiments to see what--you know, one of the questions was did this drift scale test dust never have nitrate, or was it somehow destroyed by the heating? So, that's the subject of the next few slides here.

And, what we did, we took aliquots of existing dust collections, carefully split into two samples each, and one sample, we analyzed both--we analyzed one sample as control at normal room temperature. We heated the other to 180 degrees C for two months. This is all at one atmosphere in open containers, no environmental controls at all, and analyzed them. So, we'll just see some examples here.

The first one is ESF dust, and the key points, there are three key points here. One, we lost ammonia, no doubt about that. We lost a consistent amount of nitrate, and we seem to have gained some organic acids.

And, we looked at the ECRB dust. Now, there, we lost nitrate. We didn't seem to lose ammonia, but we also gained organic acids.

And, then, finally, the last one is the cyclone dust, and there, we lost two orders of magnitude nitrate, a fair amount of ammonia, and gained organic acids. So, there’s a consistent pattern that emerged here.

HORNBERGER: How did you gain fluoride?

PETERMAN: Well, you know, that’s still a puzzle. I don’t know why we gained chlorine and fluoride there, unless there was something that was, you know, some insoluble phase that broke down at 180 degrees and became soluble. Now, I don’t think that’s likely to be fluoride, but this is the only one we saw that in.

So, one possibility--missing a slide, but that’s all right. We were thinking then that there was some sort of de-nitrification of the dust, possibly by a redox reaction maybe involving organic carbon. And, you know, nitrogen is a complex element, and it has four valent states. Nitrate is plus 5, nitrite is plus 3. Of course gaseous nitrogen 0, and ammonium is minus 3. So, we were thinking that, you know, if you have an electron donor there, then you can reduce the nitrate to some form that goes off, perhaps nitrogen gas. And, you need to go from nitrate to N2, the reaction I have here, which isn’t shown, but you need basically five--or ten electrons. So, you need something to donate electrons. One possibility is organic material, and we seem to have plenty there.

So, this is just to sum up, this is nitrate on the Y axis versus chloride on the X axis, and the drift scale test samples, bulk samples, are shown as the green dots. They were depleted in nitrate relative to everything else. The gray circles are atmospheric dust, and they lost two orders of magnitude. The chlorides stayed pretty much the same. They lost two orders of magnitude nitrate. And, all of them lost nitrate to some extent, an order of magnitude, or so.

So, what can we conclude? Well, I think first of all, the underground dust has less than 1 percent soluble salts, typically with nitrate to chloride ratios of 1 to 10. We can see what elements have been added, or what oxides have been added to that by the tunneling activities.

Surface salts, surface dust have lost most of their salts through leaching by precipitation. Atmospheric dust has the highest soluble salt content, and the highest nitrate to chloride ratios. We don't have bulk analyses of the atmospheric dust yet because of the small sample size.

We observed low nitrate in the drift scale dust relative to the ESF dust, and we can simulate that by heating samples for two months at 180 degrees C. And, we think probably lose the nitrate by redox reactions, possibly involving organic carbon. Now, there may also be some loss through the decomposition of ammonium salts.

The bottom line that people are interested in, this de-nitrification has resulted in reduction of nitrate to chloride ratios by one to two orders of magnitude.

And, that's it. Everything else is supplemental. So, I'm happy to try to answer questions.

GARRICK: Thure, why don't you lead off.

CERLING: Cerling, Board.

Thanks, Zell. That was very informative. One of the things that struck me, if you could go to Slide 7, just had to do with the amount of dust that we're getting, and in this case, I mean, if we take that upper limit of 400 grams of dust per sample, and assume that maybe that's from four square meters, that ends up being about 10 milligrams per centimeter squared. Now, I was just wondering how that squares with the values that Dave Sassani was talking about earlier, if Dave is still here, because that 1 percent dust content, that would be 100 micrograms per centimeter squared.

PETERMAN: Yeah, we did, on this particular collection, we tried to measure--we did measure the area of vacuum, and of course it wasn't 100 percent efficient, but we have some photographs, and we got them pretty clean. And, I think the average--well, I figured the dust had accumulated for six years, and the average was about 20, I did it in grams per square meter, 20 grams per square meter, that's considerably higher than the atmospheric dust flux outside is about 5 to 10. The salt flux outside is higher because the salt concentration is higher. So, here, about a half percent, so about .1 grams of salt per square meter. I think the numbers are probably as good as you can get using that approach.

SASSANI: This is Dave Sassani with the lead laboratory.

Thure, I don’t have off the top of my head the actual values, but these are higher values because in these tunnels, the ventilation is running directly, and the air flow is higher. And, in the other tunnels in the drift analysis that was done, the effect of the turnout and the baffles on the front end of the emplacement drifts causes velocity drops. And, so, you have dust settling over a distance. And, then, what’s carried into the drift through the baffles is a smaller mass. And, so, what’s used in terms of constraining the mass of dust deposited on the very first waste package, which gets the largest amount of dust, which drops off down the drift, is a calculation that accounts for the change in the velocity profiles, and the dust dropping out of the system in the different locations.

So, then, the actual mass of dust deposited on the first waste package is used to constrain all other masses of dust, it’s shown to be the largest amount. But, off the top of my head, I don’t remember the numbers right away. I can take a look and see if I can pull those up for you.

CERLING: Yeah, just as I recall, but I don’t remember exactly what the numbers were, which we saw, I don’t know, two or three meetings ago, just seems to me that these numbers are higher, but I just--

SASSANI: Yes, these would be higher, and these are probably also higher than the dust deposition rate in the drift scale test also for similar reasons.

CERLING: Thanks.

PETERMAN: The 20 is clay and silt size, and it’s pretty uniform throughout the ESF. We didn’t see a large variability. I think it was 20 plus or minus 7, or something like that, excluding one sample very close to the north portal.

CERLING: And, I guess following on on that, if you, I guess it would be interesting to know kind of what the variability is, what is sort of the maximum dust deposition rate that you get, and sort of what the distribution of values is. I’m sure you haven’t got it on the top of your head. But, that would be an interesting thing to know.

PETERMAN: Well, excluding that one sample, it was 20 plus or minus 7, that’s one standard deviation.

GARRICK: David?

DUQUETTE: Duquette, Board.

Zell, I’ve got a couple of questions of you, and a question of someone else, based on this, as you might imagine. Slide 30, please.

If I look at this date just for all of the heated samples, it looks like most of the data clusters around about a .1 ratio of nitrate to chloride, with the exception of the ESF is a little bit higher, on a log scale, and a few data points that are somewhat lower. These are single data points taken after two months at 180 degrees Celsius.

PETERMAN: Yes.

DUQUETTE: Do you think the ratios would change even more if I went for a year instead of two months?

PETERMAN: Well, you know, that’s a very good question, and it’s something we want to address this year. We just finished our test plan, and hopefully it will be approved very shortly, so, we can get to work and do some of these experiments. But, it was suggested also that maybe we should do a sequential or heating, you know, and go for less--try to characterize the time, nitrate relationship.

DUQUETTE: Right, the kinetics.

PETERMAN: Right, yes.

DUQUETTE: What is your personal opinion?

PETERMAN: I don’t really know if I have one. I mean, this was kind of a surprise, and I just don’t know.

DUQUETTE: Okay. The next question really is related to this. Is Doug Wall still here?

WALL: Oh, yeah.

DUQUETTE: Doug, would you come up to the microphone? I’ll be the professor for just a minute, and say have you paid any attention?

WALL: I think I’ve paid enough attention, yes.

DUQUETTE: Are you taking this data into consideration with the experiments that you’re proposing?

WALL: Certainly.

GARRICK: Good answer.

DUQUETTE: How?

WALL: Well, I guess the question that I’m anticipating is if this change in chemistry occurs, how does that affect the screening argument for the dust deliquescent scenario, and I--

DUQUETTE: I’m glad you asked my question.

WALL: Yeah. Well, you know, it wasn’t too hard to figure that one out. I think first off, what we have to keep in mind is that the dust compositions do not necessarily equate to the brine chemistries under the deliquescent scenario. So, at elevated temperatures, we’re still going to have to have a minimum nitrate composition in order to get deliquescence. So, for the higher temperature regimes where corrosion under a deliquescent environment would seem to be the most probable, we’re still going to be limited in the types of aggressiveness of chemistries we can have.

And, then, if we go down to lower and lower temperatures, and start to ask the question could we get deliquescence of one of these more aggressive brines, I think it’s a little premature to try to analyze this on the fly.

We have to get a very good understanding of what that environment would actually be, and it's certainly more complex than just a pure chloride environment at that point. There's still other things in the brine components, so we would have to take a look at all that.

DUQUETTE: I agree. But, it's just that I think this is the first time we've seen this data. It's obviously very interesting, given the discussion we had this morning.

WALL: Sure. And, I think that goes a long way towards the point of having a multi-faceted approach to screening out the localized corrosion. And, so, having parts in that decision tree that are independent of the aggressiveness of the brine composition certainly helps strengthen that.

DUQUETTE: Thank you.

GARRICK: Bill?

MURPHY: Bill Murphy. I have a question about the difference between the composition of the dust at the ground surface, and those underground. There was a substantial difference, and much more nitrate at the ground surface, which is not surprising to me because there's biology there and fertilizers, and whatever. But, I'm curious, underground over a long period of time, there are other sources of dust that you haven't seen. For example, there will be dehydration of the rock, and precipitation of salts as the waters evaporate, and there will be earthquakes from time to time that shake things up and grind up the fracture coatings and stir up dust to some extent. Can you speculate, based on your data, how the dust composition in the interior of the repository might evolve over time, over hundreds of years?

PETERMAN: Well, first of all, we see no evidence that earthquakes have ground up the fractured minerals at all. I mean, that was used as an argument at one point for saying the underground effects of past earthquakes underground has been pretty minimal with regard to upsetting the rock system. Those are pristine things, can show that they've been, some of them have been there for 10 million years.

I would expect, you know, there's two phases here. There's the active phase of the repository, and there's going to be any activity underground is going to generate a lot of dust. I have no idea how the emplacement drifts are going to be cleaned, or how the canisters are going to be cleaned. But, I would expect some--I think we can see that, the difference between the ESF dust and the ECRB dust. It's quite different. ECRB dust is--the silicate fraction is pure rhyolite. You know, there are no other rock types in the cross drift. It's more complicated in the ESF. And, so, yeah, I would think things would evolve.

I would think the soluble salt fraction over time would tend to increase, at least during the construction and emplacement phase. I don't know what else to expect. The surface dust bulk composition is quite different than the underground dust, but now we’re seeing the influence of carbonate dust from the--such as Bear Mountain, and much more complicated, the bulk compositions.

MURPHY: Okay, I have another question on a completely different subject. The significance of the dust goes beyond corrosion issues, and I wonder if you can--if you have used your data to look at issues such as chloride mass balances or infiltration rates, and issues of the chlorine 36 chloride ratio based on the chloride contents of the dust?

PETERMAN: We haven’t done that. We’ve talked about, at various times, about doing chlorine 36, but that seems to be a closed chapter and I’m not sure we want to reopen it. We have another dataset that’s totally on pore water, so that’s the data to use for chloride mass balance, not the dust data.

MURPHY: Do you think that the chloride in the pore waters comes initially from dust deposition at the ground surface?

PETERMAN: Some of it does, certainly. I mean, we demonstrated that by collecting surface dust and then seeing that it’s very impoverished in soluble salt. So, those salts have to get washed down the cracks, and eventually they get moved through the mountain.

MURPHY: Thank you.

GARRICK: Did you want to make a comment?

SASSANI: Yes. This is David Sassani with the lead lab.

Thure, the numbers that we were getting are about a few hundred grams per square meter. So, about twice what they were collecting per square meter, and there's sampling, but that's for 50 years of ventilation as opposed to the ESF being operated for about ten years, with ventilation running. So, it's about a factor of five. But, again, that decreases because we consider the turnouts as the air flow that comes through the turnouts. It drops out some of the dust because of the velocity change through the baffle system. And, then, some of the dust is actually carried, all the very fines, are carried all the way through the drift. But, those analyses are in the dust deliquescence AMR, and the bases for where those numbers come from are all laid out in there. But, yes, it is a different number.

I wanted to offer a little bit of a discussion in terms of Bill's comment and question. And, this is, I think, what we discussed this morning a little bit, some of the uncertainty in the evolution of these materials. In these experiments, we see a fairly rapid change in the nitrate content, possibly due to interaction with the organics, nitrate reaction with organics, those tend to be fast reactions. We do expect chloride to evolve at higher temperatures over the longer times, and, so, some of these trends may swing back up a little longer period of time, we're not sure about that. But, in fact, again, all of these are removing these constituents that would cause them to create high temperature deliquescent brines.

But, in terms of the evolution of dust through time, it's probably just a shorter thermal period that matters, because once we're down, back below the boiling temperature, we're looking more at the seepage environments at that time.

GARRICK: Ron?

LATANISION: Latanision, Board.

An observation, and then a question. I mean, I think this data on Figure 30 does add emphasis to the importance of the conversation we had this morning. This is a corollary comment to what Dave said a few moments ago. What I think one would want to do is to determine what ratio of nitrate to chloride would provide inhibition as a function of temperature over the range of, I don't recall, the interested range, and then determine whether or not from these kinds of experiments whether or not you would be able to achieve that ratio. I think those are very doable experiments, and my hope and the observation I made, with these facilities as being constructed at Sandia, I think there's an opportunity to look at this very rigorously. And, my hope would be that that would take some priority, and rise in the list of planned experiments over the coming year or two. The second is a question. We can see what the data tells us in terms of the depletion of nitrate. How can we assess the mechanism for that depletion? Is it gasification of the nitrate at a faster rate than chloride’s interaction with carbonation materials. If we can understand what’s leading to that observation is it conceivable that we could somehow work that to our advantage in terms of a repository environment? Could we reverse that trend somehow. What can we learn about this that would give us some guidance in terms of what’s possible?

PETERMAN: These experiments were pretty, you know, pretty simple, open dish, heat them up to 180 for two months. So, you know one thing we talked about is how could we sample and collect the gases, and analyze that. That’s one thing to do. That would certainly help understand the process, and maybe as discussed this morning, you know, and we’ve talked to some of the people at Sandia about this and possibly collaborating with them on that sort of thing. We’re just not quite there yet, because our effort for the past couple of months has gone and getting the test plan in place.

LATANISION: Well, I would certainly encourage that. I think that sounds like a very productive way to build.

WALL: Yeah, I think so.

GARRICK: Andy?

KADAK: Yes, Kadak, Board.

Could you put up a chart to help over there? What should he use in his analysis of deliquescence in terms of the materials that might be found in the dust in the repository.

PETERMAN: The soluble salts.

KADAK: I'm just asking what do you think the experimenter should have as a dust sample so he can test what happens at the--

PETERMAN: Well, I think in the long-term, and this may be addressed as a question I didn't answer very well, in the long-term, you know, there's going to be the dust generated underground, but in the long term, there's going to be a flux of atmospheric dust come in, and it's very different. So you need to look at, and that's one reason for doing the lead isotopes, those whole promise for assessing that external component.

KADAK: You've got to help this guy out. I mean, clearly, you know, the atmospheric dust is quite different from the dust underground.

PETERMAN: Right.

KADAK: And, you'll be ventilating for 50, 100 years, who knows how long, and that will create its own dust. Now, we're looking at whether or not deliquescence causes localized corrosion based on the minerals that are found in this dust. How do you answer that question? What is in the dust.

PETERMAN: I think if there's enough resources, you do both N members, and then figure out what really might be there.

KADAK: But, you're the person who can more or less tell them what is there.

PETERMAN: Well, I think the atmospheric, the cyclone is, I think, a good representation of atmospheric dust.

KADAK: Okay. So, that's one set of dusts.

PETERMAN: That's one N member. All right.

KADAK: You mix that with--

PETERMAN: With the underground dust, the ESF dust.

KADAK: So, you take a mixture of all that, and that's the dust?

PETERMAN: Yeah. At the same time, I think it's important to note that, you know, all the dust isn't bad. Most of it is ground up rock. If there's any acid generated, it can be quickly neutralized by the very finely ground rock. There's also calcite is enriched in the dust by at least an order of magnitude, and that increases the acid neutralization capability of the dust significantly.

KADAK: Is there like an AMR on this discussion you just shared with us?

PETERMAN: I don't know.

KADAK: What we're trying to do is what is the dust, what is the environment, and whether deliquescence will affect these waste packages.

SASSANI: Well, the dust deliquescence AMR does cover a lot of the discussions of the dust compositions and their potential evolution, and what ranges look like can develop. But, those ranges tend to be large when you look at the processes that can be involved, particularly post-closure with the heating of the dust. And, as Zell is pointing out, if there are reactions going on where perhaps the organic component is dominating a very short-term reaction with another trait, it’s unclear then, you know, in terms of a nitrate to chloride ratio, what exactly do you use, because this might be very appropriate for this couple of months experiment, but over a ten year period, perhaps the chloride evolves out of the dust sample, out of the salts also and you slide back to a higher nitrate to chloride ratio.

So, I think the way the experimental approach is going is trying to get after parameterization of various nitrate to chloride ratios, what they do to the corrosion process, and we’re going to see how that fits in with our understanding of what these could possibly be. The mass balance aspects are a little bit easier to address, because the atmospheric dust has a much higher content of the soluble salts. It’s about 10, 13 percent. And, so we have used those salt contents for the dust to bound things like the amount of chloride, the amount of fluid that can develop from it. But, those are easier questions to get after as opposed to what exactly is the right target composition, because that’s a relatively broad answer at this point.

PETERMAN: I have a table summarizing the average composition of soluble salts. Okay, 39? There is. All right, so you can see the differences there, the SF has much more salt. The surface dust is pretty depleted. Cyclone dust is enriched, and there are scale tests, alcove dust is quite different in many respects, roughly equal amounts of ammonium and nitrate to start with. And, we lost a significant fraction of that. Well, this is the dust after it was heated by the thermal test.

GARRICK: Yes, Mark?

ABKOWITZ: Okay, this is someone who has no background in this, but has been listening to this discussion.

To the lay person, the way this comes out of the wash is that the Department of Energy has very little understanding of what the dust environment is really going to be like over a long period of time in the repository. And, because they don’t know what it’s going to be, they really can’t judge what the possibility of deliquescence causing localized corrosion might be, and, therefore, that’s justification to FEP it out of the analysis. Do I get that straight?

PETERMAN: You don’t want me to answer that. I don’t know the answer to it.

SASSANI: This is Dave Sassani with the lead lab. I guess I’ll try to answer that one.

No, I’m obviously not being clear. We have a fairly large amount of data on dusts. We have a very large amount of analyses, which are out in the cutting edge of geochemical understanding of how to analyze these systems, and we’ve done an enormous number of looks at it from different perspectives of what could possibly happen, what could the dust be like, what could the--how long could they persist, would they persist, how would they evolve, and we have approached that from a series of steps of asking will we have any deliquescent type dust. Is it possible?

If the answer to that is not no, we go to the next step and analyze okay, if we have dust, it can deliquesce at high temperatures. Does it look like those brines can actually persist for any length of time that would be meaningful to do any kind of corrosion? And, if the answer to that is not no, we go to the next step, and we go all the way down that series of five stages, all the way to well, even if you could initiate corrosion, and even if it can start to propagate, do we expect that localized corrosion from these brines could possibly penetrate the outer Alloy 22 layer. And, the answer to that is no, based on very fundamental mass balance aspects.

So, I think we have a very good understanding of the possible dust compositions, but they are broad and there are a lot of processes that can wiggle them around a bit. But, we have evaluated that entire range of possibilities.

ABKOWITZ: Let me just add one other lay person comment, and then I will turn it back over to the experts.

My understanding is a large body of that work, as in a ten year period, most of that has been done under the assumption of a calcium chloride environment, which you now acknowledge is probably not the proper environment?

SASSANI: Calcium chloride environment has been one that's been investigated as for corrosion aspects, and it's been investigated because it is always a possible environment, if you look at some of the compositions that exist. And, so, we had analyses that look in detail at it, and say no, we're not going to have these because of the processes that are involved, and because of the evolution of the system, calcium chloride brines are not a concern.

But, yeah, as we've gone along through the program, we've looked at different possible extreme conditions to make sure that they aren't an issue. And, as we can line those up and say these are not an issue, yes, they are no longer an issue.

GARRICK: Ron, and then Thure?

LATANISION: Latanision, Board.

Just to add a comment, you know, what may begin with deliquescence, if it begins, and let’s assume it stifles, I mean, I’m still not convinced that stifling is a real phenomenon, but let’s just suppose it does stifle during the rise part of that thermal transient, if there is some initiation event that occurs during that period, it doesn’t heal at any point during its history. And, so, on the decline side of the transient, when cool down occurs, and you may have some seepage, for example, that site is an active site.

So, I’m concerned that in the TSPA, seepage induced localized corrosion is considered a potentially viable scenario, but deliquescence induced localized corrosion is not. That doesn’t hang together to me.

SASSANI: Well, I can’t answer the actual question about the corrosion aspects of the site, but--

LATANISION: I think Doug is going to answer for you.

SASSANI: I’ll let Doug handle that.

WALL: Doug Wall, lead lab.

So, Ron, you’re talking about, let me just rephrase this scenario, of your coming down in temperature, there has been some sort of an event under a deliquescent scenario, and a concern is that you get into a seepage case where that would then take off under seepage conditions, and continue to propagate.

LATANISION: Yes. Assuming that some phenomenon like stifling, as it has been discussed at these meetings, let's assume stifling occurs during the rise transient.

WALL: Right. You're assuming it does or does not occur?

LATANISION: No, I'm assuming it will. I'm just going to say for the sake of argument, let's assume--

WALL: Will the process turn itself back on, given that case?

LATANISION: Yes.

WALL: Well, I think if you look at this, and you can choose different paths to come in and talk about this question, but if you think about it in a way we do in modeling space, in terms of looking at deliquescence based corrosion initiation, and looking at seepage based corrosion initiation, and in the seepage based case, if we get to the point where we have an environment capable of initiating, we just propagate to failure. So, we don't take credit for the outer barrier at that point.

And, the seepage model is designed to make sure that it doesn't under estimate the propensity for localized corrosion, and it's based on the ER crev parameter, the repassivation potential parameter, and that parameter is based on the assumption that you have a propagating crevice and that you have conditions where that will have to turn off, that is, you’re at a potential below that value.

LATANISION: Yes, Latanision, Board.

But, it must also assume, however, that all the initiation events occurred during the seepage event. And, all I’m suggesting is that it’s conceivable, even if you accept for the moment that stifling may occur, even if you accept that, some of the initiation event, and, therefore, the density of those localized geometry, will be a function of what happened during the rise part of the thermal transient, not the cool down section.

WALL: So, you’re suggesting that we would have a state sufficient to translate into further damage under seepage conditions?

LATANISION: You said it better than I could, yes.

WALL: Right. Well, I mean, you know, based on the entire analysis of the dust deliquescence, we just don’t believe we’re going to end up in that condition. If you’re referring to then the step five of the analysis argument, if we were totally hanging our hat on that and saying we’re going to accept the fact that we populate the waste package outer barrier with small disbursed corrosion sites, is there evidence to suggest that those will then re-initiate under seepage conditions? I believe that the work we have done under seepage, simulating seepage conditions with inundated tests does not favor that moving forward. I mean, we're still testing under an artificial geometry that's meant to mimic that very condition, and that's what our models are based on.

LATANISION: Let me understand what you've just said. Have you done experiments to simulate in seepage conditions, a situation where you have a pre-existing localized cell, and you're saying it won't re-initiate?

WALL: I'm saying that that is incorporated into the way the experiments are run to generate the data that's used as the basis for the seepage model. Because you're running that test and you're sweeping potential down, you've created as bad of a localized environment as we're going to be able to generate on this sample, with an experiment that's designed to do that, and then looking for where it shuts off. So, I really think it's been conceptually integrated into the model for under seepage conditions.

LATANISION: Okay, you may be right, Doug, and I'm willing to accept that answer for the moment. But, I would suggest, Mr. Chairman, that we have a--we include a discussion of the work being done on seepage induced localized corrosion at our next meeting, because I'm not aware of the experimental work or the basis that Doug has just described. I accept your answer, Doug, but let's see if we can fill that gap.

GARRICK: Yes. Okay.

LATANISION: Thank you.

GARRICK: Okay, Thure?

CERLING: Cerling, Board.

Just one question looking at this figure here. I'm just kind of puzzled, have you actually tried, I mean, for instance, the surface sample, just in my mental calculations would suggest that that actually is really close to calcite saturation, so I'm just puzzled as a person interested in water chemistry just about the chemistry of some of these waters, because like the cyclone sample in ESF, it looked to me like they'd be supersaturated with calcite by a factor of 20 or more. So, I'm just puzzled about that. We can talk about it later, or something, but I'm just kind of surprised that one could actually get calcium and bicarbonate, you know, those concentrations in the same fluid. It just looks very puzzling to me.

PETERMAN: Well, we haven't done any of that--those sort of calculations.

CERLING: So, I'm just wondering if it--so, were these on a single--so, all of these analyses were on a single dissolution of the dust as it were, just a single leach, or were there several different?

PETERMAN: No, these are averages and they were single leaches. You know, 20 to 1 leachate to solid, agitated for a minute and allowed to stand for an hour, and then the leachate was decanted.

CERLING: It might all wash out if one looked at the individual analyses. It's just kind of surprising. That's all.

PETERMAN: Yeah.

GARRICK: Here's a comment here.

MARSHALL: Brian Marshall at USGS.

Thure, these are normalized to kilogram of solid. These are not milligrams per kilogram.

CERLING: Okay. Okay.

GARRICK: David Diodato, you have a question?

DIODATO: Yes, thanks. Diodato, Staff.

Zell, thanks first of all for your continuing efforts to develop an empirical basis for our understanding of the site itself.

With regards to Slide 30, I'm also interested in maybe then going backwards to a theoretical understanding. So, the question is do you have any plans or thoughts about developing speciation calculations to support this experiment, to investigate it that way?

PETERMAN: The leaching experiments?

DIODATO: Well, these heating experiments, right, doing a speciation calculation?

PETERMAN: Yeah, we haven't done that yet, no. I mean, yes, we could do that, sure, just haven't done it.

DIODATO: Thanks.

GARRICK: Bruce?

KIRSTEIN: Kirstein, Staff.

Do you have any plans to measure the total carbon in future samples, or these samples, so we have an idea of what we're talking about, and carbon to nitrate ratio that starts out?

PETERMAN: We do measure total carbon by combustion. I think it's around 1,350 degrees C, and then the evolved CO2 is measured by some sort of infrared detector. Then, we measure carbonate carbon by titration, and the difference then is often called organic carbon. But, it could also include elemental carbon. I mean, that's the technique we use.

KIRSTEIN: Okay.

GARRICK: Would it be possible to characterize a family of specifications of dust in such a way that you could make some sort of judgment on the basis of the experimental work and other supporting evidence as to which spec might be the most representative?

PETERMAN: Well, you can do a statistical evaluation like John's orders initially said we would do, and we've done that. So, you know, you can describe the distribution and the central tendency, and all that. That's about as far as we’ve carried it.

GARRICK: Now, is that input to the experimental program?

PETERMAN: All of these data that I talked about today, except maybe some of the last leaching studies, they’re all in the TDMS. We’re up to date, so it’s available. All the data are available.

GARRICK: David?

DUQUETTE: Just one brief comment. Duquette, Board.

And, maybe I’m misinterpreting what Dave said, and perhaps what you said, if I take a look at the gray data, which is the cyclone data, I think from what Dave said, that was what we would expect to be coming into the drift with time, and would probably represent the majority of the dust that might be in the drift with time. I think that’s what, Dave, am I misinterpreting what you said?

SASSANI: No, you’re correct. The atmospheric component will be brought in with the ventilation.

DUQUETTE: So, then, the next question I have, and maybe I can put some of this to bed right away, if I take a look at the gray crosses, which are what happens after you heat that dust, will that dust deliquesce?

PETERMAN: I don’t know. Dave, do you know?

SASSANI: Right off the top of my head, I don’t know, although I would suspect that it could.

DUQUETTE: Eutectic?

SASSANI: Right, it would have to be eutectic, but without sitting down and looking at the complete specific composition, because everything we're looking at is a relative one, I mean, we've lost nitrate, we've gained some organics, if we just look at the nitrate and chloride, we haven't lost that much chloride, but it would depend then on the sodium/calcium ratios.

DUQUETTE: The reasons for asking are obvious. If it doesn't deliquesce, then we shouldn't be concerned. Then, maybe the problem goes away. If it does deliquesce, that's where we ought to be doing the experiments. That seems to me to be a fairly simple way of looking at it.

SASSANI: Since we have the dust apparently, since it's been collected, it seems to me we can decide whether it's going to deliquesce or not.

HARDIN: This is Ernie Hardin, Sandia.

If you take a nitrate salt, let's say sodium nitrate, and a chloride salt, let's say sodium chloride, you throw the solids together in some sort of random relative abundance, you will get a multi-soft deliquescent phase at elevated temperature. It's determined by the intensive characteristics of those salts rather than by the amounts that you threw in in the beaker. So, if we take this, and we remove part of the nitrate, if there's any nitrate left, you will still get some deliquescence. It’s just that the volume of the deliquescent brine is now restricted by the availability of the nitrate.

DUQUETTE: Right. But, in this case, we have the dust. It seems to me we should be able to decide whether it deliquesces--

HARDIN: Well, the answer to your question is unless you can quantitatively eliminate nitrate, you will get a deliquescent brine under the same conditions, let’s say 180 degrees C, for that multi--for salt assemblage. You just get a lot less of it.

DUQUETTE: No, no, but for this dust, this dust that has been collected has been heated, has been reacted, now we have the composition--can we do that? Can we decide whether this dust will deliquesce and how much?

HARDIN: I’m sure it’s within Zell’s capabilities to give you a number on that, yeah.

DUQUETTE: I mean, it might answer--

KADAK: But, that’s the whole point. That is the question. Well, you’ve got the dust; right? He’s got the experimental facility. He can recall it, decoupage it, whatever he calls it--what is it?

PETERMAN: Decorate.

KADAK: Decorate it. And, do the test. I mean, with all the other stuff that you mentioned that could be neutralizing agents--

PETERMAN: Well, you know, it’s not like we haven’t communicated. We have communicated with Sandia, Charles Bryant and other people there, and our emphasis so far this year is getting this “S” plant in place so we can actually get back and do some work.

GARRICK: Okay, Ron?

LATANISION: I think this line of questioning is really important, and Andy said it in a very friendly way, and it should be friendly. The fact is it can be done. You know, it’s conceivable this is not a problem. We just simply don’t know. But, the experiments can be done, and they can be definitive, and it seems to me they can either answer the question up or down, we have a problem or you don’t have a problem. I think that’s what everybody who is concerned about localized corrosion wants to know. And, if it’s not a problem, then it’s done, you put it to bed.

WALL: Yes, Doug Wall, Sandia Labs.

We certainly have in our current plans using collected dust as an input to this experiment. I think, though, from my perspective, looking at the corrosion behavior, I think that might be a little bit overly optimistic to think we could have one dust collection, and to know that that’s a definitive sample. The corrosion program is a bit broader than that, and the objective is really to determine how this corrosion process will initiate, under what conditions, and then what its path looks like past that. I think testing in these real collected dusts is an important part of that study, but I don’t believe that can be the definitive point at which time you say we don’t need any further information.

I mean, if we then find out that, you know, you age it for ten years and you get a slightly different chemistry, now that data point we collected has to be supplemented. So, I think having a broader program is really the way we’re going, and this information will certainly be incorporated in the testing to include real dust assemblages, moving forward.

KADAK: Could I make a suggestion? You have atmospheric dust. You have dust that’s in the repository regime with concretes and all your wires, which you will likely have in the real repository. You also know that it can get hot down there. So, you have three tests, atmospheric dust, the dust in the repository, elevated temperature, normal temperature, four tests. It is not conclusive, but it sure gives people who may have a question about this some confidence that you’re at least sampling the right dust and not creating something artificial that you think is the right dust. That’s all I’m suggesting.

WALL: Yeah, Doug Wall, Sandia Labs.

Certainly starting from those dust compositions, I think, as you said, would improve confidence that we're testing in an environment that's been collected from the actual source where this will come from in the future.

I still think we have questions, though, even doing that, which is a very valid concept, but then, you know, what is the total thermal history that that dust sample has seen, and does that affect the composition that will be on the waste packages.

GARRICK: This is what bothers me, though. The Performance Assessment is supposed to be probabilistic based analysis. You have a series of specifications, each of which has a different level of evidence. It seems to me that no, you shouldn't pick just one and go with it, but you should pick the ones that have been measured and for which there's evidence that this would be the way it is in the repository, and weight the thing, weight them and incorporate them into your analysis. I just don't know why there is such a hesitancy to embrace the information you have, and put it in your analysis, or put it in your experimental program.

WALL: Doug Wall, Sandia Labs.

I think these are very valid inputs into the test program. I would caution, however, that some of the information I shared this morning showed some of the accomplishments we'd made in terms of capability for running these experiments. Decorating a sample with a salt assemblage is not the same as decorating a sample with a collected dust assemblage. There are technical hurdles to be overcome in doing any of these things, and they sound straightforward maybe when we're throwing around ideas, but I'm taking this information back and trying to incorporate it into this test program. All good ideas, we do have technical restrictions, and just capability restrictions on what we're actually able to do. We'll take this information forward. We're going to do our best to represent these assemblages as appropriately as we can.

I'd also caution, though, if we take a real dust assemblage and we run an experiment and we get a null result, we leave ourselves open to criticism, saying that, well, you hit a dust assemblage where the brine droplets were in contact with the sample. If we take analog systems, like a pure multi-salt assemblage, we basically alleviate those criticisms later on in the experimental program. So, we're really looking at a balance between looking at the real dust, the collected things, and what is achievable and defensible from a laboratory perspective.

So, I appreciate all the inputs, and we're doing our best.

GARRICK: Yes. Okay, well, thank you. It's just been a very interesting discussion, and I think Ron is correct, that there needs to be some sort of a sequel to this to see if we can't converge, because we have been kicking this issue around for a long, long time. At least we seem to have an experimental perspective being developed. There's field measurements that have been made, and if we can see a connection between the two, and something that answers the story about what happens to the nitrates, for example, that would be very encouraging.

All right, we're behind schedule, but it's been worthwhile, in my judgment. Let's take a 15 minute break, and continue and continue.

(Whereupon, a brief recess was taken.)

GARRICK: As a kind of a postscript to our deliquescent corrosion discussion, I guess I would like somebody to tell me what would be the radiation dose consequences if there was a small chance of some localized corrosion due to deliquescence. What the hell difference does it make?

DUQUETTE: Be careful. George, you've got the--

HORNBERGER: That's the difference.

GARRICK: Well, we've got another very interesting subject that's also something that we've been pushing for answers on for a long time, particularly with respect to the source term. And, Pat Brady is going to give us a heads up on where we are, and give us some encouraging news, I hear.

BRADY: Okay. I'll spend the next 20 minutes talking about the water balance model.

The water balance model is a subcomponent of the larger performance margin analysis, the PMA. The PMA is a quasi independent endeavor to do two things. One, determine if the TSPA is conservative, and, two, to quantify how conservative it is.

The source term portion of the TSPA was put together largely by people working for Ernie Hardin and myself. The water balance model is the PMA independent assessment of that model. And, that was put together by Yifeng Wang and co-workers. Yifeng, wave your hand. He's the gentleman right back there.

Now, so, I'm in a peculiar position here. While describing and defending the TSPA source term approach, I'm going to emphasize and highlight the independent analysis of that effort by my peer, Yifeng Wang. Gorbadal (phonetic) once said whenever a peer of mine succeeds, a little piece inside of me dies. Well, that's not going to happen today, because although Yifeng and his group have succeeded wonderfully at uncovering new features of the source term, they have also confirmed and quantified the conservatisms in the existing model, and, therefore, strengthened TSPA.

So, this is the trend of the talk. I've already covered the objectives of the PMA, and I will go into greater detail on that. I'll cover how we do the TSPA, well, how water balances are treated in the TSPA, and I'll show you some results.

Let’s be clear about the performance margin analysis does and does not do. It quantifies the extent to which conservatisms in the TSPA model individually and collectively over estimate the total mean annual dose relative to the model projections of the PMA, and it confirms that when propagated through the TSPA model, the evaluated conservatisms are indeed conservative with respect to the total system performance measures.

Next slide? Okay, the TSPA model. Now, the TSPA model focuses on the radionuclides, and how they’re moved out of a breached waste package, and what they’re chemical forms of and what their fluxes are. It does not explicitly balance water as it comes in, though it explicitly recognizes that you aren’t going to get movement of the radionuclides out unless there’s water movement in.

The water balance model looks at the same problem, considers a lot of the same processes we consider in TSPA, except does it from the water side.

Now, both of these models have, they consider, three fluxes. You have a waste package that breaches, you have an advective flux in, there is a diffusive flux of water in, there are reactions that consume water inside the package, and then there’s water that advects out, goes into the invert, into the UZ, and on down.

When we add up those three--when we consider those three--well, there’s actually four fluxes, there’s advection in, diffusion in, advection out, and then there’s the interior negative flux for the consumption of water by corrosion products.

The existing TSPA considers these in the following fashion. Given a corrosion scenario, and throughout this talk, I’m going to deal only with the nominal scenario, not talking about when a volcano goes up, or the seismic scenario. This is you have patches opening up on the surface, water going in. In the existing TSPA model, we look at a flux of water onto the package--oh, yeah, also I’m assuming the drip shield is gone. These are highly stylized calculations that have as their objective an understanding of what happens to the water once it gets on and into the waste package.

All right, the existing TSPA model takes one to a thousand liters per package per year, drops them on the package. Some of this splits off, goes off--well, some of it doesn’t hit a patch. The water that hits a patch, goes into the waste form. You will see in a moment that the water balance model treats that differently. We consider the fact that you don’t corrode a patch away. You leave something there behind, namely corrosion products, and there is an intrinsic resistance to fluid flow that those things provide.

All right, so that's the advective flux difference. The diffusive flux, the way that the existing TSPA gets water into the package as a vapor is, okay, corrosion of the internals occurs. The corrosion produces corrosion products. They have a finite surface area. That surface area will absorb water from--basically, the relative humidity of the drift is assumed to be the relative humidity inside the waste package. The corrosion products will equilibrate with that relative humidity. The water that stacks up on the ironoxyhydroxides, you then account for it--well, you then calculate the water saturation inside the package by adding up all the water that comes in, that equilibrates with the external relative humidity, and ends up on the surfaces. I can never think about that.

The corrosion products themselves are hydrated. So, water had to be there to form the corrosion products, and then they absorb water. Okay? So, this is one of those rough edges in the existing TSPA. The chemical reaction, the internal H2O term that I talked about. The way it's treated in the in-package chemistry model where we try to assess what's the chemistry of the fluids that are equilibrating with the fuel rods, and picking up radionuclides, and then going down into the invert, those calculations track the amount of water in the vessel.

All right, so there's a certain amount of water that goes in. A lot of that water gets consumed to take, for example, UO2 and form schoepite, to take low carbon steel and form gertite. We track those H2O molecules and as they are consumed in the course of chemical reactions and the ionic strength goes up, because the remaining salts are concentrated, we track that, and that shows up in the handoffs to the solubility models, to the--and, on downstream.

But, there’s no explicit mass balance done elsewhere, where you take that water subtraction, the water that went in in the advection, and the water that might have diffused in, there’s no place in the existing TSPA where all of those things are added up. That’s what this model is going to do.

So, what the water balance model here does is we take the water, consider what happens to it when it hits the waste package, what fraction of the water can make it through the corrosion product filled patches, and how much goes off inside. We do a calculation of how much water can diffuse in. We do a calculation of the water saturation inside the package, and that involves some numerical calculations of the water potential, which is a function of the surface area of the corrosion products inside, and the amount of hydroscopic salts that come from the waste form as well. And, we have to do this all in the face of a temperature gradient that goes up at first, and then goes down. So, let me show you how it's done.

Next one? These are all the equations. Starting off at the top, we have, again, Q is the water flux. The water flux inside the 21 PWR package here, it's a difference of--well, you sum up the amount that advects and diffuses in, subtract out what gets consumed by reaction, and that's our water balance.

All right, so advection, dealing with that one first, this is probably the most important one for this talk. We consider the same fluxes that the TSPA considers, 1 to 1,000 liters per year, no drip shield to prevent it hitting the package. It lands here, a fraction of it drifts off. A fraction of it, though, will advect through a corrosion product filled breach. The quantity of that is calculated by all these equations.

All right, there's the advective flux in. It's equal to a dripping flux, multiplied by, this is a fraction of one, it's a function of the angle at which the impingement occurs. That's theta. You can see a bunch of other terms. There's the porosity, which we vary between .35 and .5. Let's see, we've got the hydraulic conductivity for the corrosion product mass. We use 10 to the minus 10, and 10 to the minus 14 meters squared. That corresponds to what you'd see for a silty sand. Obviously, that's an uncertain number.

That's why we have to vary it. H3, well, that's the--H cubed, H is the thickness of the water layer on top. You can imagine water landing on the very top of the package, it's more likely you'll pool water there. That's the head that drives water down through the corrosion patch.

KADAK: Could you describe the condition of the waste package at this point in time? Is it all patched, or is it just thinned, or what is it?

BRADY: The way we set it up is--well, actually, I'm going to cover it in the next slide, if that's okay. But, in a nutshell, it's not thin. We have discrete patches, and they're opening up. I'll show you in a second. It will become clear.

All right, so, that's the advective flux. The vapor diffusion flux here, there's an area, there's a tortuosity term that points up the fact that we're calculating diffusion of water vapor through a corrosion product filled area. That ultimately ties into the relative humidity difference from the drift to the inside of the package.

The advection out, it's equal to it. There's an area term, multiplied by--this is the hydraulic conductivity, which is a function of the water saturation. There are some cross-cutting terms that are--through here. We have to calculate out the relative humidity, the water potential, which is a function of matrix potential. That’s the particle aspect, and then there’s also an osmotic potential that comes from the fact that the fuel and the steels contribute the hydroscopic salts.

Let me make one more point here, and then I’ll show you how the calculation is done. The third flux, the reaction flux, that consumes water, the two primary reactions are listed here. The 21 PWR package is, for all intents and purposes, about 36,000 kilograms of 316 stainless steel, plus roughly 30,000 kilograms of UO2. When those things are oxidized and hydrated, they go to respectively ironoxyhydroxides, oxides and schoepite. Note that you use water I both reactions.

If you take the surface area of all of those components in the package, multiply them by the mean rates, you can estimate the amount of water that gets consumed per year if all of those surfaces were wetted. And, the numbers come up between 30 to 60 moles of water per year. Consider there’s roughly 50 moles of water per liter. The maximum amount of water that can get consumed through reaction with the internals of the waste package is roughly two liters. That’s the most you can get.

KADAK: How about zirconium?

BRADY: The zirconium is neglected in this reaction. Okay? There’s a lot of zirconium there as the clad. The expectation is that that’s going to oxidize to ZRO2, ZROH4, but we don’t consider that.

LATANISION: Sorry, let me understand what you just said. You’re assuming that you have 36,000 kilograms of 316 stainless steel, and you’re converting what fraction of that to the reaction products?

BRADY: Well, the--what’s more important is I have a certain 10 to the X meter squared surface area of the stainless steel, which I multiply by 1.2, I multiply it by the corrosion rate.

LATANISION: Okay. So, this would be the corrosion rate of 316?

BRADY: Yes.

LATANISION: At these conditions, it’s probably passive, so your corrosion rate is very low; right?

BRADY: Right.

LATANISION: The corrosion mode is uniform corrosion, assuming passivity?

BRADY: Yes.

LATANISION: Okay.

BRADY: You’re head of me. We consider a range between .001 to 1.57 microns per year. So, the .001 is kind of--

LATANISION: Okay.

KADAK: The two liters is an absolute number, or is that a rate?

BRADY: That is a rate. Two liters of water per years.

Okay, so, you will note that in all of these equations, water shows up as a term. So, what is done is--would you go to the next slide? We do a Monte Carlo analysis with the inputs being the thermal--we know the waste package temperatures over time, we know relative humidity is in the drift over time, we have a model for patch area growth and coalescence, which I'll hit in just a second, and we sample on degradation rates for the steel, and said there's the fuel. If you take the fuel and glass rates that are used in the TSPA model, the ranges are varied by roughly a factor of ten. We vary the porosity of the corrosion product patch between .35 and .5.

Hydrologic conductivity, I already told you the range of that, and the dripping flux. The rivulet thickness, the water film thickness on top of the package is varied between .1 and 3 millimeters, this 30 realizations to try to predict, try to get a handle on what happens to the water flux over 10,000 years into the package.

We've got four pictures here. Three of them are boundary conditions, this one, this one, this one. These are results. These are thermal loads for commercial spent nuclear fuel and co-disposal. These are temperature profiles for each of the packages, in drift relative humidities. This is how we do the stylized patch opening. We start off and assume that at 500 years after emplacement, a patch begins to open. We don't say it is localized corrosion, general corrosion. It's probably the latter. But, we then take the maximum Alloy 22 general corrosion rate, calculate how long it would take for all of the Alloy 22 on the waste package to corrode away, and then divide that by years, so that we can basically open up our patches.

That number right in there, the red line in there, that's the mean value. It gives--all the Alloy 22 goes away in a couple hundred thousand years. Again, the objective here is not to speculate about corrosion mechanisms, but about what happens to what water inside the package, once water makes it through a hole.

Now, the first thing, and to take away from this talk, is covered in this slide right here. This plots the advective influx, the drip is splitting, and then the advection through the corrosion products in the patch over time. This is, unfortunately, my slides, this one and the next unit, sometimes they are in meters per year, sometimes they are in liters per year. So, you'll have to divide or multiply by a thousand sometimes to compare them.

But, we take a dripping flux that goes between a cubic meter per year, a thousand liters per year, and then one-one thousandth of that, and we vary all of those other parameters, and then calculate how much water actually gets into the package versus how much goes off to the side.

And, what you will see is the fraction is, at most, a percent. What this is pointing at is that there is a substantial resistance that is imparted by the corrosion product filling of the patches. There is uncertainty there, again, because we don't really know how to predict that hydraulic conductivity ahead of time. But, the big point is what lands on the package probably isn't going to go into the package.

Next slide? Well, actually, let's go back here. Two points. Let me reemphasize how the calculation is done. We've got a thermal profile and we've got a relative humidity profile. We've got an Alloy 22 rate, and we march forward in time opening up patches, raising the temperature, and dropping it, dropping water through the patches and watching where it goes.

The second point here is the highest dripping flux that we see is roughly 2 liters per year. Recall, that's in the ballpark for what corrosion product formation will erase.

KADAK: It's dripping into the package?

BRADY: Dripping into the package. This is just dripping. We haven't considered any chemical reactions. We haven't considered any diffusion.

DUQUETTE: What if there were no corrosion products?

BRADY: You mean if--

DUQUETTE: If there’s localized corrosion, there will be--all the corrosion products will be--

BRADY: I’m sorry, I didn’t the last part.

DUQUETTE: If there’s localized corrosion, all the corrosion products should be soluble.

BRADY: Okay.

DUQUETTE: What happens?

BRADY: Okay, at that point--all right, so that would suggest that our model--that would differ with the way our model is set. I think the results wouldn’t change, though, because once you get through the Alloy 22, you’ve got roughly two, three inches worth of stainless steel, which is corroding faster than your Alloy 22. So, if you don’t have corrosion products from the first one, you’ll certainly have it from the second.

HORNBERGER: Why wouldn’t you have corrosion?

DUQUETTE: If you have pitting, hydrolysis in the pit eliminates, solubilizes the corrosion products inside of the pit. It’s actually quite acid, the pH is getting down to minus 1, minus .1, minus 1, if there’s localized corrosion.

LATANISION: Latanision, Board.

Let me also amplify, as I asked a few moments ago, if it’s passive, the corrosion rate is very low, and, in fact, you know, you’d be producing a corrosion product that might be a few atom layers thick. It’s not a thick passive film. So, you know, the mode of corrosion makes a difference. And, what Dave is saying, and I agree with, if it's localized corrosion, you have soluble corrosion products, you're not, you know, particularly filling the volume of the internals of the waste package. If it's passive current density on the stainless steel, once again, you're not--it wouldn't be the equivalent of oxidizing carbon steel, which produced voluminous corrosion products.

So, the mode of corrosion makes a difference, and I think we're both trying to understand, all trying to understand exactly what mode is involved here.

BRADY: Well, I agree with you that you'd like to know exactly what's happening there. But, if you're corroding--well, if it's not corroding at all, then there's really no breach--I mean, how do you get passive or non-existent corrosion--well, in that case, you don't get water in the package, unless there--

LATANISION: Well, again, Latanision, Board.

To return to John's question at the beginning of this session, even if there's a breach, if the environment that sees the internals, the stainless steel, for example, and other components, is inducing passive state, you know, you may not have a--you may never release radionuclides. I mean, it may be a non-event.

BRADY: Okay, I'm sorry, I misunderstood your question.

The performance margin analysis looks at many of the sources of conservatism. That’s not one that we’ve looked at. The boundary condition we set here is that water is going to get into the package, and it is going to react with the steel.

LATANISION: It’s going to react with the C-22 or the stainless steel?

BRADY: There’s going to be a corrosion--there will be corrosion products that will form.

All right, next slide, please. Okay, these are the other predictions for this calculation. A couple points to be made here. First of all, if you look up there, there’s the vapor fluxes that are calculated. There’s a bit of scatter. They can be positive or negative, depending on if there’s in package aberration. Typically, though, the value is on the order of one to two liters per year.

What’s surprising about this is that diffusion, vapor diffusion of water into the package, it looks like it’s in the same ballpark as the advective flux, which was somewhat of a surprise.

KADAK: Could you explain to me the condition of this waste package to allow for this vapor diffusion of Alloy 22?

BRADY: Okay. Alloy 22, it is, let’s see if I can do this right. Stylized scenario is this is Alloy 22. We open up a patch, and in between there, there are corrosion products either from the Alloy 22 or from the underlying steel in between here, and they are about 3 centimeters thick, the original thickness of the Alloy 22.

KADAK: There's diffusion through the corrosion patch?

BRADY: Yes. Through the corrosion products that occupy the place where the Alloy 22--

KADAK: So, there's no real hole. Is that a real hole?

BRADY: No. No. The existing TSPA model assumes a hole. We assume the corrosion product filled hole.

All right, if you take a look down at the bottom, you see the advective flow out of the package that we calculate for CSNF and co-disposal. The maximum values, over there, it's about a liter a year. Over here, it's about a factor of five less. Again, these are with dripping fluxes on the order of 1,000 liters per year. So, what that reflects, what it largely reflects is the corrosion product presence.

Let's go to the next slide. Okay, this has a couple of implications for the chemistry inside the package, the fact that you don't get as much water in and you get less water out. The first thing is that the water saturations that are calculated are routinely low, .3, .5 here. Co-disposal, it gets higher because there's more of an osmotic pull because there are more hydroscopic salts produced in the degradation reaction of the fuels.

The other feature of a water starved interior waste package is that the ionic strengths tend to be very high. Most of these--there's a thermal pulse right here that you pull out a lot of water, and, so, the ionic strengths really go high. But, they flatline out about one to two molar.

Recall that colloid, the threshold at which colloids become destabilized from solution, it varies depending on the colloid, but it's typically a small fraction of this. The upshot here is that the ionic strengths in the package are going to be such that colloids won't go out.

Now, the last slide, please? These are preliminary results from the PMA. The PMA is much larger than the water balance model. The preliminary results here that we get for this stylized situation is that most importantly, the advective outflows are going to be very small, roughly half a liter per year. The ionic strengths are going to be high enough to destabilize the colloids. Water saturations are going to be low.

Now, typically, if this were an academic talk, at the end, there would be a thing at the bottom that had acknowledgements where you'd point to the folks that gave you the cash, say who helped do the real work in the lab, and so on. Well, obviously DOE paid for all of this, so I'll leave that out. But, the inspiration for a lot of this work came out of a different project altogether, namely the OSTI YMP S&T Source Term Program.

This was originally begun by Margaret Chu, and the source term was carried forth by Rod Ewing and Mark Peters. A lot of the conservatisms that we've targeted in the PMA got their birth in the source term S&T program. Abe and Dave are both part of that. Kay Hulen (phonetic) and myself were, too, as was Yifeng Wang, and I suspect we'll hear more about these that, although they are PMA now, they were S&T things before.

Questions?

GARRICK: Go ahead.

MURPHY: Bill Murphy. I think these are interesting possibilities that corrosion products will divert water, and that alteration phases will consume water, and that could lead to increased ionic strengths. And, I've seen talks before that also addressed the volume increase associated with corrosion products that take up space and fill up the drifts eventually. But, it seems--my question is that it seems that this has to be a transient thing. You can't forever add more water to the waste package than is coming out, because eventually it fills up, or some kind of steady state is established. Your conclusion is that the advective outflows are less than a liter per year compared to your advective inflow which was assumed to be somewhat greater than that; right?

BRADY: Yes.

MURPHY: That can't go on indefinitely.

BRADY: No, I haven't violated the laws of mass balance. If you go back to the--okay, this one right here was done just looking at the advective influx, and I said a couple of liters per year, just to give you all an idea of the volumes we were talking about. Okay, these ones are steady state. These are ones where you take the advective flux in. You're constantly doing a running mass balance for the diffusion in, for the advection out, and, so, these things, if you'd go to the next one, and the way you can see it is the water saturations, those water saturations are steady state. So, there is a mass balance there. It's just that the very first slide, I was giving you the range. We're adding and subtracting numbers on the order of a couple meters.

The consumption rates for the maximum consumption rate is a couple liters, but typically, it's a lot less. But, that gets you in the ballpark. So, you're right, it does get to a steady state, but the steady state configuration that we envisioned is that patches are going to be filled with corrosion products, and that's going to limit the amount of water than can get in.

HORNBERGER: But, do you ever use up all the fuel? That's the question.

Put the black chart on and tell me when you get to the point where it's all used up, the 36,000 kilograms or--

BRADY: Well, not in 10,000 years, which is the length of the simulations. So, as I recall, depending on which corrosion rate you use, the, I'm going to say 50,000 years is what the estimate for the stainless steel lasting is. Depending on the clad coverage for the fuel rods, those go in somewhere between a thousand to ten, but don't quote me on that number. So, what we are talking about is--I think I've answered your question, Bill, or have I?

MURPHY: Well enough.

BRADY: Okay.

GARRICK: Ron?

LATANISION: Latanision, Board.

I think what you're doing here is really very important. At the end of the day, you know, we've been quibbling a lot this afternoon about localized corrosion, and it is important, if in fact it breaches the waste package and allows water into the package. But, the bottom line is well, whatever transpires beyond that release radionuclides, and that's going to be, at least in my perspective, maybe I got this wrong in terms of what you're trying to do, but if you're getting water into the package, you somehow have to ask about the modes of corrosion that are occurring internally, and, in particular, the mode of corrosion that may be occurring on the zircaloy clad fuel.

So, I mean, at the end of the day, are we assuming that fuel is perfectly clad? Is it going to be breached by corrosion? How are we going to release radionuclides, or did I miss something? Well, just walk me through it, because I think this is really important, and I don't understand it.

BRADY: Okay. Now, at the beginning, I said that we do those considerations in TSPA, where we say okay, what the cladding coverage is, and right now, it's zero. So, we get everything. We always neglect the clad hydration reaction. But, we always focus on the radionuclides. This one is only on the water balance. It only deals with the interaction with the steels--

LATANISION: Stainless steels?

BRADY: Stainless steels, to give you water consumption and conversion to gertite, and some other thing with uranium. There is no solubility calculation here. There is no sorption calculation here.

ARNOLD: It had to breach the clad to get at the UO2.

LATANISION: That's the point.

BRADY: That's right, yeah. But, if you take out the UO2, all right, so you take away roughly half of your water consumption rate. If the cladding works perfectly, well, yeah, there won't be a radionuclide release rate. But, how it affects the water balance is what we focus on here. It would probably take away roughly half to two-thirds of your water consumption rate. Again, this model is not a radionuclide transport model. It's a water balance model.

LATANISION: No, I understand. Then, I guess someone needs to go to the next step, because this isn't a full answer to the ultimate question, and that is are we in danger of releasing radionuclides to the environment?

BRADY: Right. It wasn't intended to be--

LATANISION: No, I know this wasn't. I understand. But, where--is the next step something, who's dealing with that?

BRADY: And, Peter might embellish or correct me. The TSPA/LA, as I described it is going to go in. This argument and arguments like it are going to go in with it as well to show that the water balancing that is done in the TSPA is conservative.

LATANISION: Okay.

BRADY: And, it will give a ballpark figure about how much. But, as far as are we going to do, you know, the absolutely anatomically correct model? Dr. Swift?

LATANISION: Well, if we're getting a corrosive medium into the package, and if it has access to the fuel, which is clad with zircaloy, is the fuel susceptible to being breached somehow, which would be the ultimate concern? How do we deal with that?

SWIFT: This is Peter Swift from the lead lab. And, the emphasis in the TSPA used to licensing case, has to be on all the different scenarios combined. And, one of the things we realized, we realized this one quite a few years ago, was that although clad is robust in a static environment, it could be subject to breakage and ground motion. And, our cladding experts looked at the ground motions it would take to break cladding, the basic drop tests, and concluded that actually within several tens of thousands of years, we're likely to have had ground motions that will have broken the cladding.

That discouraged us from wanting to put a lot of effort and to trying to take credit for cladding in the licensing case. Certainly, it's there, and in the first 10,000 years, probably does play an important role, but that and the shift of emphasis towards the million years, we decided that we would not try to pursue that one further.

So, what Pat's starting with here, or what Yifeng is starting with, is a mixed bag of boundary conditions supplied from the TSPA. TSPA didn't use cladding. They didn't attempt that one either. Does that answer?

LATANISION: That helps. I mean, that puts a perspective on how you're approaching it. But, I mean, I think the ultimate question is if the package is breached, are we concerned that we are going to release radionuclides? I guess the answer is yes, given what you've just said.

SWIFT; Yeah.

LATANISION: At some stage, we will.

SWIFT: My perspective on that would be will the releases be acceptable. And, that's our licensing case is to argue yes, they will be.

LATANISION: Okay.

GARRICK: Are you assuming, Peter, that the crumbled up zirconium doesn't have any impact on the subsequent chemical reactions?

SWIFT: Actually, I'm going to put that one back to Pat.

BRADY: Yes, the expectation is the zirconium, again, it goes to ZRO2, or ZROH4, depending on how much water is about, but it's a relatively benign, it doesn't affect the in package chemistry, and neglect it altogether, because the passive layer will be sitting there, and it may hydrate, and, so, it might pick up some water molecules.

GARRICK: And, what happens when the geo side of materials start entering into the process?

BRADY: That's the--it's not the result of this calculation, but the in package chemistry calculation shows that--it turns out the geo component, I assume you mean the seepage, the seepage compositions really don't affect what the composition of the fluid coming out is, which is a good thing, because a lot of the radionuclides are at least soluble around neutral pH. You take a fluid, anything from J-13 well water, concentrated J-13 well water, sea water, and you put it in contact with UO2 stainless steel, if you dissolve the U02, you end up going to the solubility minimum of U02, depending on the PC02. Well, that's about pH 6 ½. Maybe go up to 7 ½ if the PC02 is lower. But, it's pretty close to neutral.

The point here is the package itself has a very powerful intrinsic asset and base neutralizing capacity, centered on about pH 7. So, it's hard to get you off that beam. So, the solubilities of a lot of the radionuclides, they're going to be fairly minimal, with the exception of the co-disposal packages, because the co-disposal packages have the alkali producing glasses.

GARRICK: George, and then David, and then Andy.

HORNBERGER: Pat, so this is a--I'm trying to think about how you might parse the margins here, because David wants me to believe that you might have the corrosion without the corrosion products, so that you could have a hole with no corrosion products. But, that wasn't totally your analysis. And, my question is how much of your margin depends upon the advection through the corrosion products, because you still have the consumption of water and all the other stuff.

BRADY: Right. A lot of it, because you see the flux going--okay, the existing TSPA has a dripping flux, one to a thousand, and if that hits a patch, it goes in.

HORNBERGER: Is it one to a thousand uniform?

BRADY: Dr. Wang says yes.

HARDIN: This is Ernie Hardin, Sandia. The seepage distribution function is a bell curve, and, so, it’s log normal. You can think of it that way.

HORNBERGER: Log normal--

HARDIN: Seems to be in an immediate range.

HORNBERGER: Log normal meaning that the median is down closer to the one liter than it is to the thousand.

HARDIN: It means probably ten.

HORNBERGER: So, the question is if you’re going to use up two liters per year in the corrosion, and your median is two liters per year, you’re getting a lot just from the in package use of the water.

BRADY: Right. But, if you bag the full one thousand, the two is not going to buy anything. So, I’d have to answer your question, the corrosion product, yeah, it’s an important part of it.

GARRICK: David?

DUQUETTE: Duquette, Board.

I’m trying to understand your model. Can we go back to the black slide? I have a couple of problems with--what it looks to me like is you’ve got water coming into the canister, reacting with the stainless steel inner liner, then again reacting with whatever stainless steel might be in the casements on the fuel bundles, and then you ignore the zirconium.

BRADY: Right.

DUQUETTE: And, then, you have it interact with the U02; is that correct? So that the little equation down on the bottom right-hand corner here where it says 36,400 kilograms and 316 stainless steel, and so on and so forth?

BRADY: Yes.

DUQUETTE: If you have uniform corrosion of the stainless steel, the assumption we made in the corrosion world is in the time that we normally use passive metals, there is a constant corrosion rate. It’s about at the what we call the passive current density or passive corrosion rate. We know that with time, that that passive film thickens and that the corrosion rate is actually decreased, probably approaching zero, because that film is very difficult to breach. And, so, if you make the assumption that you’ve got uniform corrosion at some constant rate, and you’re using that to eat up water, which is what you seem to be doing, it’s a water balance, then your assumption is way off because in a say 1000 year period, the consumption of stainless steel is going to approach zero if you don’t have any localized corrosion.

BRADY: Okay.

DUQUETTE: That’s also true for the stainless steel casings around the bundles, and it’s also true for the zirconia, which will probably be passive as well.

So, I’m not arguing--I guess I’m arguing that your model is somewhat less conservative, but just in a water balance process, I don’t think you can take credit for the internals of the canister, forgetting about the alloy 22, I don’t think you can take credit for the stainless steel, the zirconium, or even the fuel using up water, because I think in each case, they will build a film on them that will reduce the consumption of water rate considerably with time, and that at some point, you will reach a point where all the water coming in comes out, and it may or may not have any radionuclides in it, but all the water coming in will come out, and it won’t be consumed at all, and your model changes considerably, it seems to me, in terms of time. Am I missing something?

BRADY: No, I agree with everything you’ve said, and that’s part of the reason, and Yifeng, you might want to step up and talk about the source of corrosion rates. That’s why we used down to the really low value, .00l microns per year, so if the passivated, if passivation goes to zero, you’re right, there will be no water consumed at all.

If the cladding never fails, there will be no water consumed by those reactions. Of course, if the cladding never fails, then we win anyway.

DUQUETTE: I agree with that.

BRADY: So, my question for you then is once you get to the steady state, you've got a very thick passive surface layer on the stainless steel. Is the rate--does it really go to zero, or the rate it goes to, is it going to be above the absolute lowest value we use, which is .001 microns per year?

DUQUETTE: I don't know. Once again, we're stuck with what is the environment that this stainless steel has seen. But, if you assume, I mean, we do know that passive films thicken with time, and that the rate of corrosion underneath the passive film is, some people say logarithmic, some say that it's parabolic. There are different models for that when the film is super thin. But, we do know that in--you are modeling for a 10 or 20 or 30 year period of using a stainless steel for example, we do a polarization curve and extract a passive current density and use that as sort of a constant rate over the period that you're going to be using it.

We do also know, though, for a fact that with passive films, they do thicken with time, and as they thicken, corrosion rates drop to extremely low levels. And, I just think that if your model is using the stainless steel to use up the water, if you will, that it probably won't do that.

BRADY: Well, we don't go down this path by saying ah, we're going to quantify how stainless steel uses up the water. I mean, we have to consider the fact that yeah, the passive layers are going to form, but these things may get bounced around, and the passive layers may get disrupted due to seismic motion.

We know for a fact we're going to end up with really highly concentrated solutions from time to time. The way we do this in the TSPA/LA then is we use a very, very wide range of stainless steel degradation rates. We've done the same thing here, because we are unable to predict unambiguously down the road that yeah, passive layer is going to form, it's going to stop it all.

GARRICK: Getting back to the radionuclide source term, if the process proceeds as Dr. Duquette just articulated, and that leads to the conservation of the water, then there are no radionuclides; right?

BRADY: Well, I think he's putting words in my mouth.

HORNBERGER: Well, you're dealing with water.

LATANISION: Latanision, Board.

On the contrary, if in fact Dave's scenario is correct, and I would subscribe to that, it seems to me that the water you're putting in has complete access to radionuclides because we're assuming that the clad is not protective. So, what you're flushing in, goes out, but it's carrying radionuclides with it.

GARRICK: I thought the films were protecting it.

LATANISION: That’s not--no that--

DUQUETTE: If the assumption is that the fuel is completely encapsulated by passive metals, that’s right, you will never get any transport. But, if there’s a breach anywhere that allows access of water to the fuel, then all of the water coming in will meet some fuel and will be able to carry radionuclides out. I’m not suggesting that would happen, because I don’t know what models are made on the assumption for direct access of water that’s in the canister to fuel, not to the components of fuel, but fuel itself. If they can’t contact the fuel, if the fuel is completely encapsulated, we should all go home.

GARRICK: Then, what starts happening when all this then comes in contact with the media, the geological media?

BRADY: You mean once it gets out the bottom? Well, then, you have the combination of colloids and dissolved radionuclides heading down, and that’s the subject of another talk, John.

GARRICK: Yeah, afraid so. But, I’m still curious about the mineralization processes that take place. You’re right, it is another talk, but we would sure like to hear that one.

BRADY: Hope they have a sunny day in Las Vegas.

GARRICK: Right. Andy, you had a--

KADAK: Yes, I have a couple of questions.

If your analysis is right, I just want to try to interpret it, you’re saying that a half a liter, less than a half a liter per year of water will escape the waste package, under your analysis, carrying whatever it carries from the fuel; is that how I’m supposed to interpret that?

BRADY: Yes.

KADAK: Okay.

BRADY: Not my analysis. I presented the TSPA, and I’ve been presenting Yifeng Wang’s, and, so, this is our--

KADAK: But, that’s the analysis.

BRADY: This is the lead lab analysis, yes.

KADAK: The lead lab’s analysis is half a liter per package per year at some point in time?

BRADY: Yes.

KADAK: Okay. If this water balance would be applied to the criticality calculations in a wet environment, what could we say about how much water accumulates or doesn’t accumulate from this waste package?

BRADY: You’d have to ask someone besides--here comes someone.

HARDIN: Hardin, Sandia.

We have a water flooding calculation report that we prepared for the criticality people. It presents a conceptual model for what happens hydrologically inside the waste package. It’s not a numerical prediction, but it’s a progression of degradation steps, where unless you have a gross breach in the outer barriers, and unless that gross breach is subjected to seepage and it goes directly in a hole, then maybe you can get some flooding. But, if it’s a cracked package outer barrier, or other types of minor damage, you’ll get slow degradation of the stainless and the other materials inside the package, and we don’t believe you will see flooding. Does that answer your question?

KADAK: I’m just trying to be sure that your analysis is somewhat consistent with that. Is it, or is it not?

HARDIN: I think it is in concept, but this goes beyond what we did for that application in terms of quantitative predictions.

KADAK: What is your mechanism for drainage in this waste package if you don’t apply this kind of an approach?

HARDIN: I mean, to answer that question, I would have to carefully parse out the different cases for degradation of the outer barrier. You can have cracks, you can have breaches due to localized corrosion, and the spatial distribution of those flaws is important if you want to talk about drainage. For the case of cracking, if you have cracks on the top, you’ll have probably cracks in the, very likely cracks in the bottom. There is a drainage pathway. We have three independent vessels, if you will. We have the outer barrier, separated by a one to five meter gap from the inner vessel, separated by a gap from the TAD canister wall. Those are three different gaps that afford bypass pathways whereby water that leaks in, can find a crack and leak out without entering the inner container where the fuel is.

KADAK: The last question. Is there a passivation of uranium, as is there passivation of metal?

DUQUETTE: If it were pure uranium, yes.

KADAK: Well, I'm talking about U02.

DUQUETTE: It reacts with water and other--

GARRICK: Better move your mike a little closer.

DUQUETTE: It reacts with water and other insoluble product. That's that last reaction he has up there with oxygen H20 and U02, and form the schoepite. And, so, it's also a precipitate. It doesn't solubilize very easily in neutral water.

KADAK: I mean, can we make that same argument after a certain point, the fuel itself does not have access to water?

DUQUETTE: What I don't know about, because I'm not an expert at all in the corrosion of the fuel, although I know something about it, is it's not uranium we're concerned about getting into the environment. It's the sister products that are contained in the uranium. I don't know what happens to those from a corrosion point of view. It's the nasty things that get out, and uranium is probably never going to get out of the package. But, some of the other things that are contained in the fuel bundle, are the things that I think we’re concerned about.

GARRICK: We’re still looking for what I would call an integrated source term model.

KADAK: Is there anybody on the project that knows how this fuel reacts with water, spent fuel?

KNOWLES: This is Kathryn Knowles. I’m the PA manager. Those analyses are largely done by John Wagner of Oak Ridge National Laboratories, who is our criticality manager, and he is not here today.

KADAK: Well, we’re not talking criticality. I’m talking dissolution or passivation of fuel pellets in a water environment.

BRADY: To answer your question, if it goes to schoepite, and there’s some cladding there, since the schoepite has a higher specific volume, it can fill a pit in the cladding, and thereby passivate it. There are other reactions if it formed to uranium silicate. That might passivate it. But, these things each depend on other things happening, like, one, there being enough available silica inside the package. There’s a whole lot of uranium, not much silica coming in. So, there are passivation pathways, and we have considered them in the spent fuel degradation AMR. We don’t feel there’s something we can put our hat on as prevailing over the long-term.

Although Dave was right, yes, schoepite is reasonably insoluble, it's not completely insoluble. And, so, there's the potential for it dissolving away, uncovering stuff beneath it and leading to this steady bleed of uranium from the fuel into solution.

GARRICK: Any other questions? Ron?

LATANISION: Latanision. Following on Dave's comments a few moments ago, if the water which is entering the package is not restricted by corrosion product, and, therefore, is likely to exit, and if in the spirit of your model, you would like to find some way to essentially immobilize the water so that it wouldn't flow out, could you not conceive of adding something inside the waste package that when exposed to water, would react to produce a voluminous product of some kind that could, in fact, immobilize the water when hydrated? I mean, this is really extreme, I suppose, but if you added steel wool, or something like that, that would react to produce a lot of iron oxide corrosion product.

BRADY: No, it's a good point. In fact, we did have steel wool. We had high surface area, low carbon steel.

LATANISION: Yeah, steel wool.

BRADY: Yeah, but that is in the past now. We don't--

LATANISION: Okay.

BRADY: At other times, people have considered aluminum shock because it's a straight conversion to gibbsite (phonetic), or some other hydrated form.

LATANISION: Yes.

BRADY: Depleted uranium is another candidate.

LATANISION: Yeah, sure.

BRADY: That's been pushed over the years.

DUQUETTE: Bentonite might work, too.

BRADY: Yeah, but although I agree with you, I think, Ron, it's too late in the day for that sort of thing. It's just a thought.

GARRICK: Abe has a comment.

BRADY: If the stainless steel does not corrode, then that does take away a lot.

VAN LUIK: And, under S&T, we actually had a getters program that was looking at exactly this kind of issue. But, I wanted to make sure that you're not left with the impression that the release rate model for specific radionuclides in the source term is not based on experimental data. We did a lot of experiments at PNL and ANL on actual spent fuel to look at the oxidation of U02 and how the oxidation, even though you form secondary phases, that oxidation is like peeling the onion on the U02, and it releases the dissolved materials in it.

And, I think we have a very good basis for the modeling that we have done, although just like everything else we do, we made conservative decisions along the way to make sure that we didn't under estimate the release of the

Actinide and the fission product content in the spent fuel. So, we do have a basis for that. I thought the implication a minute ago was that we were just blithely saying everything goes into solution. It doesn’t.

GARRICK: All right. Well, any other questions, comments. David Diodato of the Staff?

DIODATO: Diodato, Staff.

Pat, thanks for an interesting and challenging presentation. You know, it’s potentially significant thinking here, so I commend you for that.

I was wondering about the package itself, this 36,000 kilograms. Does that have a TAD in it? Is that TADed or not?

BRADY: Yes, that’s with the TAD.

DIODATO: That’s with the TAD, yeah, so--

BRADY: Let me, again, I do the difficult presentations, but the difficult thinking on this was Yifeng Wang’s.

DIODATO: You were a good team there, yeah. So, the surface area, in terms of I kind of try to think of reactivity in surface areas, the corrosion people know better than I about this, but does that surface area change in your model over time, evolve?

BRADY: In both this model and in the TSPA model, it does not, because we don’t know how to model it. Now, the place in particular it shows up in the TSPA model is it shows up in the in package chemistry, and it shows up in the EBS-RTA. The surface area that we start off with, we stay with.

DIODATO: Thank you.

GARRICK: Any other questions, comments, or what have you?

(No response.)

GARRICK: Thanks a lot, Pat.

All right, well, I think we've done pretty well considering. Now, we've come to the point in the agenda that is a very important one. It's the public comment period. So far, I have time requested for one person to talk to us. That's Dr. Jacob Paz. And, if there's others, I wish they would notify me.

PAZ: You wonder, you haven't seen me here for nine months. What's happened here, I went from Exon, from Sin City to Church City to repent my Yucca Mountain sin, but I cannot repent.

I have actually three comments. The first one is on plan for the long-term corrosion testing and recent results. The second paragraph conclusion, test results support the conclusion that Alloy 22 will not undergo localization corrosion under the deliquescent conditions in the presence of three and four salt, et cetera.

I have been brought to the attention of the committee about a year or a year and a half ago that they should include sulfate. If you're going back to chemistry and you add sulfuric acid to nitric acid, you're getting an oxidation. And, it's a repeated and not using the sulfate which has been reported in another paper present in the mountains. And, this is a very serious scientific error.

Second, I have a little question, when I read the report, S&N and GAS, the fact of high temperature of the repository for a thousand years, and subsequently, the cooling effect will increase the fracture, therefore, the infiltration rate will increase. This should be taken into account.

Last, I'm going to write a letter to the Board and requesting the Board to address the issue of risk assessment. Metal first, and metal to metals, and the combination. I will send you citation of the law which mandates the Department of Energy and also EPA, and this has not been, and this will be a stumbling block, and if any license application will come, I will very clearly come and state it here is the law, here is the regulation. You failed to do it.

It doesn't matter who does it. If the Department of Energy does not want to take responsibility and to file an accurate risk, they will be at fault.

That's it. Thank you.

GARRICK: Thank you.

Are there any other comments? Any particularly public comments? Where in the heck is Steve and Judy?

TREICHEL: No, I’m worn out.

GARRICK: So am I. Any comments or final points by any member of the Board?

(No response.)

GARRICK: Or the Staff?

(No response.)

GARRICK: Hearing none, I want to thank all of the presenters. I consider this to be one of the better technical sessions we’ve had in a long time. I thought the engagement from the audience and from multiple resources of experts was outstanding, and we thank you very much.

And, with that, we are adjourned.

(Whereupon, at 4:33 p.m., the meeting was adjourned.)

C E R T I F I C A T E

I certify that the foregoing is a correct transcript of the Winter Board Meeting of the Nuclear Waste Technical Review Board held on January 16, 2008 in Las Vegas, Nevada taken from the digital recording of proceedings in the above-entitled matter.

January 25, 2008

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