UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER BOARD MEETING

Wednesday

January 28, 2009

Marriott Suites
325 Convention Center Drive
Las Vegas, Nevada 89109

NWTRB BOARD MEMBERS PRESENT

Dr. B. John Garrick, Chairman, NWTRB
Dr. David J. Duquette
Dr. Ali Mosleh
Dr. Andrew C. Kadak
Dr. Henry Petroski
Dr. William Howard Arnold
Dr. Thure E. Cerling
Dr. William M. Murphy
Dr. Mark D. Abkowitz
Dr. Ronald M. Latanision

NWTRB SENIOR PROFESSIONAL STAFF

Dr. Bruce E. Kirstein
Dr. David A. Diodato
Dr. Daniel S. Metlay
Dr. Gene W. Rowe
Dr. Carl Di Bella

NWTRB STAFF

Karyn D. Severson, Director External Affairs
Joyce M. Dory, Director of Administration
Linda Coultry, Meeting Planner

INDEX

INDEX
(Continued)

PROCEEDINGS

8:00 a.m.

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

My name is still John Garrick, and I’m still the Chairman of the Board. Let me, as per usual, introduce the Board members as part of our routine. As most of you know, we have to remind you of that from time to time, that we are a part-time Board. Most of us have other things that we’re engaged in, so, if we let something fall through the cracks, we have that as our excuse. But, we really don’t because we have a full-time staff, and that’s what they’re supposed to do, is to help us avoid that.

My job is as Chairman, and also I have the technical lead on the radiation dose issues. And, my background is nuclear engineering, applied physics, and risk analysis. And, right now, I spend a considerable amount of time in the consulting arena in those disciplines.

As I introduce the rest of the Board members, I want them to raise their hands, and I’ll start with Mark Abkowitz. Mark is Professor of Civil Engineering and Management Technology at Vanderbilt University, and Director of the Vanderbilt Center for Environmental Management Services. And, he 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, previously holding such senior management positions as vice-president of the Westinghouse Hanford Company, president of Louisiana Energy Services, and Engineering Management and General Management of the Westinghouse Pressurized Water Reactor Systems Division. Howard chairs the Board’s Panel on Preclosure Operations, and will be leading the Board’s discussion today on the agenda topic having to do with the final closure welds on the waste packages.

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. Thure is our technical lead with George Hornberger on the Natural System.

David Duquette. David is the John Tod Horton Professor of Materials Engineering at Rensselaer Polytechnic Institute. His areas of expertise include physical, chemical, and mechanical properties of metals and alloys, with special emphasis on environmental interactions. David, with Ron Latanision, is the Board’s technical lead on Corrosion.

Andrew Kadak. Andy is Professor of the Practice in the Nuclear Engineering Department of MIT. His research interests include 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, and will be leading the discussion today on the three agenda items having to do with criticality.

Ron Latanision. Ron is Emeritus Professor of Materials Science and Engineering and Nuclear Engineering at MIT, and Corporate Vice-President and Practice Director, Mechanical Engineering and Materials Science with the engineering 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. Ali's fields of study and practice are risk and safety assessments, 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 also serves as an administrative judge on an NRC Atomic Safety and Licensing Board Panel. 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 an accomplished author in engineering and science and is the Board’s technical lead on the design of Surface Facilities.

Unfortunately, Board member George Hornberger had a conflict and is unable to be with us today. George is a Distinguished Professor at Vanderbilt University, where he is the Director of the Vanderbilt Institute for Energy and the Environment. He has a shared appointment in the Departments of Civil and Environmental Engineering and Earth Environmental Sciences. His research is aimed at understanding how hydrological processes affect the transport of dissolved and suspended constituents through catchments and aquifers. George co-chairs the Board’s Panel on Postclosure Repository Performance.

Incidentally, last month, the then-President Bush reappointed several Board members whose terms had expired, to serve until 2012. The members involved are Howard Arnold, George Hornberger, Andy Kadak, Ali Mosleh, Henry Petroski, and myself. The terms of the other Board members do not expire until next year.

Shortly after the appointments, the New York Times published a piece about appointments by a president coming to the end of their term in office. One of our Board members, namely Andy Kadak, was interviewed for the article, and I would like to repeat a quote from that article because it succinctly describes what the Board is all about. Andy was quoted as saying, “We are apolitical. Whoever is president, we are indifferent to that. We are attempting to see that the work that the DOE is doing is technically correct and appropriate.”

Since September, a number of us have been asked about the role of the Board now that there is a docketed DOE License Application. And, the answer to that question is fairly succinctly stated in the Nuclear Waste Policy Act Amendments of 1987. The Act states the following.

> The Board’s role is to evaluate the scientific and technical validity of DOE’s activities undertaken pursuant to the Nuclear Waste Policy Act, as amended.
>
> We are to report the findings, conclusions, and recommendations of our evaluations to Congress and the Secretary of Energy at least twice a year.
>
> We are to remain in existence until no more

than one year after the first unit of waste is disposed of in a repository. And, there’s lots of jokes about that.

And, we are to have unfettered access not only to final work products, but also to drafts and documentation of work in progress by DOE and its contractors.

That latter privilege is a very rare one, and most advisory committees do not have such access.

Any DOE work having to do with technical aspects is fair game for the Board’s review. But, we are limited in what we can do. The eleven part-time Board members, plus the technical staff, all of which is over there against the wall, plus a limited budget for part-time consultants in areas where we have limited capabilities, such as volcanology and seismology, is our total capability. We have to pick and choose what we evaluate very carefully, and, frankly, we have a lot on the table.

When we do pick a topic, we try to take a systems approach, total systems view of the project. That is, we look at how the entire system is affected by any particular and specific technical issue.

A good example is the thermal response white paper we published on the Board’s website last year. The decay heat from spent nuclear fuel affects all preclosure and postclosure aspects of the waste management system, from storage and waste acceptance, through transportation, final packaging and emplacement, and beyond closure.

The licensing process, as we understand, is well underway. We are not a party to the licensing process, and we don’t intend to become one. However, any work that DOE undertakes that has a technical aspect, which is most of the work that they do, is a candidate for evaluation by the Board. This work could be analysis or thinking work. It could be computer modeling. It could be design work. Or, it could be laboratory or prototype work. We will not choose to evaluate something because it is part of a contention or an NRC request for additional information, that is, an RAE. We won’t choose not to evaluate something because it is a part of a contention, or an RAI.

Whether it arises out of the licensing process or as some other part of the OCRWM program, such as transportation or the waste management system, is really immaterial. Our intent is to work on issues, on things that are important from the standpoint of public confidence in the technical understanding, integration of the entire waste management system, or operational risk. As such, some things may fall inside the licensing fence, while others, like waste acceptance, will not.

Speaking of our role and our resources, when you look over to the staff table, you may note a conspicuous absence. Executive Director Dr. William Barnard is no longer there. Bill retired earlier this month after 35 years of federal government service, 18 of which were as an Executive Director of the Board, during which he served several boards, several different boards, and six different chairmen. He has been here essentially since the Board’s inception. We, the Board, would like to think that our many accomplishments over the years are our own doing. But, that’s just not the case, it is the staff, and it’s been under Bill’s leadership, who arranges these meetings, does most of the legwork and some of the brainwork, and actually pushes product out of the door.

Bill is in the audience as a member of the public. On behalf of this and past Boards, I would like to thank you, Bill, for the fine job, the excellent job, you have done over the years, and, we certainly will miss you. And, we wish you all the best. Thank you very much.

The Board posted the Executive Director vacancy announcements on our website and USAJOBS almost two months ago, and it will close soon. We’re casting the net as far and wide as possible in the hope of finding someone who can fill Bill’s shoes. Except for educational requirements and U.S. citizenship, we have no restrictions. We hope we can fill the position in the next few weeks. In the meantime, Karyn Severson, who is sitting at the staff table, is serving as interim Executive Director. Karyn has been with the Board almost as long as Bill. Thank you, Karyn, for serving such a noble task and providing this gap bridging that we need to do at this time.

Now, let us turn to today’s agenda. First up is Russ Dyer. Russ is standing in today for Chris Kouts, who is the acting director of OCRWM as of Monday of a week ago, or so. Russ will give an update on the program, and I will introduce him in just a moment. Next, we are reviving something that used to be a stable of the Board meetings, and, that is, a science update. Yes, there is a considerable amount of science still going on in the program. Peter Swift will give the talk, with help from Zell Peterman and John Whitney of the USGS.

Then, we will finish the morning with a series of three talks, all related to the possibility of rock falling from the roof of repository drifts, particularly after closure, and the insulating properties of that rock and the effect of the rock on waste package temperatures. Mark Board and Ernie Hardin will provide these talks.

For the afternoon, we have three talks dealing with the potential for criticality and related topics. This is not just the potential for criticality after repository closure, it’s also the potential for criticality during transportation. We have John Wagner of Oak Ridge, Albert

Machiels from EPRI, and Drew Barto from NRC’s Division of Spent Fuel Storage and Transportation. To the best of my knowledge, none of these individuals have addressed the Board before, and we are looking forward to hearing from them.

After the break, our last talk of the day is about Idaho National Laboratory’s work on developing a system to make the final closure welds of the loaded waste package. Eighteen months ago, many Board members, including myself, visited the site at INL where this development effort is taking place. We are very much looking forward to an update on the work. Our speaker, Chris White, who has not appeared before the Board, is with us, and we welcome Chris.

Following the meeting presentations, we have scheduled time for public comment, which is always important to the Board. And, if you would like to comment at that time, please enter your name on the sign-up sheet at the table near the entrance to the room. By the way, we also have an attendance sheet back there, and if you haven’t jotted your name and e-mail address down, please do so. If you prefer, remarks and other material can be submitted in writing and will be made part of the meeting record.

Incidentally, I have learned that Bruce Breslow, the new Director of the Nevada Nuclear Projects Agency, is in the audience. Bruce, would you please stand up so everybody knows who you are, if they don’t already? Welcome. This is your first Board meeting, and we very much would like to have your impressions during the public comment period.

Now, some of you have asked about questions during the course of the presentations. We do have a pecking order, and a time element that determines how far we can go with that approach. First, Board members ask questions. Then, if time permits, staff members. Then, if time permits, members of the audience. But, we do have other mechanisms for the audience participation.

Frankly, we rarely get to the point where staff members can ask all the questions they have. Thus, our suggestion is that you write down your questions and submit, and they will be made part of the record. And, we will actually read them if we have time.

As usual, to minimize interruptions, we ask that all of you turn off your cell phones, or at least to the silent mode. And, I also want to remind everyone that it is very important that you identify yourself, if you are speaking, and speak into the microphone. We do have to have an absolutely accurate record of the meeting, and some of the microphones don’t have as good a pickup as others, and, so, you have to be pretty close to them. So, give us your name and your affiliation and any relevant information to complete the record.

Now, it is my pleasure to welcome back Russ Dyer to give our first talk. Like Chris Kouts, Russ also has a new title and additional responsibilities as of January 20th, or thereabouts. Before January 20th, Russ was Director of the office of Chief Scientist. And, now, he is Acting Director of the Office of Technical Management and Director of the Science Division, which is part of the Office of Technical Management. Russ has a Ph.D. in Geology from Stanford and a bachelor's degree from Rice, also in Geology, and has addressed the Board many times, and we are very pleased to hear from him again. Russ?

DYER: Thank you, Mr. Chairman.

First, I'd like to note that Chris Kouts, our Acting Director and the Deputy Director of the Program, sends his sincere apologies, but he's tied up in Washington this week dealing with the new administration.

This presentation that I'm going to go through is what Chris was intending to present to you, and there's two parts to it. There's kind of a year-end review of what events or accomplishments took place over the past year, and a look-ahead of significant upcoming events or activities.

Although we have a new year, a new administration, we've got the same issues that have been the rationale for the program since 1982 when the Nuclear Waste Policy Act was put in place, although things have changed a little bit. So, let's look at some of the accomplishments of the last year.

The License Application, of course, was completed and docketed. License support network was certified. Our NEPA documents were completed.

The EPA issued the final radiation standards for Yucca Mountain.

We awarded contracts for the design, licensing and demonstration of the TAD system.

The new reactor standard contract and amendment is available for the new reactors that are in planning.

Management and Operating contract was selected and awarded. That's to support the OCRWM program.

We issued a couple of major reports that were mandated either by regulation or Congressional mandate. The second repository report and interim storage report, Total System Life-Cycle report and the fee adequacy assessment were issued toward the latter part of the year.

One of the things I'm going to talk about later is the status of funding for FY09. Of course, we're all, everybody in the government is currently under the existing continuing resolution, which as currently structured, will expire on the 6th of March.

The License Application submitted on June 3, 2008 to the Nuclear Regulatory Commission, a high water mark for the program so far. Secretary Baudman was present at the celebration we had at the National Press Club. This is the actual submission. This is Ward tendering the application to Mike Webber of the Nuclear Regulatory Commission.

Next slide, please?

The NEPA documents that were generated in 2008, a number, the Repository Supplemental Environmental Impact Statement, the Rail Alignment EIS, National Transportation Corridor Supplemental EIS, and, in October, we came out with the Record of Decision for the Rail Line. Two major findings in that Record of Decision. We notified the public of the decision to construct and operate a railroad along a rail alignment with the Caliente corridor. And, we also allow shipments of general freight on the rail line. So, it’s a shared-use option.

Next slide, please?

Now, the License Application, let’s talk about next steps in the licensing process. First off, whenever the License Application was docketed by the NRC, it started an internal clock within the NRC, and a number of actions that are triggered by that clock. Shortly after the docketing of the License Application, we started to receive requests for additional information from the NRC staff. 129 received to date. That’s a moving number. It changes literally every day.

It also started a clock for potential intervenors to file petitions to intervene for requests to receive status as interested government participants. Those petitions were received, I think it was the 22nd of December when those petitions needed to be received by the NRC. We had a period of time to respond to them. There were 12 petitions to intervene, a total of 321 contentions, some of which are duplicates. Some of the potential intervenors chose to duplicate some of the contentions. DOE has responded to all of the contentions based either on a technical and/or a legal basis, as appropriate. And, the clock on that, there is a series of activities laid out in the schedule that is Appendix D to NRC’s 10 CFR, Part 2, which leads to petitioner’s response to answers by the 24th of February, and then the first prehearing conference on the 11th of March of this year.

The Nuclear Regulatory Commission, on Monday of this week, issued in the Federal Register notice, I think it was actually announced last week, the establishment of three Atomic Safety and Licensing Boards for the Yucca Mountain proceedings, named who the chairs and the members of the three boards are. And, those boards will be dealing with the petitions of the intervenors, our response, and the response of the parties here.

The other things on the schedule--if I could back up one, please? Other things on the schedule here, these dates are laid out in this schedule in Appendix D to Part 2.

So, these are not DOE proposed dates, but, rather, these are laid out, at least down through the 2011 date, are laid out in the NRC’s schedule.

The dates down at the bottom, the 2016 estimate, submission of an application for the license to receive and possess. 2019, decision on the license to receive and possess. And, then, repository operations by 2020. Many of these are contingent on the schedule that’s followed by the actual licensing process, and it’s also highly contingent on the funding scenario that program elements will get over the next literally decade, or so.

Next slide, please?

The Environmental Protection Agency produced the final radiation standards. It’s consistent with the National Academy of Sciences recommendations. It satisfies the court decision back in July of 2004 regarding the duration of the standard.

The pertinent parts of the final standards: First, retains the dose limit of 15 millirems per year in the postclosure for the first 10,000 years after closure of the repository, after disposal. Establishes a dose limit of 100 millirems annual exposure per year for the period of time between 10,000 years and one million years. Considers the effects of a number of potential events, climate change, earthquakes, volcanoes, and corrosion of the waste packages, to safety during the one million year period. And establishes a radiological protection standard at the time of peak dose, up to a million years after disposal.

The transportation, aging, and disposal canister. We awarded contracts for the design, licensing and demonstration in May of 2008. In September of this year, 2009, the vendors are to submit TAD designs to NRC for review, with demonstration of the canister system at a utility site by May of 2013.

Next slide, please?

The New Reactor Standard Contract and Amendment. This is an amendment to the standard contract for disposal of spent nuclear fuel, and it supports the development of the next generation of nuclear power reactors. And, an applicant for an operating license must have a contract with the Secretary for the disposal of spent nuclear fuel or high-level waste that may result from the use of such a license. And, what we’re doing is putting in place the vehicle that allows that condition to be fulfilled. Eighteen contracts have been signed as of December of 2008, last month.

On October 30th of 2008, we awarded a contract to a new M&O contractor. This is USA Repository Services, which will be taking the place of Bechtel SAIC, which has been our management and operations contractor supporting the program for, I think, about eight years.

The transition is to be completed by the first of April. It’s a two and a half billion, five-year period of performance contract, with a potential for a five-year option. USA Repository Services is composed of URS, Shaw Environmental and Infrastructure, and AREVA Federal Services. Those are the main entities involved in USA Repository Services.

The main activities that their scope includes is providing management expertise and support for repository design; addressing questions or requests for additional information from the NRC supporting the licensing process; and providing construction management and integration support.

The reports that were issued in the last year. The need for a second repository--this report, of course, is out in the public arena--concludes that unless Congress raises or eliminates the current statutory capacity limit of 70,000 metric tons of heavy metal for the first repository, a second repository will be needed.

We were also asked to produce a report regarding interim storage of spent nuclear fuel from decommissioned nuclear power reactor sites. And, in the report that we produced, we discussed the status of the inventory, contractual arrangements, related litigation, financial liabilities, and concluded that there are changes that need to be made in legislation and actions that would have to be taken for the Department to develop an interim storage facility and demonstration program.

The Total System Life-Cycle Cost, we looked at. There is an increase in the cost of the program from--we did it in 2001, 2007, and I’m pretty sure this is 2009. The cost has gone up somewhat. The reason for the rise in the Total System Life-Cycle Cost is consideration of more waste, more years of shipping and operations, refinement of designs, better estimates, and increased materials cost, of course, with the cost of metals going up recently, that’s a substantial part of this.

If you look at how the cost share breaks out, the utility share would be about 80 percent, about 77 billion. The defense share is about 20 percent, about 19 billion.

Next slide, please?

The Fee Adequacy Report looked at the status of the Nuclear Waste Fund, and what we determined from this examination is that the current fee structure is adequate. We are getting the revenues into the Nuclear Waste Fund, average about $750 million per year. Since we are investing the excess of what is not spent in Treasury securities, we’ve been accruing interest. Current value of the fund is approximately $22 billion. So, we’ve found no rationale for recommending an increase in the fee.

The status of the program funding. This is, of course, a big question mark, not just for us, but for everybody. The fiscal year 2009 Presidential request for the program was $494 million. The mark out of the House is, the Presidential request, $494.7 million. The senate mark was somewhat less, $388 million. Of course, we have not had an appropriation. We have not had the Conference Committee in an appropriation. So, we are acting under a continuing resolution. You get either the lowest of a House or Senate mark, or what the previous year’s appropriation was. And, the previous year appropriation was $386 million. So, our current appropriation is $386 million, but if the government comes out with an omnibus bill, then there will be some reconciliation between these marks, and sometime, hopefully before March, we’ll understand what our budgetary scenario is for the remainder of the year.

Next slide, please?

Program accomplishments. Kind of a moving chart here. We are moving up the list. You’ve seen this probably for years. Starting back with the Nuclear Waste Policy Act in 1982. Now, we’ve got another big check mark, in that the License Application is submitted to the NRC, and we are in the licensing process.

Next slide, please?

So, where is the program today? The licensing support network is certified and updated periodically. We’ve got a high-quality License Application and NEPA documents submitted to the Nuclear Regulatory Commission. We are in the licensing process. The TAD canister development contracts are underway. We have contracts for dealing with spent fuel for new nuclear plants available.

I’m going to spend a little bit of time talking about this bullet, senior management and support teams in place to support the license review. And, the last one is one of the reasons why Chris is not here. We’re looking forward to working with the new administration on this important National issue.

Dr. Garrick mentioned that I have a new title. Could we go to the backup slide, please? On the 4th of January, we put in place an organizational restructuring, and actually Ward had talked about this back in July. I think he talked to this Board about his intent to put in place an organization, a federal organization structure, that he felt was necessary to execute the three major federal projects that OCRWM is tasked to perform. And, those are, first, to build and operate the repository, second, to build and operate the Nevada Rail System, and, third, to develop and operate the National Transportation System. And, we called this our 2010 Organization.

So, the organization that we put in place on

January 4th of this year is the framework for that organization that we will flesh out as we get closer to 2010.

Some of the key concepts in the new organization. Increasing the organization size and capabilities. We were looking at almost doubling the federal staff within OCRWM. And, with a focus in some organizations, shifting emphasis from office in the east, in the Forrestal Building, moving those functions out to the west. Establishing an SES level chief operating officer within the Office of the Director. Reducing the number of direct reports to the Office of the Director. In the old organization, I think we had about eleven direct reports to the Office of the Director.

There is a new office established. That’s the Office of Technical Management here. And, this actually sweeps up three existing organizations, the Office of Engineering, which is now the Engineering Division of the Office of Technical management, the Regulatory Authority office, which is Bill Boyle, becomes part of this, and the office of the Chief Scientist, which is now the Science Division, becomes a part of this. And, I’m acting in here until we can identify an individual. We had a vacancy announcement out on the street back in the late summer and fall for both this position and the chief operating officer position. We did not get to the point of making a selection when OMB froze the--these are both SES level positions--when

OMB froze the SES process. So, this selection is in limbo for a while. The rest of these positions are filled by people who I think you--these are all names that have been in the program for quite a while.

One of the big things that this organization will focus on is project management, the establishment of integrated project teams with a federal project manager assigned to each of the major activities that I laid out, the repository construction and operations, the Nevada Rail, and the National Transportation System. So, those major federal project directors will be in the Office of Project Management, and we will implement a matrix management system, where line managers provide staff support to support the project managers.

And, with that, sir, I'm through. Any questions of the Board?

GARRICK: Thank you. Thank you very much.

Questions? Yes, Andy?

KADAK: Thanks, Russ. Just a couple of questions. Relative to the RAIs, could you summarize what major issues thus far the NRC has identified that they believe require additional work?

DYER: Well, I'm not sure we've got anything that necessarily requires additional work yet. What we've got so far is questions on the part of NRC. About 60, 65 percent of the questions to date, RAIs, have involved the postclosure arena. Our response has been primarily to try to clarify information that we think is out there. But, it may not be stated too well. So, we haven’t got to the second part where NRC may tell us that we really feel that there’s more work needed here.

KADAK: Well, in the postclosure area, what are they focusing in on right now then in terms of clarifications from your perspective?

DYER: Well, I mean, we’re getting RAIs literally in every field. So, there’s a suite of RAIs right now that we’re dealing with on unsaturated flow and transport. We have dealt with some on geotechnical issues, seismic issues, seismic design. My expectation is it’s going to cover the entire gamut of the table of contents of the license application.

KADAK: But, your view is more explaining what you’ve done rather than having to do additional new work to satisfy their questions? I guess, really, that’s what I’m trying to figure out.

DYER: Well, we think that the work is adequate if we explain it well.

KADAK: Okay.

DYER: If the NRC feels that’s not the case, then we’ll have to re-evaluate and do something.

KADAK: Okay. Now, in the area of contentions, the same kind of question. What is the major emphasis of the contentions from the standpoint of Yucca Mountain? And, you say you’ve already responded to those in writing; correct?

DYER: Correct.

KADAK: And, what is your conclusion about the contentions in terms of opening up new areas of investigation?

DYER: Well, I mean, it remains to be seen how it plays out before the Board, but we feel that we responded adequately to every one of the contentions, all 321 of them, some on a technical basis, some on a regulatory basis. And, they covered the entire gamut, everything from sociopolitical to engineering technical, to postclosure scientific.

KADAK: Has DOE basically accepted any of the contentions as legitimate?

DYER: We responded to all of them.

KADAK: I take that as what?

DYER: we think there’s a basis for dismissing every contention.

KADAK: Okay, thank you. The last question--thank you, Mr. Chairman--is what was the reason for naming three licensing boards rather than a typical one for a typical license application?

DYER: I think that’s one for the NRC.

KADAK: But, are they covering the whole, I mean, are they doing the same thing, or are they divvying it up?

DYER: No, my understanding is that the three boards will be operating simultaneously. They will split up parts of the, for instance, the contentions. They will split up the petitions to different boards, so that each board will examine a different part of the petitions for intervenor status.

KADAK: So, I mean, the licensing board needs to make a recommendation on whether or not to allow a contention, and then, finally, you know, resolve issues.

DYER: Right.

KADAK: So, you will have--which board is going to be the one that says everything is either okay or not okay, and comes forwarding it to the Commission?

DYER: I’m afraid I don’t know.

KADAK: You don’t know?

DYER: I think each board is going to make a recommendation.

George, do you have any insight? This is George Hellstrom with our general counsel.

HELLSTROM: George Hellstrom, DOE, Office of General Counsel. In general, what we’re asking is really the overall regulatory structure, and NRC is also here and it’s part of their structure as to how this proceeds. NRC’s regulations at 10 CFR, Part 2 describe the process. The board is in place. We have an adjudicatory process that is started. The boards will consider the pleadings that are filed, and make an initial determination as to those contentions and the adequacy of the petitions. They will file orders. There will be a process that we'll go through eventually, for admitting contentions.

KADAK: No, I understand all that. I'm trying to figure out how you manage three boards on one project.

HELLSTROM: That is actually a question that should be asked of the NRC and/or the ASLAB.

DAVIS: Jack Davis, Deputy Director for High-Level Waste, NRC. There's a separation between us doing the review, and the board. So, I've got to be very careful with what I say. The reason that they assigned a number of boards was just to meet the schedule, the Congressionally mandated schedule. And, given the number of contentions, they choose how many boards. How they go about consolidating that to make sure that they are all consistent, I can't answer. All I can say is that that, you know, Russ is correct, the number of boards are there to just divvy up to meet the schedule.

KADAK: Okay, thank you.

GARRICK: Mark?

ABKOWITZ: Abkowitz, Board.

If we could go to Slide 2 for a moment, please?

Russ, I’m just trying to understand the bullet points you have on this slide relative to the comments you made later on in your presentation about the new reactor standard contract and amendment. Does that activity pretty much preclude the rationale for the third bullet that you have on this slide? Is it not true that by that other action, you’ve pretty much circumvented the argument on that third bullet? Is Yucca Mountain no longer an impediment to--

DYER: If we had more reactors come online, the 20 percent will increase.

ABKOWITZ: Okay. But, my understanding, and maybe I’m misunderstanding your new reactor standard contract and amendment slide, but in essence, with the contracts that you’ve signed in December of ’08, does that not provide the capacity to continue to generate additional nuclear energy such that energy security has kind of been--that Yucca Mountain is no longer an impediment to the ability to supply that degree of nuclear energy?

DYER: Well, I would tie this back to the waste confidence rulemaking of the NRC, which, if I remember right, the license of every operating reactor, present or future, is tied to the development and operation of a disposal system I think by 2025, if I remember right. So, that’s still an underlying consideration here. There is still a need for a way to close the fuel cycle. And, I have a feeling I’m missing your question.

ABKOWITZ: I’m just trying to connect the dots. It seems to me one of the principal arguments for why Yucca Mountain was important was because we needed to have a waste management solution in order to continue to generate the amount of nuclear energy, or even expand the amount of nuclear energy that we produce. And, I guess maybe I misunderstood your Slide Number 9 to imply that actions are being taken that I guess sort of marginalize the need to have a Yucca Mountain in order to be able to continue the ability to rely on nuclear energy and the production in the future.

DYER: No, the new reactor standard contract is a contract between the Department of Energy and utilities to add future spent fuel into the queue for a repository. So, the government is signing a contract and taking on the liability for disposing of fuel.

ABKOWITZ: Okay, thank you.

GARRICK: Ron?

LATANISION: Yes, if we can turn back to Slide 2 for just a moment. I don’t think I’ve seen this particular iteration before. Is this a document that reflects, in a historical sense, the Department’s point of view, or does it today? Is it endorsed by the new Secretary? What is the standing of this document?

DYER: Well, this particular slide we’ve used for the past year and a half, or so. It’s been an element of most of our program briefings. It does have an understanding of support from the previous administration. I don’t think it’s been debted by the new administration yet.

LATANISION: Yes, that was my question, whether the current Secretary would endorse this.

DYER: Right.

LATANISION: Is that something that’s, the question that’s being asked of him?

DYER: Well, Dr. Chu has been in place for a little over a week, so--

LATANISION: Things move fast in this new administration.

DYER: They do. But, everybody gets their turn. So, we haven’t had our chance to provide a programmatic briefing and have a dialogue with Secretary Chu yet. That should be coming up fairly soon.

LATANISION: Okay, thank you.

GARRICK: Henry?

PETROSKI: This is Petroski, Board.

On your Slide 12, perhaps before you have your briefing, you could clarify. You say these are millions of dollars. That would make the total in trillions, wouldn’t it?

DYER: Well, I’m hoping it’s billions. It ought to be

96.1 billion.

PETROSKI: Well, it should be dollars; is that right?

DYER: Well, I think it’s right.

PETROSKI: I think a thousand million. No, it’s a billion, okay. Well, then, the other slide, I thought I saw an inconsistency here. I’m looking at 14. Okay, my error.

GARRICK: Russ, picking up on Ron’s comment about the impact of the new administration, when can the Board expect to get a project status review that reflects input from the new administration? Is that what you were alluding to with respect to the appropriations that will possibly take place by March? When are we really going to see the impact of the new administration in the project status?

DYER: I can’t give you a date certain. I mean, first, we’ve got to have the dialogue to understand what, if any, changes the new administration is looking at for the program, and then come up with a proposal for how to address whatever changes those might be. Whenever our charter is clear, we will certainly be happy to share that with the Board. As we develop a response, we would be willing to share that. But, I can’t give you a time schedule.

GARRICK: Are there any hints? I’m sure you’ve had a lot of interaction with transition team members, and there’s some knowledge about what the expectations might be. Is there any evidence whatsoever out there that would indicate the changes that might take place?

DYER: The only thing that I can point you to was the hearing testimony of Secretary Chu, and it was pretty ambiguous.

GARRICK: Yes, I saw all that, and, you’re right, it was ambiguous. It was more of a pork discussion than it shed much light on nuclear waste.

David?

DUQUETTE: Duquette, Board. And, this is a follow-up on Dr. Petroski’s question. We’re not supposed to be involved necessarily with dollars, but a very quick piece of mathematics says that so far, you’ve collected 16 billion, and 13 billion in interest, which is 39 billion. You’ve got 22 billion left. Does that mean that $17 billion has been spent on Yucca Mountain so far?

DYER: No. I thought that our expenditures to date were on the order of about 10 billion, and that’s total costs of all the programs since 1983 when OCRWM came into existence.

DUQUETTE: Well, I’m looking at that slide, and it says 16 billion in fees and 13 billion in interest. That’s about 40 billion. And, the current value of the fund is 22 billion. 29, okay, sorry. All right, that closes--

DYER: 29, 22, about 7 to 10 billion.

DUQUETTE: Yeah, 7 to 10 billion has been spent so far on Yucca Mountain?

DYER: Well, the majority of it has been spent on Yucca Mountain. Remember, there were other programs in place before Yucca Mountain.

DUQUETTE: Okay.

GARRICK: Any other questions from the Board? Yes, Andy?

KADAK: Just one. Russ, in terms of the technical resources available to answer the questions and participate in the licensing process, I understand a lot of the people were laid off who had been working on the project. Could you just comment on your remaining work force relative to being able to answer the technical questions, either in contention world or RAI world?

DYER: Well, when we assign skill priorities, our organization of skill to our contractors, whether it be Sandia, the lead lab, or BSC or USA, the number one priority is to maintain the licensing process, so keeping the capabilities, the people needed, to maintain the technical basis and to respond to RAIs and contentions, is the number one job for our contractors and for us.

KADAK: So, the answer is you feel comfortable in saying that at least at the current level, you are staffed sufficiently with the resources needed to answer the licensing questions?

DYER: At the current level, we’ve been able to respond on the NRC schedule, which is generally 40 calendar days after they give us the RAI, they expect a response. But, there are people a lot of days and nights, but we’ve been able to make it so far.

KADAK: What has been the number of jobs lost as a result of the cut-backs, from your essentially two or three years ago staffing level to today.

DYER: Oh, this is off the top of my head. Two or three years ago, we had a total program contractor staffing of around 1800 to 2000 people, as I recall. Right now, if you look at the head count at the M&O, it’s probably 600 to 700 people. I think lead lab is maybe 350 to 400. So, we’ve lost somewhere on the order of 900 people, I think, out of the program in the last two to three years.

KADAK: Thank you.

GARRICK: David?

DUQUETTE: Duquette, Board, again.

I think it was the last Board meeting, maybe the one before that, there was some concern on the Board that with the emphasis being put on the License Application and getting it through the NRC, including the contentions, and so on and so forth, that the Science and Technology Programs, the Science and Technology Program that was a separate issue, but the general Science and Technology Programs would take a back seat to what was going on. Do you see that happening, or are your primary resources going to be directed towards the License Application and the contentions, or will there still be about the same level of Science and Technology that has been supported over, say, the last ten years?

DYER: Well, I told Andy what our number one priority is, that is our number one priority.

GARRICK: Yes, Howard?

ARNOLD: Arnold, Board.

I'm curious about the transition to a new contractor, and how that affects what you've been saying about keeping the people who need to respond to these RAIs and contentions. Would you give me a little more detail on that transition, please?

DYER: Well, transition is always a challenge. The Department of Energy's M&O construct for a contractor is kind of unique. Generally, when an M&O contractor changes out, there's not that much of a change in the work force. The top management, through maybe part of the middle management, will change out, but the work force itself pretty much moves from getting a paycheck from one organization and wearing a badge, to changing out a badge and getting a paycheck from somebody else.

But, there's going to be kind of a step function here as we go through the transition from BSC to USA because we're going to have to deal with whatever the budget realities are that come out after the ’09 appropriation is finalized. I would love to think that it’s a smooth transition, but there’s potential for a big speed bump.

ARNOLD: Is USA itself an entity created solely for this project?

DYER: That’s correct.

GARRICK: Okay. Well, I think we’re kind of running out of time for this topic. Thank you very much, Russ. Unless there’s a burning question from somebody, I think we’ll move on.

Okay, we’ll get into the science overview, program and project--or, I’m sorry, the science update. Peter is the Yucca Mountain Project lead laboratory chief scientist. He’s also a distinguished member of the technical staff of Sandia National Labs. Peter?

SWIFT: Thank you. And, I want to start off first by just saying that we have a very full hour here in front of us. I want to make sure we do get to the two presentations by the USGS, by Zell Peterman and John Whitney. So, I’m going to move fairly quickly through what I’ve got here, and I want to make sure we do get those other two.

I want to start by acknowledging the others who contributed to this, in particular, Doug Weaver. Michael Schuhen is here in the audience to field questions on some of the testing activities. Rich Quittmeyer, who is not here, who worked on seismicity activities. Bob Jones is here. And, Frank Perry contributed a little bit on volcanism, isn't here.

Can I have the next slide, please?

There are three topics I'm going to try to cover. The current geotechnical field investigations, a very brief summary of current performance confirmation activities, and a little summary of the probabilistic volcanic hazard analysis update. And, the Board has scheduled on its website on April 16th a meeting devoted to the igneous topics, and I expect that you will hear more about that then. But we'll have just a small update on it now.

Next slide, please?

All right, the two technical activities.

Next slide?

The activities that we have, additional activities since the last time this Board was briefed on geotechnical activity, that would have been in January of 2007. I believe Russ Dyer gave that briefing. And, two points here on this slide. First, the purpose of these activities. They are to support licensing work by enhancing confidence in, and the topics listed here are the material properties, seismic velocity, basic alluvium data, thickness, for example. The understanding of general geologic structure in the surface facilities area, and the material property data for use in the design of the surface facilities.

This information is documented in the Safety Analysis Report. That’s the License Application, Section 1.1.5, and it’s well worth the read.

Next slide, please?

The drilling and testing program activities here that are of note, four types of things here. Boreholes from which we have geologic logs, geophysical logs, and seismic velocity testing. Surface test pits, which we have used for material density tests and basic geologic characterization of the alluvium in the area of the surface facilities. Field tests, primarily here, the seismic work done on spectral analysis of surface waves. That’s a seismic technique for getting material property information on the near surface rocks. And, some in situ testing of the alluvium. And, then, lab testing for dynamic and static material properties.

Next, please?

43 boreholes since the last time we briefed you. These are geotechnical boreholes, relatively shallow boreholes in the area of the surface facilities. Major purpose here to run geophysical logs for lithologic and engineering properties. Downhole velocity surveys, geologic logs, and optical televiewer data gave us oriented data of the structure downhole.

KADAK: How deep were these holes?

SWIFT: In general, they’re in the alluvium until they reach tuff. I’d have to ask Michael Schuhen to get more information than that. Michael, do you want to add to the depth of these holes? This is Michael Schuhen from the lead laboratory.

SCHUHEN: Michael Schuhen, lead laboratory, Sandia National Lab.

Most of the boreholes were drilled into the Tiva formation. Generally, we tried to hit the middle of the Tiva formation. There are a few that run deeper for testing purposes.

KADAK: What is the depth? I’m trying to get a depth.

SCHUHEN: Well, it varies on the location of where they were drilled. If they were drilled closer to the Calico Hills then, they may be 300 or 400 feet deep. Further out in the Valley, they may approach 800 to 1000 foot depth.

KADAK: Thank you.

SWIFT: Next, please?

This is a map here. This is taken directly out of the License Application Safety Analysis Report. It just shows the location of new boreholes. Also, the location of, just to orient yourselves here, in a contour map, you can actually see the North Portal right here. So, the existing surface facilities are in this area here. This would be the proposed aging pad area here to the north, and other surface facilities out here. So, what you see here are the location of the new boreholes in red, and the new test pit facilities, which are these three, these four in here.

Next slide, please?

Just a photograph here of field operations, the drilling and the core management facility.

Next, please?

I’m sorry, I said there were four. There were three of these recent ones. They’re the three furthest to the northeast on the map, which supplement the four that were previous done.

The purpose here is to provide geologic characterization of the alluvium. And, in particular, we did field tests to measure soil property, density, and we took core samples back to the lab for testing. I have some pictures of that in a second here.

Next, please?

This is an example of a test pit. I’m sorry, I don’t know which number pit that is. To put it in scale here, each bench is on a scale of a meter, the vertical cut on them, and test pits like this, ring densities were done on each level to measure the density of the alluvium in each one.

Next, please?

And, the alluvium is pretty well cemented, at least some of it is. And, here’s an example here of a piece of alluvium being collected for lab sampling. There is not a sleeve around this. It’s been milled into that lathe, cut into that column shape. It’s about a foot in length there. So, the alluvium, we think of it as a sand and gravel. It’s pretty well cemented here. That’s holding together on its own and ready for rock testing. On the right there, it’s got a sleeve put around it for testing.

Next, please?

The spectral analysis of surface waves testing. This is a surface based seismic test done primarily in the vicinity of the surface facilities and out over the new test pad area, the aging pad area. The purpose here is basically to get shear wave velocity data for the near surface rocks.

Next, please?

It’s done with improvement here. We’ve used a Vibroseis truck. It gives us a stronger signal, somewhat deeper testing, down, I believe, about 1000 feet we’re getting data now. And, this data is a primary source of information to confirm the seismic hazard analysis for the surface facilities.

Next slide?

So, the results, the data from the recent tests I just described is being compiled in an addendum to a January 2008 data report. This data itself, obviously, the tests are ongoing, much of it is not included directly in the License Application. The fact the tests are ongoing is described in the License Application. We expect to have a supplement, an addendum to this technical report sometime later in this calendar year. That’s conditional on funding, and other things. And, we will be setting the consistency of the new data with that used to support the License Application, and we will report it as it becomes available.

The initial examination is the information we had at the time we submitted the License Application. The initial examinations indicate data is indeed consistent with the previous observations.

Next, please?

Okay, moving quickly on. Performance Confirmation. Keep in mind that from the point of view of the License Application, Performance Confirmation is a regulatorily defined term. So, when we classify something as a Performance Confirmation activity, it creates a regulatory status for it, and it enters a different regime of documentation.

In the License Application, well, this sentence here is a quote out of the License Application. That is our description of what the purpose of the PC plan is. It’s in the Safety Analysis Report, Chapter 4, which does call out 20 activities for the Performance Confirmation. And, the next slide has a general framework of it.

Next, please?

This slide comes from, the reference down here at the bottom, the Performance Confirmation Program Annual Fiscal Report, Annual Report for Fiscal Year 2008. But, the basic point you see here is that we envision the PC Program going through stages, beginning with site characterization, which is in the past, through the baseline phase where we establish the baseline for the License Application. Then, the submittal of the License Application, which has happened. We’re now in the phase of the review of the License Application. We’re here now. And, should the site be authorized for construction, we’ll move into a construction phase, and eventually should it be licensed for operations, the operational phase. The role of the Performance Confirmation Program changes as we move through these phases.

What you will see is that during this licensing review period here, it’s a lot of time for the PC Program. We have three activities that are actually ongoing, and in operation now. And, others that are considered to be ongoing science and testing programs that are potential activities being included formally in Performance Confirmation in the future. We do not consider them part of the Performance Confirmation plan or program at this time.

Next slide, please?

Just a little bit about the three who actually are active Performance Confirmation activities today. Construction effects monitoring, precipitation monitoring and seismic monitoring.

Next slide, please.

Construction effects monitoring. Basically, we have extensometers in place in the underground that are measuring deformation of the rock walls, convergence of the drifts, and the short answer there is that we’re seeing very little movement in the rock wall. I think Mark Board will probably say more about that later.

Next, please?

Precipitation monitoring. There is a map here that shows the blue area shown on there, is actually our model domain for infiltration modeling. Of course, precipitation is a direct interest to our infiltration work. So, these are the sites where we have basic field stations for our precipitation monitoring. We are collecting that data. We are comparing it to the historic baseline that was used for the infiltration modeling, and, to date, that is confirming that we had an appropriate basis for the infiltration work.

Next, please?

And, seismicity monitoring. We have monitoring stations for ground motion. We’re monitoring regional seismicity within 50 kilometers of Yucca Mountain, and following local or regional seismic events, we are evaluating surface and subsurface displacement, if any. We are not seeing it. And, we are comparing what ground motion information we are collecting to what was used for inputs for the probabilistic seismic hazard analysis, and, to date, the information is confirmatory and consistent.

Okay, next, please?

Now, a little bit briefly about the probabilistic volcanic hazard analysis update. All right, this is an update to the probabilistic volcanic hazard analysis completed in the 1990’s. It is used as the primary basis for the probability of igneous events, as reported in the License Application. The availability of additional data, primarily high resolution aeromagnetic survey data, and additional drilling information about the location of buried basalts in the region, led the project to conduct an update to the PVHA. Several years ago, we had convened a panel of eight experts, a significant overlap between that group and the original panel, and eight experts participated in a four year expert elicitation, five workshops, a field trip to the site, individual elicitation interviews, and then documentation. That report is available. I believe the Board has it. And, then, the final result that came out of this process is the weighted average, the equally-weighted average of the eight individuals.

Next slide, please?

Just so you have the names of the experts in volcanism who participated in this. There there they are. Their initials appear on a subsequent slide. I wanted to make sure you had the names.

Next, please?

The type of information they considered. This is a summary map of the aeromagnetic data that we had available. The colors shown here. From this data, we interpreted potential buried basalt anomalies, about 30 of them. Seven of them were drilled. Four of them turned out they were indeed basalts. Three of them turned out to be tuffs rather than basalts. The youngest of these new anomalies that we identified was 3.9 million years. The other three were greater than 9 million years. We did not find any additional evidence of young volcanism in the region. The youngest one is this one here. The other sites that were drilled on the map, a couple up here in Jackass Flats, others over here in Crater Flat.

Next, please?

So, from this, what do we come out of it with? Well, confidence that we indeed can distinguish basalt from tuff on the basis of the magnetic characteristics, and known surface features. Can I go back one, briefly?

We show on this figure here the major faults here at Windy Wash and Paintbrush Canyon. The reason to highlight those here is the area between them, the Yucca Mountain region.

Next slide, please?

You see here in the topographic image. This is where tuff is at, or very close to the surface, the area where the aeromagnetic data is useful for interpreting buried features, of course, out in the basins where features are buried. So, we’re now with pretty high confidence. We are interpreting the buried features out here on the basis of the aeromagnetic data and the drilling.

One other piece of information that came out of this is we increase our own confidence that the basalt primarily intrudes along feeder dikes, structures that occur coincident with the north, northeast trending faults.

Next, please?

An example here of an analog volcanic structure. This is well to the northeast, 60 kilometers northeast of Yucca Mountain on the test site, east basalt ridge. And, what you see here, it’s easier to see in the handouts, I hope, a volcanic dike coming along to a conduit. This would have been the conduit that fed interruption at a depth that’s over 100 meters. The scale of this, this is perhaps a 75 meter outcrop there. This helps provide us more information to confirm our estimate of conduit diameter, which is a parameter useful and used in interpreting the amount of damage done by an eruption.

Next, please?

All right, here's the result of the PVHA update, which, again, this is available in the report from the PVHA update. But, just to compare things here, the blue curve here is the new result, and the black one is the old one. This is a cumulative density function display of the annual frequency of intersection here, the black being the old one, the blue being the new one. And, here, is a probability density function, a histogram of the 1996 numbers and the 2008 numbers.

What we see other than that, they're not really that different. Their basic pattern remains the same. These are the estimates of the individual experts all displayed here. The mean annual probability during the first 10,000 years is higher in the update, 3.1 times 10^{-8}, versus 1.7 times 10^{-8}. This is not a tremendously large difference. Yes, it's almost a factor of two, but those are small numbers. And, the newer results have a slightly broader distribution, a little more weight at both tails, a little more at the higher probability, and more at the lower probability end. So, what we see here is a somewhat broader treatment of uncertainty from the experts. But, by and large, a confirmation of the original estimates.

KADAK: Peter, could you explain those spikes? What are those suppose to mean?

SWIFT: Well, these are, this cluster here, or here, which appears in both, you know, ask the same question of Frank Perry when you have him in April, but these correspond to conceptual models adopted or by individual experts that would result in a clustering of frequencies at a relatively low level here. So, I'm not sure which conceptual model for volcanic activity in the region produced that, but the experts looked at the possible causes and explanations for volcanism, and established probabilities consistent with different conceptual models for it, and it comes out like this. So, no, I don't have a better answer.

I think I'm just about done. That's it? No, one more here.

The experts went ahead also and provided an estimate out to a million years, and the blue here, this compares, and you see here the individual expert's initials. You can back up and figure out who they were from the earlier slide. And, this is interesting because it does show the range of values each individual expert came up with. It also shows how their view of uncertainty changed when they were asked to go out to a million years. So, we'll pick CC here. For 10,000 years, that's the range, and the median and the means he comes up with. And, for a million years, it's a broader range. Out at a million years, the aggregate mean of the eight experts is again slightly higher than it was at 10,000 years, but is small. And, the uncertainty band is slightly larger at a million years, and largely reflecting one expert actually.

I will reiterate these numbers are not the numbers that are used in the License Application. The License Application has been submitted with the 1996 numbers, which are on the order of 1.7 times 10^{-8} per year for 10,000 years, and held constant for a million years.

Next, please?

And, that's my summary slide here. We are reporting these results to the NRC, and we are publishing them as the analyses are completed. We are not publishing results until we have completed analysis of them.

And, do you want to take questions from me now, should I take questions now, or should we move ahead to--

GARRICK: Well, maybe we could ask a question or two. I wanted to ask mine, and that is how do you make decisions as to what the scope of these analyses in these programs are? In some cases, it looks like you're addressing the License Application. In other cases, as you just cited, it's not related to the License Application. How is the scope determined of these programs?

SWIFT: The geotechnical activities are very strongly focused on supporting the information needed to evaluate the surface facilities. That’s in support of operations and preclosure activities primarily.

GARRICK: So, that’s design driven?

SWIFT: Yes, that’s design driven. We’re working closely with BSC, and we will work closely with the new M&O to meet their needs for evaluating the surface facilities.

GARRICK: Right.

SWIFT: The ongoing Performance Confirmation activities, the subsurface monitoring, the seismic monitoring, and the precipitation monitoring, frankly, these programs are already in place. They’re of relatively low cost to continue to maintain. It would be foolish to lose that source of information. And, the probabilistic volcanic hazard assessment update, it’s a multi-year commitment, which is coming to a close now.

GARRICK: Now, is that multi-year commitment is to whom, to the laboratory?

SWIFT: No, no, the commitment was made by the DOE to the NRC.

GARRICK: To the NRC.

SWIFT: The update was as the information indicated.

GARRICK: Yes. Yes. Okay. Thure?

CERLING: Yes, go to Slide 28. Just to clarify something, my interpretation and understanding of these frequencies, these are the averages of the information that's basically on the next slide, Slide 28, these frequency distributions; is that right?

SWIFT: Yeah, it's just different display of the same information. You could take the bars shown here--

CERLING: So, the 10 to the 9th spike is due to the analysis of MS and GT and AM on the previous slide?

SWIFT: I expect that's correct, yes.

GARRICK: Bill?

MURPHY: Peter, I'm interested in the status of the ESF at present. You showed a picture of some stability testing going on there, but are those just monitors that have been placed and they're being monitored remotely? Is the ESF open or is it closed?

SWIFT: It is not open except for very specific purposes. I'm going to ask Michael Schuhen to describe the conditions under which one would actually get in and how we're getting that data out.

MURPHY: Well, I have one specific question in that case. I'm very curious in the hydration state of the ESF if portions of it have been closed, and whether or not that is being monitored. Is it getting wet? Is it starting to drip? And, is the water being monitored?

SWIFT: And, presumably also the amount of ventilation would be of interest to you, too. Michael, do you want to field those?

SCHUHEN: The question regarding the hydration, I’m not sure on that answer there. They are making periodic re-entries to make these measurements as part of the assessment of the stability of the drift, even though the tunnel is generally shut down to normal day-to-day activities.

MURPHY: So, are you saying that the humidity or the hydration state of the ESF is being monitored?

SCHUHEN: I’m saying I’m not aware of whether or not they are monitoring that right now. I’m aware of the construction monitoring aspect of the re-entry only.

MURPHY: Just to follow up, I mean, when they go down there, is it wet inside?

SCHUHEN: Again, I’m not sure.

MURPHY: You’re not the one that goes in there.

SCHUHEN: I’m not the one that can answer that question.

SWIFT: Bob Jones, do you have anything to add on that?

JONES: No, I don’t really have anything.

SWIFT: Okay.

GARRICK: Any other questions? Russ Dyer, yeah.

DYER: Yeah, generally, I talked to Doug Weaver or somebody after the re-entries. We have no monitoring program per se for seepage or dripping moisture beyond observational, and nobody has mentioned that they’ve observed anything in the way of seepage, anything out of the ordinary.

MURPHY: Thank you.

SWIFT: Sorry I couldn’t do better on that.

GARRICK: Andy?

KADAK: When is the next scheduled entry to take a look at things around how the repository tunnel is doing? Do you have any formal program to just sort of keep an eye on that?

SWIFT: Yeah, I’m going to ask either Russ or Michael. What’s the frequency of the scheduled entries? This is Robert Jones, Bob Jones.

JONES: Yes, I’m with Sandia National Labs, Test Coordination Office.

We are making routine visits down to the Alcove 5 area on a monthly basis, but it’s in support of the seismic monitoring program. And, I can say that there have been no reports of any dripping or moisture in that location since we started that, and we’ve done now four entries in the last four months.

KADAK: Okay, thank you.

JONES: That’s about as much as I have.

KADAK: For Peter, let me see if I can summarize your findings as best I can. On the surface facility geological studies, you’re saying the results thus far in the pad area and I’m not sure where else you were drilling, and why, you’re seeing consistent results based on your prior analysis; is that correct?

SWIFT: That is correct. The main information here of interest would be the material properties that support the seismic hazard analysis for the surface.

KADAK: Okay.

SWIFT: And, to date, we have not found things that would lead us to believe that we need to change those analyses.

KADAK: My recollection at one of our last meetings was that there was an issue about the location of the storage pads.

SWIFT: Yeah. Could we have the map slide? I’m not sure which one it was.

KADAK: And, that had to be moved for some reason.

SWIFT: Yes.

KADAK: Could you kind of go over that again for us?

SWIFT: These are the locations as proposed in the License Application. These are the sites we want to license. The northern pad here has been moved since it was first proposed. This is the moved location. And, that was to avoid a possible subsurface trace of the Bow Ridge fault. But, that’s the first fault you come to as you go in the tunnel here from the surface, and the fault is on, topographically, on the back side of the hill here, and it runs up, earlier maps showed it running up this way, and drilling found it over in this area here. And, so, the aging pad was moved to the east. That was two years ago.

KADAK: And, how does that affect the seismicity of the soil structure interaction based on what you assumed before? Based on what you found now.

SWIFT: I’ll need Rich Quittmeyer to do this properly. But, the pad is not on the trace of the fault, and it is set back sufficiently, and it’s the material property of the alluvium at the location, not how close it is to the fault, that is of interest here. The set-back is sufficient that the seismic analysis for a pad location here, with a fault here, is essentially the same as it would have been for a pad location here, and a fault over here.

KADAK: But, the soil conditions are capable of handling the weights and loads without either what I would call accelerated motion of the, say, TADs, or other things such as maybe subsistence, I think is the proper word?

SWIFT: Yes. The answer is yes to that. But, for a more detailed discussion, I’m not the right person. That would be a design issue, and we would need our seismic hazard experts and our design people here.

KADAK: On the volcanism one again, if you go to that slide that says the variation of ideas about the size or the magnitude of the earthquakes?

SWIFT: Is this the third slide from the end?

KADAK: Not earthquakes, but volcanic events. The range of uncertainties of these estimates of those eight experts is really quite large. And, I'm not sure how you would interpret the difference between 3.1 and 3.8, or, you know, 1.7. I think it was 3.8 you said was the different, I mean, is that a significant impact relative to, say, the doubling of the risk, if you will? And, does that significantly affect the TSPA result, if you believe those changes?

SWIFT: As we have presented this to the NRC, we do not believe the changes are significant. We believe the original basis and License Application remains an appropriate basis for making the evaluation. And, you have to think here of what our long-term dose estimates look like. During those periods of the performance period when the total dose is dominated by the probability weighted consequences of igneous activity, then your first approximation, you can scale that total linearally with that, too.

KADAK: And, is that what you would need to do to accept these results as true?

SWIFT: Yes. I would note, though, that the peak totals, both at 10,000 years and at a million years, are not driven by an igneous event happening during seismic ground motion event. The effect, if we were to follow through with this and carry it into the total performance assessment, it would be a smaller effect on the total than a simple linear factor of almost two.

KADAK: Okay, thank you.

GARRICK: All right, I think we'd better move on. Thanks, Peter.

Our next speaker is Zell Peterman of the USGS. Zell has kind of had the lead of the geochemistry team at Yucca Mountain.

PETERMAN: Thanks, John.

This is sort of an update on a presentation I gave to the Board a year ago, almost exactly, on the status of the geochemical studies of dust at Yucca Mountain. I know I've got too many slides, so I'll try to work through them pretty fast.

A number of people in the Survey have worked on the project. At the present time, it's mainly Tom Oliver and me.

Next, please?

And, the reason, of course, that there's so much interest is the dust, subsurface dust and atmospheric dust contains a fraction of soluble salts, and some of these salts deliquesce and can form brines, which if on a waste canister, might facilitate corrosion.

Next?

Most of the underground dust is composed of ground-up rhyolite, as one might think, ground up during construction of the tunnels and during operation. It differs from purely ground-up rhyolite in the sense that some elements are, larger iron has been introduced. Enhanced CO2 and chlorine are from fracture minerals, are softer and more easily ground up. Organic carbon and chlorine probably from conveyor belts, and other introduced materials, and there's a certain amount of chlorine from pour water that evaporates at the tunnel, at the walls of the tunnels. Excess trace elements over the rhyolite, metallic elements that can be related to construction activities.

Next?

Okay, this just sort of summarizes some important parameters of the dust. ESF dust typically contains a half percent, on average, of soluble salts, and the range is about from .2 to 1.3, or something like that. The nitrate/chloride ratio, which is an important parameter for the corrosion folks, is relatively high, 2.2. ECRB dust doesn't have much salt, and I won't talk about that anymore.

Dust collected from natural collections at the surface we discovered had most of the soluble salts leached out. So, I won't talk about those either.

Protected dust, dust collected from structures, a wide range in composition. The most important dataset we have for the atmospheric dust is collected by a cyclone that's run by the M&O contractor, and it's been collecting for at least a couple of years, or more, now. And, we continue to get samples of that. And, then, there's a regional dust studied by the USGS, and it collects both wet fall and dry falls, so, the results aren’t directly comparable to what we’re collecting, and it has a 13 percent soluble salts.

Next, please?

KADAK: Excuse me. Could I just ask a clarification question? This is Kadak. Can we go back to that slide?

What do you expect to see in the repository over the time period that we’re concerned with?

PETERMAN: Well, you know, I just have my own personal feeling is that initially, you’re going to be dominated by construction dust.

KADAK: So, that would be the first one?

PETERMAN: The first one. And, I think you’re not going to clear out that dust. I mean, it’s going to be there. It’s going to be redistributed. It’s going to be every time a train goes by, or whatever, it’s going to be kicked up. In the long-term, I would expect there could be infiltration of atmospheric dust.

KADAK: Which one of those?

PETERMAN: I would say that would be the cyclone type dust.

KADAK: So, that would be sucked-in air?

PETERMAN: Right. And, even the natural breathing of the mountain, you know, is going to move some fine dust around. You know, any disturbance of the rock, you know, dust trickles down from fractures, and that sort of thing. And, that would be more like the ESF dust. So, I would say the ESF dust, or ESF-2, and atmospheric, are the two, I think, the two major components, and the proportion is going to change with time.

Next, please?

Our experimental studies, I'll try to be brief here, what we did, we could collect a lot of underground dust, you know, several hundred grams per sample. So, when we decided to heat the dust to see what happens to the composition, we mixed the sample very well, and we'd take 250 milligram aliquots, put them in separate containers, a number of them in an oven at 180, and then periodically pull out an aliquot and analyze it. So, that's what you're going to see shortly.

For the cyclone dust, we don't collect a lot of samples. It's something like a half a gram to a gram per month. And, so, you can't conduct these detailed geochemical studies on that. After the heating, or periodically, samples were removed and then leached with deionized water, and the leachates were analyzed.

We also have been doing some heating of pure salts, and there are no surprises there. There was a lot of information in the literature, and we're not finding anything new there.

The pure salt heating, we’ve taken the nitrates and the chlorides, the likely ones--we have done some work on ammonium chloride, but I didn’t include it here. The results are a little bit ambiguous. Individual salts heated up to 180. We lose moisture and nothing else.

Mixtures of salts, we get some, you know, liquification. There’s some lowering of melting points if you mix several salts together, lose moisture, but no nitrate. And, the same with the nitrate salts, plus chloride salts. So, you can find all the same sort of stuff in the literature.

Next?

Now, here’s ESF dust. Long-term heating. And, again, this is, if nothing changed, the ratio concentration of the sample over the concentrate, everything would fall along the one line, and that doesn’t happen. What you see here is the chloride content, and I’m talking soluble chloride now, water soluble materials, water soluble salts. So, the amount of soluble chloride actually increases early on very quickly, and then gradually decreases.

CERLING: Zell, why doesn’t everything start out at one on the chloride, C/Co at time zero?

PETERMAN: It does. It’s just you can’t see it very well.

CERLING: Okay. So, there is a zero there, actually--

PETERMAN: Right, it’s all tucked together. We think this may be, you know, there’s a lot of ground-up conveyor belt in the dust, and the conveyor belt has a lot of, 40 to 50 percent chlorine, so, we think this may be, you know, somehow it’s being converted to a soluble form.

Sulfate, very systematic, increases slightly, and then tapers off. And, again, there’s sulfur in the rubber. The important point is that the nitrate decreases within the first 200 hours, you’ve lost 70 percent of the soluble nitrate from the dust, and then it plateaus out. Now, the few samples of atmospheric dust, or cyclone dust, that we’ve done are generally consistent with these trends. And, those are shown by the triangles. We don’t see the big enrichment in chloride in the cyclone dust, which is another argument that, you know, we don’t have ground-up conveyor belt in the cyclone dust.

Next?

Let’s see, this is changes in some of the cations, and, importantly, changes in the ammonium concentration, which drops off really rapidly early on, and then plateaus out. Calcium increases a bit and plateaus out, as does magnesium. We don’t really understand why the amount of soluble calcium is increasing. It’s not a whole lot, but it’s about 20 percent.

Next slide?

We do analyze for some organics. Formate doesn’t do much. There’s a little blip there early on, and then it plateaus out at about the same concentration it started with, whereas acetate increases. These are, I should have mentioned, there’s two samples here I’ve taken as representative of a much larger dataset. If I put everything on here, it would just be pretty much unintelligible.

So, the two samples I’ve chosen are quite different in formate, but the amount of the organics increases with increasing temperature, and then plateaus out.

Next slide?

These are possible reactions for getting reduction of nitrogen and nitrate to some other form where it could be lost from the system.

Next?

Okay, this is a log-log plot of nitrate versus chloride. Again, the soluble fraction, in this case, before and after heating. You can see the cyclone dust has a ratio, averages unheated, has a ratio of about ten to one. But, it loses about two orders of magnitude nitrate upon heating. It goes from here down to here. ESF dust goes from the blue circles to the blue crosses, and the ECRB dust the same thing.

This is a drift scale test, and we didn’t have enough sample to do, you know--well, let me back up. The drift scale test was heated by the drift scale, so, we don’t have the starting material.

Anyway, the next slide shows the data, the same data as fields. Now, what it does is track the change in the nitrate to chloride ratio for these two samples with increased time in that heating experiment. And, early on, you get this increase in chloride relative to nitrate, and then the decrease in nitrate takes over. So, this is just an example of what happens during the heating experiment.

So, by and large, the bottom line is you’re losing nitrate. It seems to be soluble nitrate making that statement.

Let’s go to the next one.

So, what we have observed then, just to summarize, the nitrate to chloride ratios are reduced one to two orders of magnitude. Soluble sulfate increases a bit, and soluble chloride increases by a factor of two to four, and then gradually decreases. So, it’s the nitrate to chloride ration that’s of interest to the corrosion folks, and I’ll leave it at that. I can’t comment on, you know, what the implications are because I’m not a corrosion person.

GARRICK: I’ll ask my good friend Ron here what the implications are from a corrosion standpoint.

LATANISION: Well, I have been asking for a couple of years now for a compelling argument to demonstrate that the nitrate to chloride ratio will provide protection over some period of time. And, in fact, what I'm seeing is a compelling argument to the contrary. I'd like to hear from the Project. Peter, or is Doug Wall here today?

HARDIN: Ernie Hardin, Sandia. I'm not going to try to speak for Doug Wall on the corrosion issues. I just wanted to point out something that we probably discussed in the past, which is that when you knock down the availability of nitrate in that micro-environment, you also significantly decrease the volume of brine that can be produced, even if all the salt is taken up in moisture. So, remember, nitrate is a required constituent of a multi-component brine that can exist above 120 degrees C. With the nitrate out of the system, the brine goes dry above 110.

LATANISION: You're arguing that it's going to stifle because it's dry?

HARDIN: Well, I'm making the observation that this significantly reduces the amount of brine available at all temperatures above 110 degrees. So, that our previous arguments, you know, rely on brine volume, or salt abundance, are supported by this.

LATANISION: But, you also rely on a nitrate/chloride ratio that provides protection, and you've done a fair amount of work to show there are some regimes of nitrate to chloride ratio where protection does seem to occur. This seems to be headed in the wrong direction, independent of the volume of brine issue. It’s not a very compelling argument, and I was curious if Doug was here, because when we visited Sandia a few years ago--maybe a year ago now, I’m not sure. It’s something like that. He had the facility in his laboratories to examine this question in some detail, and I understood there was experimental work underway to look at the change in nitrate to chloride ratio as a function of time. Try to understand some of what I think Zell is showing here. Is that happening?

HARDIN: That is happening, but I’m not prepared to give you a status or any results.

LATANISION: Mr. Chairman, can we get that on our agenda for next time? This is a very important piece of information, and I think it, you know, I don’t mean to be trite about it, but, you know, I honestly think it is a compelling argument in a contrary sense, and that’s not very comforting from a corrosion engineering point of view.

GARRICK: Sure. David, and then Thure.

DUQUETTE: Duquette, Board.

Zell, the discussion that just went on obviously is important to those of us in the corrosion world, and the comments that Ernie made really refer, I think, primarily to nitrate/chloride brines, but there are other components in these brines, sulfate, and so on and so forth.

The question I have is has anyone measured the deliquescence temperature of the new brines that you’re developing? You’re developing basically some, by heating it up, it’s a different composition than it was before. Have you determined, or even calculated what temperature that will deliquesce at? Because that, I think, is important to Ernie’s comment, because if it drops the temperature enough, it’s a different kind of concern, but one that we might still be concerned about.

But, the fact of the matter is that until we know what the new deliquescent temperature is for the new salts that you produced by heating, then we really don’t know where we are relative to the nitrate/chloride ratios, because it’s not just nitrate/chloride, it’s temperature related, it’s other ions related, and a whole bunch of other stuff.

PETERMAN: We haven’t done that, but Livermore, you know, did quite a bit of that work earlier, and there’s some very nice papers in the literature out there, and I think one could go to those and then make some sort of statement about that. I have not done it myself.

HARDIN: Hardin, Sandia.

To respond to your question, we have done TGA, thermal gravimetric analysis, and related investigations of the decomposition of calcium chloride salts with a temperature above the humidity trajectory that is representative of its exposure in the repository environment. We replicated, or I should say corroborated the work, earlier work done at Livermore in the new lab at Sandia that Dr. Latanision was referring to. So, we believe that calcium chloride salts will decompose to less deliquescent hydroxy chloride species. The same is true of the mag chloride salt, should they exist in that environment. So, that’s where we are with the understanding.

What’s left is the monovalent chloride salts, sodium chloride, potassium chloride. If the calcium is pulled out as a sulfate, calcium sulfate, it precipitates readily, it has relatively low solubility, doesn’t participate in the highly deliquescent brine systems. There’s really not much left, is my point.

And, I think you asked about whether we had calculated these relationships, and the answer is yes. And, we can certainly elaborate on that if we need to.

DUQUETTE: Duquette, Board.

I’ve seen some of your calculations, and I have to agree with them. For the most part, they’ve been calculations and experiments done on salts that contain primarily nitrate and chloride, and different cations. What Zell is showing is that there would be actual dust, there are other components in those salts, and it seems to me it’s a fairly easy set of experiments to conduct if you take the salts that Zell is producing and find out specifically what the deliquescent temperature is. Because, yes, I know about your work with calcium chloride, and I agree with the work that you've done on the calcium chloride/nitrate ratios, lowering the deliquescent temperatures for binary salts, and even for some ternary salts. But, now, we're looking at some fairly complex salts, and I don't know what they're going to do. I suspect the trajectory you're talking about is probably correct, but the fact of the matter is that we've got these materials because they've been produced by Zell, and it seems to me it's an easy experiment to see at what temperature they deliquesce. Yes? No?

HARDIN: Yes, there are experimental opportunities here for sure. And, as far as, you know, what these brine systems do if you take the nitrate out, you can certainly extract that from our published work where we've gone to low or zero nitrates.

GARRICK: Thure, did you have a question?

CERLING: Yes, Cerling, Board.

Slide 8. I'm just puzzled why, for instance, the nitrate seems to stop reacting, and it's almost as if in that experiment, you've kind of run out of substrate, or something, that it's reacting with. Do you have any sense of why that is?

PETERMAN: Well, you know, we don’t understand that nitrate loss either. A lot of it happens very early on. You’re losing about 70 percent, you know, within the first 200 hours, and then it really doesn’t change very much. Now, if we carried this out another, you know, doubled the time, I don’t know if there would be a gradual decrease or not. If it’s nitrate reduction, you might be running out of the whatever is causing that reaction. I don’t know. So, the answer is I don’t know.

CERLING: Okay. No, I just wondered if you had any opinions, or if you had a sense of what might be happening, because it’s just a curious set of reaction curves.

PETERMAN: There’s lots of additional things we could do to try to understand these chemical reactions. And, you know, we haven’t done them. We haven’t looked at, say, the, you know, taken the samples and looked at the gas that may be evolved, that sort of thing.

GARRICK: I think, Bill, did you have a question?

MURPHY: Yes. Bill Murphy, Board.

You seem to have shown chemical characterization of the dust from the ESF and from your cyclone. Have you done a mineralogical examination of it?

PETERMAN: We’ve tried to do that, mostly with an SEM, and the problem is with our SEM, you can’t see nitrates. So, you can see chlorides and chlorine and sulfur. So, we have identified, you know, sodium chloride, potassium chloride. We’ve got a list of minerals, natural alunite, and I can’t remember the formula for that. Of course, there’s calcite, which would be somewhat soluble. So, we don’t have a good tie on how these salts occur naturally. We have taken a few samples, evaporate them and dry at low temperature, and then an XRD on the salts. And, I think that was reported in my previous presentation. And, you know, you get sodium, potassium, chloride, you do get nitrates. We’ve got ammonium, salimoniac, the typical evaporate type minerals. And, that’s about where we stand.

We’d love to find a way to better identify what the actual minerals are in the salt, and other folks have done that. There was some work at the University of Utah, Time of Flight efforts, that seem to indicate they could identify the actual salt minerals. We haven’t pursued that either, although I’ve talked to the investigator on that.

MURPHY: But, it’s possible in any case that nitrate may have a variety of mineralogicals?

PETERMAN: Absolutely, yes.

MURPHY: Thank you.

GARRICK: Mark?

ABKOWITZ: Abkowitz, Board.

Myself, and I’m guessing the vast majority of the audience is not capable of getting down into these weeds, but

I’d like to try to summarize what I believe is the message behind this, and put this in the context, and tell me if I’m wrong.

Localized corrosion is really the subject that we’re talking about here, and at the present time in the Total System Performance models that DOE has used, they have FEPed out localized corrosion. The Board, for some time, has been questioning that assumption, and has basically challenged the Department of Energy to demonstrate the logic behind being able to FEP that out.

One of the key arguments in that, from the Department of Energy’s standpoint, was that the nitrate to chloride ratio was sufficiently high to preclude the likelihood of localized corrosion being a problem of the probability that makes the threshold.

Your experiments are demonstrating that everything else being equal, that the nitrate to chloride ratios are quite likely going to be a lot lower than were assumed. And, so, at this juncture, the evidence that we have to date is that the assumptions that were made to FEP this out are being questioned. Is that a reasonable spot that we’re at at this point in time?

PETERMAN: I’m confident that under the conditions we used, using natural dust samples, not something fabricated, that the dust, if you heat it to 180 degrees for a prolonged period of time, loses a substantial amount of nitrate. That’s as far as I can go.

HARDIN: Zell, if I could just add to that reply?

PETERMAN: Sure.

HARDIN: This is Ernie Hardin with Sandia.

Yes, it’s important to recall that our screening argument does allow for the possibility of aggressive brines to form in minute quantities from deliquescent salts and dust. So, it’s not that we have somehow shifted that position here. It’s the same position.

And, I want to reiterate the point I made earlier to Dr. Latanision, which was that the brines are much smaller. What’s embedded in that statement is that the compositions are the same, because we are arriving at a eutectic or paratectic relationship here. And, so, there’s only so much of the nitrate constituent to participate in a dissolution that requires nitrate in order to maintain a brine, an aqueous phase.

GARRICK: Yes? We’re running behind quite substantially, but this is a continuing issue that the Board has had, and it’s a little bit frustrating that we can’t get a resolution of it. And, now, we seem to be saying that with the nitrates gone, we have another phenomenon that’s taking place that offsets the disadvantage of the disappearing nitrates, and that’s dry-out. And, I somehow think that the

Board needs to get a clarification of this whole issue because it is, as Mark indicates, the core driver for our position and our reviews of localized corrosion. And, it could have at least a material impact on the claims that are being made about the lifetime of the waste package, and I guess I don’t quite understand why we can’t come to grips with it, and get a more clear account of just what is happening there. And, I think we need to push for that to happen.

I know the Board is proposing to go visit Sandia and see what’s been going on, and hopefully, out of that, get some assurance that this problem is better understood. But, I’m really surprised that it continues to linger, and it’s so fundamental to the whole issue of containment that we can’t somehow address it in a way that we know exactly where we are. So, I guess we have to ask ourselves as a Board what is it that we would like to see happen in order to clarify this whole issue.

I don’t know, Ron, if you have any suggestions on that. You know, when we talk about something disappearing, there’s something else that’s put forth as providing the necessary protection, such as dry-out. And, where does this end?

LATANISION: Well, this is Latanision, Board.

Mr. Chairman, when we visited in Sandia and examined or explored the test chamber that had been set up by Doug Wall and his colleagues to do some analytical work, it just seemed to us a natural vehicle for addressing the question of what happens to the nitrate/chloride ratio as a function of time. And, it's just a gas analysis. It doesn't seem to me to be very esoteric.

It's just a matter of doing an experiment, collecting the gases and analyzing them. That test chamber seems to me a perfectly suitable vehicle, and it would require some analytical capabilities, but they're not obtuse. They're straightforward analytical techniques. This is an experiment that could and should be done.

GARRICK: Well, I think we need to address it, and get it on our agenda as quickly as possible, and have the Board at least be able to take its own position on this issue.

Okay, well, we are running behind, something we don't usually do. We usually keep on a little better schedule, but this has been very interesting.

Our final speaker on the science part update is John Whitney. John has been chief of the tectonic and erosion studies for Yucca Mountain during the Nineties, and he's currently the Yucca Mountain project chief of extreme ground motion studies. He administered the probabilistic seismic hazard analysis for Yucca Mountain.

So, we're pleased to hear from you, John.

WHITNEY: Thank you. The first slide?

We performed, the project performed a probabilistic seismic hazard analysis of Yucca Mountain about twelve years ago, and as part of that analysis, the NRC requires that the annual probabilities of exceedance go out to 10^{-8}, and, so, for ground motions that would affect Yucca Mountain at 10^{-6} through 10^{-8}, these ground motions came up with modeled velocities that were greater than anything that’s been recorded around the globe in our seismic catalogs, as high as 13 meters per second.

So, we wondered, you know, because the seismic hazard curve is used in seismic design, if we could find a technical physical basis for limiting extreme ground motion at Yucca Mountain. Yucca Mountain is a good place to search for evidence of extreme ground motion because the geology preserves, in its surficial deposits, quaternary deposits, evidence back over a quarter of a million years. And, in fact, in some of the tectonic trenches, there were five or six of them along the Solitario Canyon Fault here, soils were dated from 200,000 back to 800,000 years. So, there’s quite a bit of old alluvium and colluvium that still remains on Yucca Mountain.

Next slide?

So, we decided to take advantage of an inventory of surface effects that exist nowhere else in the world, which is the Northern Nevada Test Site. On Pahute Mesa, between 1962 and 1992, there were 85 underground nuclear explosions. We chose to look at one particular underground explosion in detail called Rickey that was shot in 1968. It’s right along the highway, and you can see the effects from this particular event very easily.

The PGV, the ground velocities that were calculated for Rickey are the left side of that table there, go up to about 168 centimeters per second, 1.7 meters per second, and for the other larger event called Pool, they go up almost to 2 meters per second velocity. That would place these two events in the--it would be the second and third largest earthquakes ever recorded anywhere globally in the world. And, the accelerations for these particular events are two or three times higher than anything that’s been recorded in the world. So, these are examples of extreme ground motion that we can look at at the Test Site.

KADAK: Are these horizontal, horizontal velocities? What’s the direction?

WHITNEY: This is from the actual shot, so there’s a strong vertical component in these things, primarily vertical.

KADAK: So, how do you measure the velocity? What’s the factor? What is that velocity in terms of a direction? Is it horizontal, vertical, or just--

WHITNEY: It’s the actual angle that’s been modeled from the actual depth.

KADAK: I see. Okay.

WHITNEY: From the actual recordings back in the Fifties and Sixties.

KADAK: And, the same with the acceleration?

WHITNEY: Right. And, those were all done by Sandia way back when.

So, one of the problems and challenges was what aspect of the extreme ground motion would be preserved on the landscape long enough that you might see it a long time into the future. And, we really only found one effect.

Next slide?

And, that were these massive rockfalls that were created along this particular cliff. And, if you look out in the lower left corner, you can see two people, one of them Jim Breun (phonetic), has a scale showing that these boulders are between two and three, some of them even four meters in diameter there, creating a rather large volume of material.

We were able to secure the pre and post-shot photos that were taken, and were able to make digital elevation models for several of the knolls on this particular cliff. The shadowing of this particular cliff existed over a kilometer and a half, and that’s fairly important, a fairly large, wide area there.

At this particular knoll, we calculated 2200 cubic meters of rock were shattered from that cliff during just the first UNE. And, over a total of a kilometer and a half, about nearly 11,000 cubic meters of rock were released in that particular event.

KADAK: How far is this from the shot?

WHITNEY: About a half mile. So, if these ground motions are between one and a half and two meters per second, if we look at the work that Joe Andrews has done from the Survey, which is to calculate, as a dynamic model, the maximum amount of ground velocity that could be generated on the Solitario Canyon Fault adjacent to Yucca Mountain, that would be 3.6 meters per second. So, the surface effects that you're looking here are well within the capability of the fault at Yucca Mountain.

Next?

One of the challenges would be how would you distinguish an energy generated deposit from a natural deposit, climatically generated deposit due primarily to weathering? Well, there's a distribution difference that you see that you can measure and describe for the slopes. But, what we were looking to prove was that the age of the boulders would be oldest at the base of the slope, and become youngest up against the cliff. So, we chose the cosmogenic dating technique Beryllium 10 analysis were done by colleagues at Lawrence Livermore, and we did a dozen analyses at this particular slope, which is of the same rock that you saw that was shattered, which is just literally about less than ten miles away. This is the ammonia tanks formation here, and it has a fair amount of quartz in it, so it’s a good candidate for Beryllium 10.

I’m sitting down there in the right corner, and there’s a boulder behind me that’s about four meters in diameter that has an accumulation of Beryllium 10 in it that’s greater than what we were able to find at the top of the cliff. So, that boulder gets to be used for the rock erosion rate. But, the minimum age for the boulder at the base of the slope is over 530,000 years. If you assume an erosion rate on that rock as well, it becomes greater than 700,000 years.

So, even up here at 6,000 to 7,000 feet where the weathering processes are a little more accelerated, especially during glacial times, you’re still preserving evidence over half a million years old on these hill slopes.

Next slide?

The distribution of ages on this particular slope goes from about 8,000 to 250,000, which is off my graph here, but most of the deposits, the ages of the boulders fall into the time zones associated with glacial episodes. So, they are actually being weathered out during these glacial episodes, and they back up and they become younger all the way to the top. So, that's a pretty good signature, a climatic signature on these slopes.

Next?

We performed the same kind of age dating profile in Crater Flat, about seven miles west of Yucca Mountain, on the same rock, the ammonia tanks formation, that doesn't exist on the top of Yucca Mountain. And, as you can see, the ages go from 35,000 down to a quarter million, 880, to 1.3 million years old. So, the ages of the rocks at the base of these slopes is actually older than those higher up. This is about 3,000 feet in the dry semi-arid climate. So, the weathering rates down in the desert zone here are much, much slower than that up on Pahute Mesa, which is why we're preserving evidence for a much longer period of time. But, this is suggesting that if there was an event with extreme ground motion involved, we might be able to find evidence of it back to nearly a million years.

Next?

This is a view of Yucca Mountain crest, and as you can see on the right there, we don't see any large volume of skree or tailis at the base of the slope. We see some boulders that have weathered out. But, we don't see a large accumulation. And, then, along this particular cliff, and down in the middle part of the cliff, we have found over a hundred of these precariously balanced rocks. These are somewhat fragile features here that would be shaken down with about two-tenths, or three-tenths of a G. So, if we can date these features, then we can actually say something about what kind of quiescence has existed for what period of time on Yucca Mountain. And, we’re in the process of doing that. The same with the cliff. We’ll be able to say for what period of time there has been no extreme ground shaking at Yucca Mountain itself.

And, then, lastly? Next?

This is a view of that northern cliff on Yucca Mountain, and coming down across its bedrock slope onto a bedrock pediment, where you see boulders strewn with not quite a random pattern, but certainly not a large amount of debris.

It would have been nice to have all the results to tell you the age of these deposits, but the Tiva Canyon unit here actually contains very, very little quartz. And, in our efforts to date this particular unit, using Beryllium 10, we could not come up with defensible rigorous analysis for these particular rocks. So, we have changed our dating method, and now all the rocks are being re-analyzed, about 45 of them, for Chlorine 36 dating on the feldspars. And, that work is underway right this minute. So, by the end of the year, we should be able to tell you for what period of time that you’re looking at here, we can definitely say that we do not see evidence of extreme ground motion on Yucca Mountain.

Thanks.

GARRICK: Go ahead, Ron.

LATANISION: Latanision, Board.

As a non-geologist, maybe I’m missing the punchline, but isn’t the ground motion a function of how close the source is to the site you’re exploring? If the same event that you looked at at Rickey were to occur closer to Yucca, would that not have a different consequence?

WHITNEY: Well, this is a normal fault here. So, the nucleation would be at some depth basically, like that. And, we can model that particular amount of maximum energy that would be released, and that’s actually been done for the largest paleo-earthquake on Yucca Mountain.

LATANISION: I see.

WHITNEY: And, it came out about 1.4 meters per second, almost the same as that that we had at Rickey.

And, something I forgot to say was that the response of a hillslope and a rock to these extreme ground motions is highly dependent on the properties of the rock. These densely welded tuffs crack and shatter in a way that creates large boulders. When you get down into the moderately welded and poorly welded tuffs, you do not get that volume of material. So, the similarity in the density and the composition of the two tuffs at Yucca Mountain and on Pahute Mesa, they are very similar and it makes a decent analog.

LATANISION: All right, that's the punchline I was looking for. Thank you.

GARRICK: Yes, Ali?

MOSLEH: This is Mosleh, Board.

I have a question regarding your numerical line for calling something extreme ground motions.

WHITNEY: Well, as part of the project in a different task, John Anderson from the University of Nevada Reno compiled the ground motion characteristics of the hundred largest earthquakes globally. And, of those hundred that he has described, the largest PGV, peak ground velocity, is 3.1 meters per second. The next ten are between 1.6 and 1 meter per second. In accelerations, there are a few accelerations up to 2 meters per second, but most of them are around--there's only 25 above 1 meter per second, or 1G. So, actually, we're saying extreme ground motions are just beyond these values that have been recorded globally.

GARRICK: Andy?

KADAK: Kadak, Board.

Just an observation. You know, even if Yucca Mountain doesn't get built, this is great stuff, and you're probably having a good time.

WHITNEY: I’m enjoying this, yes.

KADAK: Good, yes. Now, the serious question. I want to try to get the message very clear. The underground explosions at Rickey you’re saying are something you might see at Yucca Mountain in terms of maximum velocity and maximum acceleration?

WHITNEY: Right. The USGS team in Menlo Park had modeled the displacements that we measured in the 1990s. That came up with a maximum of about 1.44 meters per second. And, then, they made some, they took the largest earthquake in the Basin and Range history, which is about 5 meters displacement. They modeled that, and came up with a ground motion that was equal to about what you might see in a million years on Yucca Mountain.

So, we have some idea what the maximums are and what we might anticipate. That doesn’t mean that when you have an earthquake on that fault, that you will have the maximum. And, quite honestly, the global earthquake dataset contains thrusts and strike slip faults which have much more stress released in them than these normal faults at Yucca Mountain. So, actually, extreme ground motions for normal faults are actually very rare.

KADAK: Okay. So, to correlate that with this boulder analysis, you’re saying that you don’t see historically any such ground motion, given the rockfall that you’ve been looking at?

WHITNEY: That’s right. And, what I hope to be able to do is to say over what period of time I can say that for.

KADAK: And, as I recall, the number was around 500,000 years?

WHITNEY: I think that we may see some numbers like that here.

KADAK: Are you able to correlate the fall to the magnitude of the earthquake?

WHITNEY: Well, probably my impressions and interpretations at the moment are that most of these boulders have weathered out during these glacial periods in the past. But, since we’re down in the desert zone, not as much material weathers out, and, so, you have to actually look at the intensity of each weathering cycle.

KADAK: Right.

WHITNEY: And, we know that some glacial times were more intense than others. So, I will be looking at those correlations.

KADAK: Now, has your study been incorporated in the seismic hazards analysis group of experts, or not?

WHITNEY: This task, which is the ground based task, along with several other modeling exercises, and John Anderson’s work, the modeling of the precarious rocks by UNR, are a suite that will be used to improve the seismic hazard curve, which can be used eventually to either revise the probabilistic seismic hazard analysis, or some aspect of it, and that may be used in the final seismic designs.

KADAK: But, to date, that has not been done?

WHITNEY: That has not been done yet. The final reports for the extreme ground motion work will be probably started this fall.

KADAK: Thank you.

GARRICK: Yes, go ahead, Ali.

MOSLEH: This is Mosleh, Board.

So, the potential effect of this thing would be on the tail of any seismic hazard curve?

WHITNEY: That’s right, the tail.

GARRICK: I think the interest of the Board is having confidence that the earth science component of the seismic issues is properly transitioned into the structural engineering, from an earthquake design standpoint, because there’s not much evidence on the basis of the current surface facility designs that some things like the absence of extreme seismic motion has been taken into consideration, at least the absence of evidence. So, that correlation doesn’t seem to be very evident to us between the seismological perspective, and what I might call the structural engineering perspective. And, I assume you’re going to try to do something about that.

WHITNEY: It’s not my world.

GARRICK: Well, I know. And, everybody says that. So, what do we end up with? We end up with four foot thick walls when we could get by with maybe two foot thick walls.

WHITNEY: Well, that’s why this is being done.

GARRICK: Right. Well, we haven’t seen much impact.

WHITNEY: I’m under the impression, but I have no specific information, that some aspects of PSHA will be revised in the future. But, I don’t know of any time table on that.

GARRICK: I see. Okay.

Any other questions from the Board? Okay, yes, David from the Staff.

DIODATO: Diodato, Staff. Thank you very much for your presentation.

It’s an interesting approach trying to establish upper limits for a strong ground motion at Yucca Mountain. And, as I’m sure you’re aware, some people have pointed out that, well, this only establishes ground motions that have not occurred and, therefore, it might be somewhat conservative. Do you have a response to that criticism or observation that people make? Or, do you care to respond to that observation that these might be artificially conservative, still, because these ground motions have not been realized?

WHITNEY: Right. But, these will be just a few data points on the seismic hazard curve. We’re also putting an emphasis on those precarious rocks.

DIODATO: That’s what I was referring to.

WHITNEY: Those are unexceeded ground motions.

DIODATO: Right. Exactly.

WHITNEY: Which would give you the lower part of the curve. And, then, there are other pieces of geologic information, like do lithophysal get crushed at certain depths, you know, they would respond to a certain amount of ground motion? There is the Calico Hills tuff, you know, has properties that would cause collapse under certain amounts of ground motion. So, we’re creating a large curve of exceeded and unexceeded ground motions that we can use to try to improve that hazard analysis.

DIODATO: Thank you. And, just for clarification, I guess, what I understood you to respond to the other question was that the report of the extreme ground motion working group will be out in 2010. Is that still the schedule for that?

WHITNEY: I’m not catching the time table?

DIODATO: The report will be out in 2010? You’re starting on it this year?

WHITNEY: The schedule, as far as I know, is that my part of this particular study is hopefully done at the end of this fiscal year. But, that goes into a larger report that will be written in the following year where all these different activities are integrated.

DIODATO: So, I guess, the only final question is are you finding any values here that are significantly different from where you were three or four years ago, in terms of the magnitude of strong ground motion at Yucca Mountain?

WHITNEY: No, we have found nothing, and, in fact, you could even argue that we are finding some evidence that maybe the slip on some of these faults is sympathetic. They're not individual earthquakes. And, so, maybe we've actually over-estimated the behavior and activity on some of these faults.

DIODATO: Thank you.

GARRICK: All right. It's supposed to be 9:55 now, and it's 10:30. So, we have a bit of a problem, but I don't think it's severe enough to cancel the break. I think we'd better take a break for ten minutes. Thank you.

(Whereupon, a brief recess was taken.)

GARRICK: I think we have to carry on. So, I think we'll have Mark Board talk to us. He has talked to us before when he was an employee of BSC, and now he will be talking to us as a consultant. So, Mark, pleased to have you back.

BOARD: Thank you. Could I have the next slide, please?

The talk I was asked to give today by the Board was a summary of the work that's been going on over the last few years on looking at the stability of emplacement drifts with thermal stresses and with seismic loading.

And, an outline of what I’d like to talk about today is, first, give you a summary of the general approach that we’ve used to do these calculations. I’d like to talk about how we’ve estimated the mechanical properties and strength of tuff for the two predominant types of engineering materials that we have there, which is classified as nonlithophysal rock and lithophysal rock.

I’d like to review the numerical model that we have developed for doing the drift stability assessments, and how we validated that model, and then summarize the results of the predictions that we’ve made for thermal stability of these tunnels, and also the seismic stability of the tunnels.

What I would like to do is, on the nonlithophysal rock, I’m going to talk primarily just about the thermal stability of those tunnels, not so much about the seismic stability. It’s not quite as interesting, I think, and I do have some backup slides if that question comes up. I would like to spend most of my time talking about the calculations that we have done for the lithophysal material, since it’s the most important aspect of the repository. Okay?

Just to review, I’m sure you’ve seen this slide before, but the repository drifts are on a horizontal plane and the various tuff units of Yucca Mountain dip fairly gently toward the east. And, so, any horizontal plane is going to cut through various units of the Topopah Spring, which is the repository host horizon.

And, so, this plot shows, of all the emplacement drifts, how much of those emplacement drifts are located in which portion of the Topopah Spring formation. And, as you probably recall, I didn’t make a specific slide for it, the Topopah Spring consists of two basic rock units. It’s the same flow, but one of those units is called nonlithophysal tuff. It doesn’t have physical cavities in it, where as, the lithophysal tuff has physical cavities in it. So, structurally, or mechanically, they behave differently, and that’s why we have separated them out.

The matrix material of both of these is the same material. It’s just that one essentially has porosity in the form of these voids that are generally around 10 centimeters, to even up to around a half a meter in dimension in size, whereas, the nonlithophysal one only has essentially grain to grain porosity, so it’s much lower porosity than the other.

About 85 percent of the repository drifts are located in this lithophysal unit shown in red. And, about 15 percent, that in green, is in nonlithophysal rock. The depth of the repository is approximately 300 meters. I think the maximum depth is around 325 meters, something in that range. So, it gives you a picture about the overburden depth. And, based on that overburden, we can estimate what the most important stress component, which is the major principal stress, is about 7 ½ to 8 megapascals. That is simply based on the gravitational weight of the material overlying that.

Okay. First, I'll tell you a little bit about the nonlithophysal tuff and the studies we've done. The nonlithophysal tuff, I know you have all been underground and looked at it, it's a good quality, and I mean that from an engineering standpoint, excavation quality material. It's a fine grain material that's quite a strong rock with strengths of the, compressive strength of the material up around 200 megapasals.

It's cut with a series of fracture sets, and we have mapped approximately four well-developed fracture sets in the material. The U.S. Bureau of Reclamation, when these tunnels were being driven, actually mapped every fracture that had a continuous length greater than 1 meter. So, there is an enormous database of every fracture being mapped, its orientation and geometry, how its ends terminate in the space, and what kind of fracture filling material, if any, there is on those fractures. It's certainly the biggest database I've ever seen of that type, and it's very high quality.

One thing very important to drift stability is that these fractures are relatively short in trace length. And, by short, I mean that if you stand in the tunnel, you cannot trace a fracture trace that goes completely around the tunnel. They are typically around a few meters in length, and they terminate either against another fracture or, in many cases, just they start and terminate in solid rock. So, they are cooling-related fractures.

In a rock this strong, with stress conditions that are much, much less than the strength of the rock, typically what controls the stability of a rock like this nonlithophysal rock is movement of material, blocks of material out of--that are formed by joint planes. So, if you create an excavation, you see these joint planes intersecting in the roof and sidewalls. Sometimes, you will hit a point where you’ve formed a wedge that is free to move into the opening. That’s typically called a key block in engineering terms. And, we’ve seen a very few of those in the tunnel out there, but they do exist in some places.

The database for describing the rock properties consists of--we have approximately 500 compression tests that have been performed over the years on this particular unit. On the tuffs in general, they are well over, I don’t exactly know, I think they’re somewhere on the order of between 1,000 and 1,500 compression type tests that have been performed under all basic environmental conditions, including temperatures to 200 degrees C, and some even higher, and saturated conditions as well.

The strength of the fractures has been determined by direct shearing tests on joints that have been drilled through very large cores, 12 inch diameter cores, with joints at an angle. So, it’s shearing a large section of the joint, and determining its frictional properties.

As I mentioned, the rock strength is quite high, 200 megapascals on small diameter two inch samples, which is the typical thing that’s used to determine rock strength. We have estimated the strength of rock blocks, which is strength on a larger scale, because of the defects that occur in it naturally, has some size dependency of its strength. And, we have estimated that to be between 70 and 75 megapascals, and I will show you something later where we basically calibrated that, or verified that based on the results of the drift scale heating test that was done here a few years ago.

Okay. I’m not going to go into this in detail. I put it in there mainly so that you could look at it. It gives you the overall way that we have assessed stability of tunnels in the nonlithophysal rock. Because, as I mentioned, the rock is quite strong with respect to the stress levels that are applied to it, the main features of importance are the fractures themselves and how those fractures form blocks of material that can potentially become unstable, particularly under seismic shaking.

So, we spent a lot of time doing a stochastic fracture representation of the rock mass based on all that very large database of fracture measurements that was done by the U.S. Bureau of Reclamation. And, we used that fracture information along with laboratory tests of the intact and block strength measurements that we have done, and we use a technique called a discontinuum numerical method. It’s something I think mostly, it’s used in a lot of fields, but I think in rock mechanics in particular, it’s used in which we physically separate a body into a solid, that is, in which the blocks are separated by joints upon which you can have slip and shearing and opening on those joints. So, it’s a numerical technique that explicitly includes the effect of the joints.

In our case, it’s a three dimensional model. And, so, we have our best estimate of the stochastic nature of the fracturing and the blocks that it forms. And, to that model then, which we can sample from many different locations to look at the variability of this fracturing and its effect on rocks, we can apply thermal loads and seismic loading from the ground motion time histories that have been developed, and from that, make an estimate of the degradation or stability of those drifts over time with thermal and seismic load.

Okay. Just to summarize, drift stability, and I’ll go ahead to the next slide where I’ve got--I couldn’t fit all those figures on one slide, so if you could back first, please? This is a summary of the thermal drift stability results for the nonlithophysal rock. It’s not very exciting because the stress levels that are applied to the rock for this material are somewhat below the failure levels that we expect for the rock.

Do you want to go to the next slide then? What we did is this is a range of temperature, drift wall temperature histories that’s from the license application, and it was developed from Tom Buscheck’s thermal--I forget the name of that program off the top of my head--multi scale; right. What he did for various thermal loading configurations and drift conditions and thermal conductivities, and things, he created a sensitivity study that showed what the variability of drift wall temperatures and temperatures back in the rock mass away from the drift would be.

We didn’t generate these in the rock mechanic side of things ourselves. We simply took the results from this external modeling that was done, and we applied those temperatures, and calculated the thermally induced stresses in our model from that. So, this was not generated with any new and different model. It comes straight from the multi-scale model.

Basically, I wanted to show these two plots. If you are not used to looking at this kind of thing, let me just explain real quickly what this is. This is, if you take an emplacement drift, we're looking at the stress state as a function of depth away from the--either the immediate roof of the tunnel in the center plane of the roof, or at the drift sidewall. These are the two sort of critical areas of stress concentration in a vertical and horizontal stress field like we have, to compare what the stability of the drift would be. So, this is the major principal stress, and the minor principal stress. You could think of this as being a driving sort of stress component, and this as being a confining stress that occurs. And, the line that I plot up here is our estimate of the failure criteria or the failure constitutive law for Yucca Mountain nonlithophysal tuff.

What I've showed here, just as a visual thing, is the path that the stresses take is a function of time based on the thermally induced stresses, based on this rise and fall of the temperature field that occurs. And, so, what this is doing is plotting at, for example, this is .17 meters into the immediate roof you're seeing the change in stress as a function of time as it heats and then cools. And, this stress component includes the in situ stress added onto the thermal stress. It's the total stress field that's being applied.

And, any area beneath this failure criteria, which is generated by the uni-axial compressive strength or cohesion of the material and its friction angle, any stress state that lies beneath this line is in an elastic state of stress. It hasn’t failed. The area above this failure criteria is inadmissible because once you hit that point, the rock has failed and it readjusts the stresses back to make it conform to this failure criteria. And, I’m only pointing this out to show you that for all the thermal, the base case thermal calculations that were done, we don’t see, the rock is essentially strong enough in the nonlithophysal rock to indicate that there’s any kind of substantial yield or failure occurring around the periphery of that tunnel.

Can you go to the next slide, please?

I would point out two things that verify these calculations, and show that we have some confidence in them. The first is the drift scale thermal test, which I think most of you have probably seen. It was conducted in the nonlithophysal rock in the ESF, and the drift is 50 meters long. There were central canister heaters that were put in the room as well as wing heaters to try and raise the temperature and stress levels of that drift up to approximate repository conditions fairly quickly. The drift is 5 meters in diameter. The heating started in 1997, lasted for four years, and then there was a four year cool-down phase.

And, the interesting point for us for this test was that after about three years, I believe, of heating, the temperature, the heating level in the drift was raised up to sort of a thermal overdrive condition, with the specific idea of testing what the strength of that rock would be, and to actually induce failure in that rock. And, what happened was is the temperature and the stress conditions were raised to the point where spalling occurred at the crown of the drift.

Okay. I've got some pictures of what that looks like. It's not very exciting. This is a side-on view of it in which the areas in which there were stress induced fractures observed was actually identified, and the area over which those spalling fractures occurred was mapped for the length of the drift. You can see some of them here. They're quite typical of the formation that you'd see of fractures in a deep mine excavation in which the tangential stress, or the hoop stress around the excavation has exceeded the compressive strength of the rock mass. You typically get small, thin plates of material that are formed by fractures in which the fracture runs parallel to the direction of the maximum principal stress, or the sigma one direction.

And, this, in cross-section, the estimated shape of this spalled zone was about a meter in width, and although for safety reasons, the material was never taken down and actually barred out of the back so you could actually see the total extent of it, our estimate was, based on this shape, that probably the fracture zone was extended in some form of a shape like this with about a third of a meter, or so, of maximum depth occurred. It stabilized spontaneously by itself, and this is what you get, and it's very typical.

Next?

I want to compare that to what you actually see and experience from the mining industry, and I showed two separate examples that show that this type of spalling behavior that we saw for those stress levels is very much expected.

This plot here is a compilation of a number of case histories from the mining industry in many different rock types in which the stress state at that depth exceeded the compressive strength of the rock around the exterior of the opening. It doesn't mean, when you exceed that strength, it doesn't mean that the tunnel becomes unstable and collapses. All it simply means is that it yields locally, pushes the stress concentrations farther back in the rock because we've got a wide loading system here, not a dead load. And, so, when something yields, the stresses move to a more confined area where they can be supported.

And, basically, what this plot shows is that even with compressive stresses--or, sorry--stresses around the excavation which exceed the compressive strength of very small intact samples, which is the highest possible strength that you will get, still, the notch formation that occurs here is limited to a radius or less of the excavation. And this covers a very wide group of case histories from many different mining situations worldwide.

What I've plotted on here is the approximate ratio of the stress to strength that we had for the drift scale test, which is around somewhat over .4, which would indicate somewhere around .1 to the radius, ratio of the depth of failure to the radius, which would mean about .25 meters of depth. And, that's approximately what we thought we saw. So, this thermal loading situation in this rock is, I think, verified quite well by just practical experience of many, many years of mining at depths where the stresses are quite high.

One of the very interesting case histories was actually done at the URL, the Underground Research Laboratory up in Pinawa, Manitoba, which is the Canadian high-level waste underground laboratory. It's in granite. And, there, they physically--it's under some very high stress conditions where you have a very high ratio between the horizontal stress component and the vertical stress component, so that the peak stress concentration that exists at this position of the tunnel exceeds the compressive strength of that rock, even though it's quite high.

And, it's quite interesting because they actually did a test there where they drove a tunnel, which I forget how long it was, but it was quite a length of tunnel, you can kind of see way at the very end, there’s some people standing there. They drove it with mechanical means because they didn’t want to damage the rock with blasting, and, so, they painstakingly drilled, line drilling, of the entire, I think the tunnel was three and a half meters circular in diameter, and they broke the webs out between these with mechanical barring means so that they wouldn’t damage the rock.

And, what they observed was a notch that formed, that it formed progressively as they advanced the tunnel, and, in fact, this is one of the example under Martin Ray, it’s one of these examples on that plot, and what they did, they took it a bit further. It was completely unsupported. They didn’t put any rock bolts or anything like that in there to support the ground, and they physically went in and barred that material out to see what the extent of that wedge was. And, you can see that shape is from them actually extending it out.

And, so, this case is a case where spalling has occurred under stress conditions relatively similar to what we would expect in the nonlithophysal rock. And, these two examples I think show that what we’ve predicted and what we’ve actually seen, and the test is what one would expect from practical experience. And, so, we weren’t surprised by that.

Okay. A summary again. Thermal response is strong, cut by short trace length fractures. The maximum thermally-induced stresses for the various cases we have are not sufficient to cause a significant spalling of the periphery of the tunnel. The minor spalling that was observed in the thermal overdrive agrees well with practical experience.

Now, I’d like to switch gears with the lithophysal tuff, which is the majority of the repository. What you can see is a picture here of the sidewall in the lower lithophysal unit. The matrix material that makes up this tuff is the same, and it’s mechanically similar and same to the nonlithophysal unit.

The fracture sets, however, in this rock are not as distinct. We actually have a fracture fabric of small scale fractures that are spaced on perhaps 20 centimeter spacings. They’re discontinuous in nature. In other words, they start and stop and don’t necessarily interconnect with one another. But, it forms a basic fabric for the matrix, in which is embedded lithophysal cavities that can be of various shapes, from spherical shapes to squished down shapes. It varies all over, but the basic thing is is that you get this relatively highly fractured matrix with holes embedded in it, and the porosity in these lithophysal cavities can be anywhere from about 10 to 30 percent, depending on where you are in the flow. The highest porosities, around 30 percent, are right up at the top of the flow directly beneath the contact with the--I mean, that section of flow beneath the contact with the middle nonlithophysal unit, and it’s fairly small in size.

So, the block sizes, when you subject this thing to stress, is exceeding its strength that we expect will be roughly equal to this average fracture spacing, because the fractures will interconnect with the lithophysae and form block sizes anywhere, we think, between 10 centimeters to 30 centimeters, in that range.

KADAK: What is the block strength?

BOARD: I’ll get into that just in the next thing. Okay?

How did we determine the mechanical properties of this material? As you can imagine, it’s a bit more complex to determine what its constitutive behavior is than the nonlithophysal material.

Basically, we have, again, many samples from the matrix material at a large range of environmental conditions. We supplemented that with samples that we drilled with large core samples, is where the samples were, up to about a foot in diameter, and then we went one step further and we conducted some in situ compression experiments with things calls flat-jacks, which are just two sheets of stainless welded on the outside where we put them in slots drilled in the wall of the tunnel, expanded those with oil to compress sample sizes up to about a meter in dimension.

So, what we tried to do is look at this in a variety of increasing scales. The large diameter core samples that we had, we ensured that they had a representative volume of lithophysal cavities in them, so we typically had five cavities across a sample diameter, and the fabric internally was representative of the fabric that we saw in situ.

In addition to this, we conducted about 30 creep tests on the tuff matrix material to examine what creep strains occur as a function of time.

The thermal properties of the material have been determined from laboratory tests, but there have been a number of in situ heating probe tests to determine what the range and variability of thermal conductivity is as a function of lithophysal porosity.

Okay. I’ll get to your question about the properties next. This is the overall general flow. Again, I’m sorry, it’s a bit of a busy sheet, but to show how we approach the problem of determining what the constitutive behavior of this material was.

The first thing we went through is data gathering stage, which I mentioned. We did panel mapping in the tunnels out there, very detailed panel mappings of lithophysal porosity and fracturing across the entire repository horizon. These are the tests I just mentioned.

We developed a range of material properties that we could use for engineering calculations. One thing that is very evident from this is that the strength and stiffness, or modulus, of this material is highly dependent on porosity. And, so, we developed porosity strength and porosity modulus relationships from the testing we did, and we divided that into a range of five different categories based on porosities, so that we could more easily do calculations with these range of properties.

I’m not going to talk about this, but we wanted to make certain that the models that we’re using, that we fully understood why the lithophysal porosity was having the impact on strength and stiffness that it did. And, so, we conducted a series of numerical exercises using a bonded particle model in which we explicitly modeled the shape of voids that we measured in the field, compressed that numerically, and compared our results, first of all, to the large scale laboratory tests we did to validate that the model made sense, and then we used it to extrapolate for many different conditions and shapes of voids, and their spacings and porosity to try and set bounds on what we thought the range of material properties were for the estimates of drift stability.

Once we had done that, we went ahead and used a number of different types of numerical techniques to look at drift stability. We used both continuum and discontinuum numerical methods. The continuum numerical methods are very common. They’re standard finite element or finite difference type methods where we use a constitutive relationship of plasticity models to represent the behavior of the lithophysal rock mass.

The discontinuum approach, which I will discuss more, we actually used to try and represent the development of a fracture zone around the tunnel, so we could see how much, or make an estimate of how much rock would actually fall out of the tunnel in an unsupported tunnel when you shook it or subjected it to thermal stresses.

We did a significant amount of confirmation of that against field observations in the tunnels themselves. The one thing that we have out there is there’s about five miles of tunnel total, I guess, out in the ESF and ECRB, and the ECRB in particular cuts all the way through the repository horizon. And, it’s at stress levels that are reasonably close to the strength of the lower quality high porosity material. So, we essentially have a laboratory out there that we can use to compare field observations versus our numerical models.

Okay. This shows the result of these large scale laboratory testing that we did. We drilled about 60, I think it was 67 one-foot diameter by ten feet long boreholes out there in various locations in the sidewall, the shoulders of the tunnel, and actually in the roof in some cases, in both the nonlithophysal and lithophysal material to gather samples, but also to be able to look back into the sidewalls of the tunnel and see what kind of damage had been done by the in situ stress conditions, if any. So, we had a large group of observation holes that we could actually go in and map and compare.

Sorry that this scale is a bit wacky here, so you don’t quite see the--the effect isn’t quite as evident here. But, this shows, of about 30 of these large scale tests we did at different conditions, it shows the range of properties that we had for the uni-axial compressive strength and for Young’s Modulus of the material. And, basically, from the lower end of the porosity scale in the lithophysal material, which is around somewhere between 10 and 15 percent, the rock strengths are as high as around 30 megapascals. You can see the range and strength is on the order of 10 Mpa, or something like that. And, for the highest porosity materials, which have porosities closer to between 25 and 30 percent, the strengths are down on the order of about 10 megapascals.

This is important because--maybe go to the next slide. We subdivided this range. This is a plot that shows the compressive strength as a function of Young’s Modulus, because the two go together. They’re not separate quantities. And, we subdivided this complete range that we had into five different categories, one being the lowest strength category, poorest quality material, which is about 6 percent of the repository area, and it represents a lithophysal porosity greater than about 25 percent.

The majority of the rock mass is in these 3, 4, and 5 categories that we developed, which is approximately 80 percent of all the material in the lithophysal rock, which is characterized by porosities of around 10 to 20 percent. The average porosity in the lower lithophysal unit is about 18 percent on whole, if you take all of the observations made.

We established a range of properties. We have a mean and median case. But, we established a range of properties, both from the laboratory testing results and field testing results that we had, and from our numerical simulations, to try and look at the effect of many different lithophysal shapes. So, all of our calculations of performance, we actually used a very wide band of strength for any particular Young’s Modulus measurement that we had.

Okay. How did we assess the stability knowing those properties? We used two approaches. One, as I mentioned, was a continuum based approach, as is typical sort of a stress analysis methodology, in which you use a constitutive model. In our case, we used a Mohr-Coulomb failure criterion for this, which assumed that you have a decrease, or a strain softening type mode where the strength reduces after the peak strength of the material is reached. So, it allows progressive failure to be modeled.

What it basically shows, you can predict an area of fractured rock that you would expect would be in a yielded state, and then you will predict some form of redistribution of the stress state based on that yielding from that model.

We used a second approach primarily because one of the issues that we had was how much of this rock do we actually think is going to fail and fall off? The first thing that you know if you work around these underground highly-stressed mines, is that just because a rock yields around the exterior does not mean rock falls out. You get fractures that follow tortuous paths that the material hangs together there, and it actually bulks and confines the material behind it. So, just because you have a yielded zone around here does not mean that material is necessarily going to fall out.

What we did is we used another technique, a discontinuum method, where we physically put in a random structure in the rock mass, in which we modeled the rock as a series of elastic particles that are bonded to one another with friction and cohesion values that replicate the constitutive behavior that we measured from the laboratory.

What it did is this allows us, though, to allow this rock to freely break those bonds wherever the stresses and energy dictate, and it can fail and shear our tension, and you can determine just how much of that material has actually become unloaded and can move.

Okay. The most important parameters for this are the two parameters I just mentioned, the modulus and compressive strength. The third thing that's important to determine the stability excavation is how this material behaves after the peak strength is reached, what the brittleness essentially, or ductility of the material is after it fails. So, we did a significant amount of work of calibrating this discontinuum model to that behavior.

Okay. This shows one of our large scale tests that were run, and it shows the typical sort of laboratory response of the lithophysal material. It shows a rather ductile post-peak behavior, even in the uni-axial compression. And, what gives rise to that ductility is is it's moving on all this fabric or fractures in there through shearing and sliding, so you get energy dissipation on sliding. Those fractures extend into lithophysal voids, and you get the whole thing as sort of moving as a sliding material.

This shows a typical sort of calibration. What we did is physically calibrated the properties of the interfaces between our material to the actual stress strain behavior of the rock samples. And, in our case, what we did is we made an assumption, the blue line shows our calibration to a peak strength of 25 megapascals, and we forced the model to show a more brittle post peak response. And, the reason we did that is because the more brittle that post peak response is, the more violent or I should say the less stability the tunnel will have once it starts failing, because it gives up that load very rapidly, and you can cause a much deeper, potentially deeper yielded zone around there. So, what we did is we purposely set this to what we thought was a conservative approach of the post peak strength reduction.

Okay. Some of the comparisons that we made with this model just to verify it is, as I mentioned to you, the rock is at a depth of 300 meters, and, so, the stress concentrations in the sidewall of the excavation there is about 17 ½ megapascals. And, that’s just simply the stresses around the circular hole.

As you may have noticed back on our categories that we had, two of those rock categories that we predicted actually had strengths that were less than, or approximately equal to what we think the stress state, the uni-axial stress state in the sidewall is. So, therefore, if those values make sense, what we should see is we should see evidence of that in at least the poorest quality of that material underground.

So, we drilled, as I mentioned, many of these large diameter boreholes through that material, and in some of it, we drilled it through all different porosity materials. This happens to be a borehole in the ESF that is through what we termed Category 1, which has a porosity of somewhere close to I think 25 percent, in that range. It's the lowest strength of all the units. And, we actually do see what we expected to see, and that is that the rock is actually yielding and forming these wall parallel fractures at that depth.

We only observed this in two boreholes out of all the 60-some we measured, because there isn't very much quantity of this lower strength material.

This just shows our numerical model where we've taken the constitutive behavior we calibrated it to in the laboratory, and applied it on a drift scale, and you get the same thing for this Category 1 rock. We would predict that in the immediate sidewalls, you would produce about a half a meter of depth of yielding, and that's about what we see.

We don't see any of this yielding in any of the other boreholes, this 60-some drilled in there, so obviously, that’s one thing that’s a hallmark of if this rock has overcome its compressive strength, you will see wall parallel fractures forming, and we don’t see that in any other location.

We also did a comparison of this technique we are using here, which is not new, by the way, I just would point out that this technique of subdividing a body up into many different small blocks with strength along the bounding areas has been used in looking at strength of concrete beams, and under dynamic loading, and things like that, fracturing since the early 1980s. And, what we found out is that with our calibrated properties for the nonlithophysal material, we imported the temperatures directly from the drift scale test, and we predict a maximum stress based on that of about 80 or 90 megapascals in the roof. Which produces a zone of fractured material that is to a depth of about a quarter of a meter, which is just what I showed you earlier, is what we would have predicted based on just practical information and what we actually saw in the test.

So, we’ve done these two things using essentially the drift scale test and the drifts underground as a laboratory to verify these ranges of properties in the model that we have.

Okay. What does this mean when we now apply the thermally induced stresses that are expected in the repository? We did a large number of sensitivity runs over the entire range of drift wall temperatures in the lithophysal rock as well.

Basically, what you see is this. You see that you'll get minor regions of yielding around the tunnels in the lowest quality material. That yielding occurs in the sidewalls. In the highest quality material, which has a higher Young's Modulus, the peak thermal stresses occur in the roof, and you see a small area of yielding in the roof.

The depth of this yielding is typically limited to somewhere around a half a meter, or so, and the drift comes to equilibrium in an unsupported condition based on that.

Why does that happen? The reason it happens is because in a live loading situation like this, when the rock yields, the stresses get shunted back into areas that can support that, and the areas around the immediate area of the drift where it's yielding become unloaded, or de-stressed. And, so, the stress is transferred back into where it's more highly confined, and the strength of the rock is higher, and it will come to equilibrium even without the presence of ground support.

This is something that, of course, the mining industry has known about for ages. If this process didn't happen, you wouldn't be able to mine at depths greater than probably around 3,000 feet, in that range, because the stresses get high enough to start overcoming the compressive strength of the rock mass. So, it’s not unusual that the calculations would show the same sort of behavior that we see in underground mines.

Okay. So, just to summarize. The thermal stability of these drifts, we basically see that the drift, even during the thermal pulse or thermal phase, we don’t see excessive rock instability. There is some small amount of rockfall, but we don’t get any collapsing tunnels, even in the lithophysal material.

Okay. The next thing we did was we said okay, well, is there any significant time dependency to this rock, how the rock behaves under uniform loading that occurs over a long period of time. And, what we did is a series of creep tests on the rock. We did about 30 of them at saturated conditions in 150 degrees C. So, to keep the water in a liquid condition, we provided a confinement to the core that was equivalent to the pressure that was generated by the water trying to boil away, essentially, so that we kept that in a liquid state, even at a very high temperature, which we thought was quite conservative for the drift conditions that we had.

And, what we developed with these creep tests, they are very similar to the ones that would be done for something like whip and salt, but it’s a hard rock, so it takes much longer to develop some kind of a failure mechanism. So, a sample is essentially loaded to some percentage of its compressive strength, and the load held constant until you reach a stage of tertiary creep, where you get progressive micro-fractured rock until it fails the sample, and we determine what that time is.

And, we have constructed a plot here that has what we term the driving stress ratio, which is the stress applied to the sample, normalized by its uni-axial compressive strength, and the time it took it to fail. And, this is a semi-log plot.

Okay. We encapsulated that behavior into our numerical model by adjusting the strength characteristics of the model as a function of time in the model to see what information it would give us about the long-term stability of these excavations over time.

And, what I have shown is three different states here for a low strength category of the rock and the highest strength category of the rock. And, unfortunately, it’s quite hard to see. I hope it’s a bit better on your handout. But, you can see on here, there are blue lines, and the locations where these bonded blocks have overcome their shear or tensile strength, and they have propagated fractures around the exterior of the excavation.

And, I wanted to point, well, the first obvious thing is is that yes, it does increase the amount of potential rock fall that we would see from the thermal stress condition without any seismic loading. We would expect that you will get some potential fracturing of the sidewalls of the tunnel, and in the roof of the highest category rock here where the thermal stresses are highest. But, we don’t get any large scale collapse of the excavation occurring due to time dependency and thermal stresses alone.

We think this is a conservative prediction, because I put on here ten years with no thermal load case here. Those tunnels out there at Yucca Mountain have been there for about ten years right now. And, as I mentioned, it’s actually quite a good laboratory to examine over time, what’s actually happening with those tunnels. And, as I mentioned, the peak stresses at the sidewall, just from the overburden, is around 17 ½ megapascals, in that range. And, so, the strength of this Category 1 rock that we have is about 10 megapascals, and this Category 2 is somewhere around 15 megapascals, is what we’re estimating as a median case.

So, based on our estimated time to failure creep curves that I just showed you, creep data that I just showed you, we would expect to see, from straight implementation of that in our calculations, some extensive cracking occurring along the sidewalls in this Category 2 rock, which composes about 10 to 15 percent of the material. We don’t see any of that behavior occurring at this stage out there at Yucca Mountain. So, based on this, and then for the Category 1 rock, we would expect to see even more extensive, so we think that this time dependency that we've put in there, this time strength loss is actually quite conservative.

Even with that conservative nature, we don't see it out through the thermal pulse range of these calculations. We expect to see a very significant impact of time dependency. And, it comes back, the reason why we don't, again, it's the same situation. If this rock begins to fail and its strength is dropped, because we've got this live loading situation, the stresses get shunted back further into the rock where you have developed enough confinement to maintain stability.

Now, if it turned out that the stress state was such that over a very large volume, you exceeded the strength of the rock, of course you would see this, this model would indicate collapse. So, it's not that the model is not indicating it because there's something inherent in it that's preventing it. It's because we don't have--these stress concentrations are confined to the very skin of the opening, and that's what really does the self-stabilization.

Okay. Finally, I've got just a couple of slides on the seismic response. I don't know what we're doing timewise. But, I'm only going to talk about the lithophysal rock. If you want to talk about the nonlithophysal rock, we can. But, this is by far the most important.

As John was mentioning with this extreme ground motion issue, one of the reasons--one of the reasons, other than the surface facilities, for being concerned that the ground motions that we develop were highly conservative, was the calculations that have been done. What that actually does was shaking the tunnels and the amount of rockfall that you get there, and how it shakes drip shields, and waste packages around. The calculations I'm going to show you, we used directly the ground motion estimates that were made from the original seismic hazard assessment. We have not used any bounding ground motions or any extreme cutoff limits on them.

We did calculations for ground motions at three different peak ground velocity levels. 24 meters per second corresponds to the 10^{-4} hazard level. 1.05 is 10^{-5}, and this is 10^{-6}, to give you a feel for what those compare to.

For each one of these hazard levels, 15 different ground motions were produced to represent the variability and potential ground motions. So, we conducted simulations for all the entire range of expected lithophysal rock properties for 15 ground motions at each one of these peak ground velocity levels. And, we calculated what rockfall would occur from that.

I'll show you a couple of pictures here in the next slide. But, basically, the analyses show that at this point, 4 meters per second level, only minor groundfall occurs at the 10^{-4} level. We see at the 1 meter per second level, a transition when you start getting significant amounts of groundfall occurring. And, at the 2.44 meter per second level in the lithophysal rock, it results in complete collapse of the excavations.

Okay, next? I think I've shown these to you before, but I'll show them again. This is the Category 1 rock, 3 and 5, and it sort of goes across the spectrum of the rock quality for one of the ground motions. I think this was at one of the worst ground motions as far as the PGV level, and also the energy level stored in that ground motion. Wait, no, sorry, it may or may not be one of the worst. I take that back. I'm not exactly sure. But, at 1 meter a second is when you start seeing essentially shear failure of the rock mass surrounding the excavation, and the beginning of significant amounts of groundfall in the weaker rock materials. In the strongest rock material, 1 meter a second is just at some kind of a transitional phase.

Okay, next slide?

As you go further on, you get larger and larger amounts of collapse. What we did for the seismic consequence abstraction that's in the License Application, is we quantified how much rockfall occurs as a function of the seismic energy in terms of the number of square meters per meter of drift length of rockfall, which you can convert to a tonnage, which was what was done in the seismic consequence abstraction.

The best correlation that we had is essentially the amount of energy density contained in that ground motion, not necessarily the peak ground velocity, but the amount of energy is the best indicator.

This shows that for these three different rock categories, for 15 runs per rock category, a value of about 20 on this scale represents a substantial collapse of the excavation. So, at about 1 meter a second, we start seeing spallation and rockfall occurring, and by the time we hit, in these analyses, around 2 ½ meters a second, or something in that range, it essentially results in complete collapse of the excavations. So, for many of the cases in the seismic consequence abstraction, we predict that the excavations completely collapse and cover the drip shield with rubblized material that has a size range of perhaps 10 to 30 centimeters in size.

You might ask does this make sense, these calculations, from a practical standpoint. And, it actually does. There’s a lot of experience in deep mining in which the peak ground velocities from induced seismic events by slip on faults is actually greater than what John had mentioned from earthquake research.

In many of these deep mines, they’re mining near faults, and the mining itself causes slip to occur on a fault over a fairly large section of area. And, some of these excavations are within 100 meters or less of that slipping area. And, you can have Richter magnitudes of, the biggest I’m aware of is about 5 Richter magnitudes in South Africa at one mine. And, there have been empirical correlations developed from this long history of seismicity in mines that say that damage first is noticed at about a meter a second, and that’s pretty true, when you get a meter a second PGV, you start seeing spalling of rock from most excavations. By the time you get a 3 meter a second peak ground velocity with these mine excavations, it’s pretty devastating, and it collapses a good share of the excavation.

In fact, in South Africa, one of the Holy Grails for many years was to try and develop fast unloading hydraulic supports that would unload at that kind of rate of 3 meters a second so that they could preserve the excavation with mean working in there. So, these calculations are showing exactly what I would have expected from sort of a practical mining based thing.

Okay. So, the conclusions that--you can read them up here, but basically, these models that we have developed, we used a wide range of modeling tools and a wide range of property considerations to do our calculations.

We don’t see significant thermally induced rockfall that’s occurring, even for unsupported tunnels. And, the reason being, again, that the stress concentrations are confined to the immediate sidewall of the excavations, and when yielding occurs, essentially, the system self-equilibrates with relatively small rockfall volume. The level of essentially over-stressed conditions is not great around the tunnel.

With the rockfall from lithophysal rock and seismic ground motions, as I mentioned, by the time we hit a 10^{-6} we typically have complete collapse, and that’s what’s assumed in the seismic consequence abstraction.

So, I think I will leave it at that, and answer questions.

GARRICK: Okay. Yes, David?

DUQUETTE: A couple of questions. One of them, the pillar wall thickness is right at the moment fairly thick. If that were reduced, at what point would one tunnel see another tunnel in terms of the stress on the tunnels?

BOARD: I can tell you, I don’t know thermally, but let’s say just the in situ stresses, the stress falls off rapidly away from the tunnel, and the influence of one tunnel outside of it is typically estimated to be about a diameter, or so.

DUQUETTE: Okay.

BOARD: And, so, the extraction, in mining terms, how close those tunnels together is called extraction ratio, and here, the extraction ratio of the tunnels are so widely spaced that it’s, I believe, what, around 4, 5 percent, or something like that. Typical mining by room and pillar mining has extraction ratios of around 50 to 60 percent, or even higher. And, that’s what you design pillar sizes based on. So, essentially, all these tunnels are non-interacting from a straight in situ stress standpoint. Thermal induced stresses, they do interact. I can’t give you as clear an answer that way.

DUQUETTE: Okay. Duquette, Board, again.

Just to put things in perspective, you mentioned a South African experience, and you tossed out the number of 3 meters per second. What’s the maximum velocity that’s ever been observed?

BOARD: You know, I don’t know, it’s around somewhere in the range of 3 ½ or 4, I believe.

DUQUETTE: So, you’re pushing that with a 2.4. Would that be a pretty severe earthquake very close to the site before you’d have to worry very much about total tunnel collapse?

BOARD: Yeah, and this is one of the things that spawned this extreme ground motion study, in that there’s no observation that this kind of event has ever occurred at Yucca Mountain. The one interesting thing about this lithophysal rock is it’s imprinted with a series of cavities in there that have been in that rock for 13 million years, and I believe the geologists would agree that at approximately the same depth of burial that it’s at right now. And, so, these cavities have been subjected to these in situ stresses for a very long period of time. And, if you had large seismic events that were inducing large compressive stresses on that rock mass of this kind of level, you would expect to see damage in those lithophysal cavities. And, we see nothing of the sort.

I mean, if you go through there, they’re essentially in an unfractured state. As far as I understand anyway, I’ve certainly spent a lot of time down there looking at them, but you do not even see micro-fracturing, small scale micro-fracturing developed, even in these thin webs between lithophysal voids, which to me is a good indication that the time dependency is quite low in this material, but also that it’s never been subjected to stresses, seismically induced stresses of the level that we have been examining here.

That’s one of the reasons that we just said okay, we’re sort of getting controlled by the tail of this distribution on the hazard, seismic hazard curve, and that we should think of it more about what kind of seismic load this rock, or any rock, can actually sustain without some observable evidence. And, that’s what John is, one of the things he’s looking at.

DUQUETTE: The final comment. Very nice piece of work.

BOARD: Thank you.

GARRICK: Ron?

LATANISION: Latanision, Board.

Can you make any judgment of the size and shape, or mass of rockfall that you might anticipate during the thermal pulse?

BOARD: We did. It’s quite small. I mean, Bronco is--by the way, I should have mentioned Branko Dameanac from Itasca is the person that did the majority of this work. I’m just the mouthpiece here. We actually did calculate the amount, and it’s relatively small. I don’t know, Bronco, if you have any other thoughts on that.

DAMEANAC: Branko Dameanac, lead lab, Itasca.

Well, I think Mark showed a couple of slides which indicates the rockfall, which is the most representative accounting for thermal load and time dependence strength degradation. So, if you go a couple of slides back?

BOARD: I think that didn’t have--

DAMEANAC: I’m sorry. No, that’s fine, yeah.

BOARD: Yes, it didn’t have specific numbers on it, but you can get the picture about the approximate volume of it.

LATANISION: I'm wondering more than the total volume, I'm wondering about the impact on the drip shield, for example. What are the loads that you might anticipate from the rockfall on the drip shield during a thermal pulse?

DAMEANAC: Okay, in the case of lithophysal rock mass, impact load is not significant. What is more relevant is basically static load of the accumulated rubble. And, in these cases, as shown here, that load is not significant. When impact becomes an important factor in case of nonlithophysal rock where, Mark didn't discuss in detail here, where we predict some large blocks, up to tens of tons for mass, becoming unstable, dislodging and impacting the drip shield. And, we did carry out those calculations and what the effect on stability of those--with the drip shield.

LATANISION: So, those calculations have been input into the modeling of the size and shape of the drip shield, and so on?

DAMEANAC: Yes.

BOARD: And, I think the other important part is, from the lithophysal rock, is the load from the accumulated rubble, the static long-term load that develops, because essentially when these seismic events happen, we've predicted that the drifts will collapse, and, so, what we've done is we have taken that, we have estimated, to the best of our ability, what that static load would be on the drip shield, and we have incorporated that into the performance assessment calculations for the drip shield, as well as creep in titanium, and the temperature effects. So, as of that has been accounted for, and we think in a fairly conservative fashion.

GARRICK: A quick one?

KADAK: A quick one.

GARRICK: Okay, Andy?

KADAK: Kadak, Board.

One thing that we have been looking at from time to time is the effectiveness of these, I think, stainless steel, I don’t know what you call it.

BOARD: Bernold sheets?

KADAK: What are they called?

BOARD: Bernold sheets.

KADAK: Bernold sheets.

BOARD: It’s named after the Swiss manufacturer.

KADAK: Okay. Now, those were not factored into these calculations, were they?

BOARD: No. We assumed that all ground support was gone, essentially, although, I mean, the reality of it is is it’s stainless, friction inflated rock bolts, as well as these sheet linings, and the linings were done to prevent--I agree, it’s extremely conservative. It was done to prevent small rocks from essentially resting on the rail line for the remote controlled emplacement. But, they’re made of stainless, and during the thermal phase, I mean, I believe the rate of corrosion of that material is relatively low, and that stuff will still be in place during, I would guess, during a fairly significant portion of this thermal phase. And, that’s very heavy ground support. I can tell you those bolts are 3 meters in length, and they cover the entire section. And, so, it’s still going to be operable, but we didn’t encounter that.

KADAK: We also started looking at traditional grouting, cement type grouting, but cement that’s compatible with that particular environment. Would that be a mitigating measure to avoid some of this stuff, or would that not help if it was a cementitious type of group that would potentially hold back as traditional mining--

BOARD: Well, I think our calculations indicate that we don’t need to count on ground support for any kind of stability of excavation. I would say that first of all, because we feel that they will be long-term stable. However, yes, the typical way that one would support that ground would be the groutable rock bolt with some sort of either resin or cement. But, we specifically didn’t do that because the chemistry issues associated with, which is my field, but that was sort of dictated to us that there was really not a desire to have cementitious materials in there. That’s why we went to the inflatable rock bolts.

KADAK: Just a quick comment. As I understood, there was some work done at Oak Ridge to look at specific cements that could work in this area that would maybe avoid these very expensive Bernold shields.

BOARD: That’s a good possibility I suppose. I wasn’t here when that work was done, so I don’t really know if there’s anyone else that can comment on that.

GARRICK: Okay, I think one final question, if it’s a quick one.

MOSLEH: On your Slide 5, which outlines the approach, you call this seismic loading sensitivity. Does that mean that the calculations did not include the seismic load and you just did that as kind of the sensitivity studies, or--

BOARD: No, by sensitivity, I mean the seismologist, ground motion experts, actually produced for us 15 sets of ground motions that they felt were for each one of those levels, the 10^{-6}, 10^{-5}, that they thought represented the complete range of what potential ground motion you would get for that range of earthquakes that make up that hazard.

Okay, we physically, in these models, directly--they’re dynamic models. We physically subjected the model to three components of ground motion, two horizontal and one vertical, that we got from the seismologist. So, they were explicit calculations. And, by sensitivity, I mean the range of sensitivity of the ground motions represented by these 15 different potential motions.

So, in some of these three dimensional calculations we did, we did a big parameter study where we used 50 different potential fracture pattern to represent what might be possible in the repository. And, then, on top of that, we applied 15 different sets of ground motions. So, we did a huge number of fully three dimensional dynamic simulations, and physically counted the blocks that got created and fell off their masses, and their velocities, what the impact loading was to the drip shield, and all that, and we then used that in the seismic consequence abstraction.

GARRICK: Okay, thank you very much, Mark.

Our final speaker for this morning is Ernie Hardin. Ernie is at Sandia National Laboratories, and he has had the lead for near-field environmental studies, including effects of heating on the host rock, and is going to give us a couple of presentations on heating issues. And, I hope that maybe we get two for the time of one, and, Ernie, and would you also not let us ask questions until you finish both?

HARDIN: Okay, when do I have to finish to get my free lunch?

GARRICK: Well, we’ll talk about that.

HARDIN: Thank you. If you’d proceed?

The first talk that I’m going to give you has to do with the drift collapse rubble and its effect on thermal properties. I’m going to go through how we address that in TSPA. There’s a little model involved. And, then, I’m going to talk about experimental support that we developed since the License Application was completed. The experiments were done by Bob Jones, who is my co-author, and his colleagues at Sandia, and at Resbak, and I will show you how they tend to support what we have done thus far in the TSPA. Bob is here to answer any questions you have on the experiments.

Next, please?

Okay, so this is sort of a setup for what we did in TSPA. The goal here is to be able to represent the temperature changes in the EBS that occur if you had a substantial drift collapse. And, I should point out that this would only be in the lithophysal, which is what you heard from Mark. In the nonlith, we would not expect drift collapse to occur at all, even over hundreds of thousands of years.

In the lithophysal tuff, what we do is we implement a strategy where if this is the intact drift, or uncollapsed curve, we calculate some conditional curves, which are the thermal response conditioned on drift collapse at closure, and then in TSPA, then, we would then jump to one or another of these curves at the time that we identify drift collapse in the simulation.

So, there are two curves here because we have a low and a high effective thermal conductivity relationship for drift collapsed rubble in the lithophysal tuff.

We use this information to control seepage, waste package corrosion rates, in-package chemistry boundary conditions, waste form degradation rates, and the onset of conditions for that degradation to occur, and then the onset of conditions for which aqueous transport can occur out of the EBS, transport of radionuclides.

So, I talked about the offset calculation method, and it’s important to recognize that in the TSPA, that drift collapse is seismically induced. If you look at the aseismic, or the thermally driven, or time dependent aspects of drift collapse behavior, they’re overtaken by seismic induced drift collapse.

Next, please?

Okay, to get after the effective conductivity to put in the model for calculating EBS temperatures, this little cartoon basically shows the original drift opening outline, and then one with twice the--well, it’s actually a cartoon, but the model actually uses a circular outline with twice the diameter, and the rubble fields in the gaps.

We did a literature search, and we adopted a literature model here of packed beds, and I will talk a little bit more about that. One point here to recognize is that convection is not included. The little arrows signify how you might have gaseous convective circulation in the system that would remove some additional heat from the EBS.

Down here, I listed some of the key aspects of the Kunii and Smith, my apologies to Mr. Kunii for the pronunciation of his name. It’s based on an unconsolidated loose packing of spheres. It’s mechanistic, accounts for conduction and thermal radiation. So, it’s a geometrical solution that accounts for the configuration of the particles in the voids. And, conduction and radiation in both the voids and the particle network.

Okay, next, please?

So, this is sort of a conceptual slide. It’s the setup for how we approached characterizing collapse rubble for the TSPA model. We have lithophysal material with fine fractures in it, with cavities, which will collapse when the material localizes, or most of them will, especially the larger ones. And, the block size, as Mark pointed out, 10 to 30 centimeters, we adopted 20 centimeters, and we used that to figure out what we thought the void sizes would be between the particles.

The temperature dependence in a collapse rubble like this depends on two different kinds of processes. If the voids are large, you get radiative coupling between the particles, and you get more heat moving through for a given gradient. If the voids are small, then you’re not going to see that, and heat transfer is going to be dominated by the conductivity of the air, or the gas phase, in between particles.

I guess what I’m saying here is that for the high interparticle porosities we’re talking about, on the order of 30 or 40 percent, is that the conduction path through the particle network moves a limited amount of heat.

So, one other point here is that in the TSPA, and this is in the multi-scale model, we treat drift collapse as an event that occurs all at once. We don’t try to model it as partial collapse, and the rationale for doing that is that rubble does accumulate slowly due to seismic events over time, that the eventual collapse due to seismic ground motion occurs late after the thermal period, generally speaking, and that the waste package temperatures really don’t see the effect of rubble until you have covered the drip shield. So, you have to have a certain amount, on the order of 8 or 10 cubic meters of rubble per meter before you begin to see the thermal effects.

Next, please?

So, this is the result of applying that literature model using some parameter values based on the lithophysal tuff. This is temperature, effective thermal conductivity, so this accounts for, again, conduction and thermal radiation in both the particle network and the voids.

And, what we have sampled on is a range here for TSPA. So, there’s a considerable amount of uncertainty in the heat transition behavior of this rubble material. The particle sizes we used for this, the 10 centimeters was selected basically recognizing that these are not spherical particles, so the voids are not really as large as they would be between 20 centimeter spheres. And, then, the 1 centimeter particle size represents the possibility that we could have some filtration of smaller particles into the larger voids. It would tend to fill up the material and give you a smaller average void size, and eventually a smaller porosity.

So, we used both ramifications. These are groups of curves, and the aspect that separates these two groups is the particle size.

So, next slide?

Now, we switch over to the experimental support for this. There is, I should say, you know, in the original papers by Kunii and Iyagi and others, they do draw upon, you know, abundant experimental support that had been done prior to their publication.

For this test, we went into this under the auspices of DOE’s--OCRWM’s Science and Technology Programs several years ago. They were asking what would be the effective heat transfer characteristic of the backfill. The backfill type material that we thought would be most favorable is one made of crushed tuff with fairly large particles, well sorted, so the portions as far and you could make them. So, this apparatus was to try and find the temperature dependent effective thermal conductivity response for that material.

So, this is similar to, you know, other tests that others have done, where you’ve used either the thermal probe method, or you’ve heated a cylindrical volume of sample from the outside by step increase in the outside, external temperature, and looked at the internal temperature, transient. The probe method is probably familiar to all. So, that’s what this apparatus is set up to do. It does both methods.

Next slide, please?

Here’s a little cartoon, a picture of the apparatus. It’s like a spaghetti bowl, 7200 watts. The dimensions of this test are 5 feet long and about 12 inches in diameter, and the particle size is about 1 inch, and it has a 1 inch tube running down the center of it. It’s a copper sleeve. We put rod heaters on the inside of the tube, and we put temperature sensors on the outside of the tube. And, we’ll see a little bit of that later.

Next slide, please?

One of the differences, or the important aspects of this test, differences from some previous work, is that the tuff was carefully sorted and conditioned prior to the experiment. So, this is the dry tuff material. Really, the dry condition is the important one from the aspect of repository heat transfer, because we’re using this to calculate temperatures throughout the temperature evolution of the collapse drift system. But, what’s really important is the peak temperature, and that temperature is well above 100 C, so the tuff is dry.

Next slide, please?

So, there are two kinds of tests, as I mentioned, the step test where we increase the temperature of the external surface of that test vessel, pulse tests where we put a constant power signal into the center probe and look at the transient heat response in both cases.

Next slide, please?

These are some of the key temperature and power results from the tests. I don’t know if you can see the legend at the bottom. Let’s start with the power to the outside is the yellow curve. So, it was increased until the temperature stabilized on the outside. And, of course, we’re looking at the center temperature response to that, so it’s coming up this way.

When we get up to temperature, up to the set point of the external heaters, now it’s time to run a transient test by putting power into the center heater, and so forth. And, then, you decrease the overall temperature of the test. The next step, repeat. Next step, and repeat. So, there are three different temperature levels that are sampled here for that tuff sample.

Okay, next slide, please?

So, these are some example temperature time histories from the pulse tests, basically just put here to show you that the difference in terms of--as a vertical orientation in temperatures measured during the tests is small compared to the temperature rise. So, this means that we’re having predominantly conductive or pseudo-conductive radiative heat transfer behavior and not convective.

Next slide, please?

These are a similar set of temperature time histories for the step tests, where the external temperature is increased thusly, and the internal temperature tracks it.

Next slide, please?

Those data were all evaluated using the classical analytical solutions of calculus. They were implemented in a least-squares fitting inversion scheme, and these are the effective thermal conductivity values that result. So, we do see a temperature dependence here. The pulse tests are the triangles, and they have a little bit higher apparent effective thermal conductivity. So, that may be because of the outward radiative dispersion of heat energy at the center around that 1 inch center probe. Maybe it had something to do with the boundary conditions of the test.

Next slide? Okay, this is it.

Okay, this is the slide that summarizes the talk. Again, we took a box, we cut a box out of the TSPA curves I showed before. So, here is the effective thermal K response from over a very wide range of temperature, the TSPA range that was sampled between here and here, okay? The tests that Bob Jones and his group did yielded these data points here. This collection is from an earlier test series that was done using a higher temperature, more power, larger thermal gradients, and there was more moisture in the tuff material, so they had a bit more scattered and a bit higher values of the apparent effective K.

And, then, we've thrown in some additional curved, calculated using the same literature model that was used for these, just to show that if you mess with the particle size, that is, the void size in the material, you can get the radiative coupling to sort of mimic what we observed in the test.

So, the bottom line is that the test did not produce very much radiative coupling. It did produce a temperature dependent effective thermal K response, and most of that response is due to the temperature dependent thermal conductivity of air. But, in this sense, they lie nicely within the envelope of the range that was sampled for TSPA. If we wanted to do more tests at a room scale, we could get up into this region. These tests were all done at about the same scale. In order to generate data of quality comparable to these up here, you would have to have much larger particles.

Okay, next slide?

There’s a summary. Air thermal K appears to dominate the dependence for the tests. You could have radiative transfer if the particles are bigger. So, in the repository, when rubble forms and the particles are of basketball size, we expect the effective thermal K to be much larger than we measured in this experiment.

The particle size uncertainty then really is what leads to uncertainty in predicted temperatures for the EBS components. It depends on a number of different factors, and those are all rolled into the uncertainty range that we then sampled.

So, that’s basically it. We say it doesn’t include convective heat transfer. I’m not sure how significant that would be. The numbers are pretty small for this system, on the order of 100 or smaller. So, that’s it for this talk.

KADAK: It does assume the drip shield intact, these analyses, or do they--

HARDIN: Yeah, right. Yes, the answer is yes. The drip shield is intact.

KADAK: Would it matter in terms if the drip shield was not intact in terms of what you might get for the calculation of peak temperature, for example?

HARDIN: It could matter. I mean, I’m speculating here, but at peak normal conditions, there’s typically a 20 centigrade degree difference between waste package and drip shield surface temperatures. If you collapse--the drip shield acts as a radiation shield. If you collapse that, I’m sure that effect would be smaller. So, it would. I have to think about the answer to that. It would potentially--some aspects, some parts of that problem would lead you to believe it would drop the waste package temperature. I mean, the temperatures in the EBS are determined by far field and integrated back to the center.

Any other questions?

LATANISION: Yes, let’s take a couple more questions.

(No response.)

LATANISION: How about the staff? Any questions from the staff?

(No response.)

LATANISION: Ernie, why don’t we move on to your second presentation, then we’ll regroup.

HARDIN: Okay, this one, I think the interest here was if you had substantial drift collapse, what would that do to the waste package temperature and to the cladding temperature.

I should point out at the onset of this, that what we’re talking about here is on the tail of the curve somewhere, it is behavior that is quite unlikely, and I’m going to present to you the general approach that we used for modeling temperatures in the EBS for collapse conditions, talk about why we think the limit on the waste package temperature and on the cladding temperature is going to be met, and then give you more basis information in the form of a probabilistic analysis where we looked at the likelihood of drift collapse, partial or complete, during the thermal period early when the 300 C peak waste package temperature might be exceeded. Okay, so that will put the whole discussion in perspective for you.

Next slide?

So, these are multiscale model results here. This is starting with uncollapsed. This is all waste packages, all conditions, these different cases here, and there’s seven of them, correspond to the range of percolation fluxes and the range of host rock thermal conductivities that we’re considering in TSPA. So, these are we call them epistemic uncertainties that generate, each one generates a different set. You can see what the behavior is here. The peak waste package temperature is around 200 degrees.

Now, these are collapsed results. Of course, they group out according to whether you're looking at the load rubble K thermal, or high rubble K thermal case. And, we separated them here in terms of the DHLW or co-disposal package versus the commercial spent fuel.

KADAK: I'm sorry. Drip shield is intact in this case?

HARDIN: Yes, it is.

KADAK: Okay.

HARDIN: You know, you're interested in that. The possibility that rubble would form on the drip shield dead loaded, and then increase the dynamic loads to the size of ground motion, has been considered in the seismic consequences abstraction.

Okay. So, this is a familiar curve. This shows the temperature history for the hottest waste package anywhere in the repository for our TSPA base case, is the blue curve, and for the two estimated limiting waste stream cases or segments that I presented to you in January of last year. So, we looked at, remember, we looked at a simulation of the actual loading of the repository, the wastes that would be received at Yucca Mountain. We applied loading rules that we developed, and then investigated the actual sequence of packages that went underground, and pulled out the hottest sequences. So, those are plotted here also.

Now, remember, these are conditional simulations of what would happen if there was complete drift collapse at closure.

Overplotted on the curve is the--this is the window of temperature and time that we used to screen out the FEP on thermal sensitization of Alloy 22. If we kick the waste package surface temperature below 300 degrees C for 500 years, and then below 200 for 9500 years, we meet the justification for screening out that FEP. You can see that in the event of an early drift collapse, that might not be the case. The hottest package somewhere in that repository could exceed 300 degrees C, and this is, of course, associated with the low level thermal K estimate that I presented earlier.

So, I’m going to leave you with that thought. I’ll get back to why that’s not important, that those hottest packages may rarely exceed the 300 C temperature.

I want to switch gears here now and talk about the internal waste package temperatures. We used sort of a back door approach on this one. We looked at the TAD canister thermal specification, which is presented this way, as this table of values in the specification document. These are basically for preclosure. But, it says that if the TAD canister is throwing off 25 kilowatts of heat, the canister internals have to be designed so that the wall temperature is no higher than 181 degrees. And, this controls the internal temperature within the canister. Okay? The numbers seem backwards, but if the package output is only 11.8 kilowatts, the wall temperature can be higher and still meet the internal temperature limit. Okay?

So, this was done for preclosure reasons, but it’s clearly applicable for postclosure because we’re talking about a regime where the package internals are in the same configuration.

Now, let’s talk about how hot can things really be postclosure. At the time of closure, I went back to that thermal management simulation I presented in January, and looked at the hottest package of any at closure, 6.73 kilowatts, so that’s really the case that we have to concern ourselves with for understanding the maximum internal package temperature.

Next slide, please?

So, if I take the values from the TAD spec, and extrapolate them back to 6.73 kilowatts, I get a temperature of around 310 degrees C at the surface of the TAD canister. Remember, that TAD canister is inside of a waste package, and it’s got an inner vessel and an outer barrier wall to it. So, what we end up with is about 10 degrees C is available to propagate that 6.73 kilowatts through those two outer layers.

And, that’s, from my inspection of the problem, that seems adequate.

Next slide?

Okay, this is a summary here. The waste package temperature will be below 300 degrees, and cladding temperature below 350, except if you have drift collapse within the first 90 years after closure. And, I gave you the reason why that, the principal reason, there are other FEPs, I should say, that are also sensitive to the peak postclosure waste package temperature.

Seismic ground motion is the major cause. You can have no significant rockfall during that period, you can have partial, or a complete collapse during that period if you have a strong enough seismic event in that 90 year period.

So, let’s look at the probability that that will happen. In the analysis, we’ll consider only single event seismic probabilities because a seismic event large enough to cause significant rockfall is not likely to occur more than once in a 90 year period.

Next slide, please?

So, the approach is to use the seismic consequence abstraction implemented in a Monte Carlo analysis, simulating, by the plus one distribution, seismic events in 90 years. We then combined that with some simulations of the peak thermal effect of partial drift collapse. This is the first time you’ve ever seen analyses of--or may have ever seen analyses of partial drift collapse on the program. And, we appropriately weighted a sampling of the host rock thermal conductivity, which is clearly an important uncertainty in this problem, and we sampled the range of those curves for effective thermal conductivity that I showed.

Next slide, please?

So, in a nutshell, this is the result. As a function of time, this is the probability of any package exceeding 300 C, conditioned on a single seismic event occurring at up to 90 years after closure. And, what we get here is that if a single seismic event occurred at around 25 years, we might get as high as a 10^{-4} per repository probability of an exceedence temperature. So, this is the low level of risk consequence that we’re operating at. Now, this supports a low consequence screening decision on the various depths concerned with this peak temperature.

Next slide?

This is the same type of calculation, but for the two hottest cases I talked about earlier associated with the ELWS, the estimated limiting waste stream thermal management analysis, same result.

KADAK: Ernie, which seismic hazard curve did you assume for these--

HARDIN: The bounding size of hazard curve, it has 4.07

meters per second at 10^{-8}.

Next slide?

This was an interesting sensitivity that we did. If we took that same analysis and reset the threshold temperature from 300 to higher values, we wanted to see what happened to the probabilities. This happens to be a snapshot at 30 years after closure, and, you know, of course the result is the probability falls off quickly. So, what this argues for is that if you don’t have threshold type mechanisms operating at temperatures above 300 C, if you had incremental step responses above that temperature, then you have some leeway there, because the temperature is not going to exceed 300 by much, if at all.

Okay, next slide?

I’ve reviewed some of the conservatisms in that analysis, that probabilistic analysis that I just showed you. We haven’t looked at the stratification case where perhaps the low effect of thermal K could be raised a little bit if part of the debris field was coarser particles, which is likely. We did look at heat transfer by convection, and we didn’t look at correlating things like lithophysal porosity with the resultant rubble characteristics.

So, finally, the summary. Again, we have shown that the temperature of the waste package is going to be less than 300 and cladding less than 350 per the TAD design spec.

That's a TAD performance spec. Except for this case of drift collapse within the first 90 years, we've done a probabilistic analysis of that and shown that the probability is on the order of 10^{-4}, or less, decreases steeply if your threshold temperature is any higher than that. There are some conservatisms there, and overall, we have a limited thermal impact of drift collapse on TSPA. It doesn't happen until later, typically well beyond 10,000 years. And, that's it.

GARRICK: You did a wonderful job. Thank you. Thank you very much.

Are there any questions at this point? Andy, yes?

KADAK: The effective thermal conductivity of this rock, can you tell me how many actual measurements you took to assess that in the lithophysal rock?

HARDIN: Sure. We did five drift scale, meter scale experiments in the lith.

KADAK: And, what's the variability of that data? I'm sure some of it was wet, some of it was dry. How did you use that information?

HARDIN: You know, we started at in situ saturation. There is the potential for moving water around when you start heating that rock. We looked at that as an effect, decided that we could use a conduction only technique for evaluating the apparent thermal K behavior. The ranges are, I'm guessing here, I’m trying to remember those five tests. There were multiple realizations of each test. There were in a different power level. They had some different aberrations of the--or perturbations of the interpretation method. But, overall, the values were between, say, 1.5 watts per meter K, and, say, 1.8. This is lithophysal and an in situ saturation condition.

KADAK: So, it’s a fairly narrow range, is what you’re saying?

HARDIN: Yes. If we had it within our test scope to completely dry out the rock, we could have achieved some lower values.

KADAK: But, the dried out rock that you used for your experiment was what? What did you measure that to be for your heating test?

HARDIN: That’s a Bob question, Bob Jones.

JONES: Yes, Bob Jones, Sandia National Laboratories.

First of all, I have that number you were looking for with the in situ test, and this of course is dry rock, and these calculations were done above boiling temperatures.

KADAK: Okay.

JONES: So, the range that I have in my notes here is from 1.2 to 1.5.

KADAK: I’m sorry? I didn’t catch that.

JONES: 1.2 to 1.5 watts per meter squared.

KADAK: Okay.

JONES: Over at least the last two tests that we did, which were in lithophysal rock. I don't think they varied too much more than that in the dry rock.

KADAK: And, based on your presentation, Ernie, it sounds like backfill is not really going to be an option for this repository if you were to replace drip shields with backfill.

HARDIN: I will agree with you insofar as backfill by itself substituted into the design would not allow us to meet some of our thermal checks.

KADAK: Okay. And, on Slide Number 6 of the second presentation, I think, you went really fast on that, what did you say?

HARDIN: I said that the TAD thermal performance spec says it has to be able to shed a certain amount of heat.

KADAK: Right.

HARDIN: And, so, I used that, I applied that in postclosure and said look, the postclosure power is less than preclosure, which means that while maintaining the internal temperature of the TAD at its limit, the wall temperature can come up a bit. How far? This is the extrapolation here, 310 degrees C. And, at that temperature, you're inside the outer envelope of the waste package, so you're allowed 310 minus 300 is 10 degrees of thermal difference, temperature difference, to get the 6.7 kilowatts out from the TAD canister wall to the waste package wall. And, that’s reasonable.

KADAK: Let me think about that again.

HARDIN: Okay.

GARRICK: Ron?

LATANISION: Latanision, Board.

Could we look at Number 7, please? The thermal sensitization of Alloy 22 that you’re referring to there, there’s a phase transformation you’re concerned about, you’re not really talking about sensitization in a classic metallurgical sense, are you?

HARDIN: That’s the name of the FEP, and I think it includes a small handful of different processes, of which phase separation might be one of those.

LATANISION: Duquette may remember this better than I do. Does Alloy 22 sensitize at 300 degrees centigrade, or is there--there is a phase transformation--

DUQUETTE: There is a phase transformation. It’s about 300. It takes a very long time at that temperature.

LATANISION: But, it’s not sensitization in the sense of carbide accumulation?

DUQUETTE: No. No.

LATANISION: Just semantics, Ernie, but it does mean something to a metallurgist.

HARDIN: Okay. I could tell you that the 300 degree limit is based on some laboratory data, long-term tests, where they quantified the, you know, the separation behavior, and then looked at its relationship to exposure time and temperature, and then fit an ehranneus expression to it, and then extrapolated it back about 200 degrees to get to this temperature.

LATANISION: Yes. Okay, thank you.

DUQUETTE: Duquette, Board.

What wasn't done, though, was to do very extensive corrosion experiments on the material after it had been phase separated. They simply show that you get phase separation.

GARRICK: Very, very good. All right, if there's no further questions, I think we will adjourn until scheduled 1:40 return time.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

KADAK: Okay, if we could grab our seats, my chairman has said that we have to start on time, and finish on time. And, my record on that is not good.

Okay, this afternoon, I'd like to make a few opening remarks about criticality. As you know, this has been one of my issues for a while, and I think one of the key questions from the Board's perspective is do we need to reopen all those hermetically sealed, welded packages, as spent fuel now currently is collecting at the existing reactor sites prior to disposal, namely, the multi-purpose containers, and the dual purpose containers, and whether or not they can be directly disposed as equivalent to what is a TAD.

And, one of the issues about that is, of course, the question of re-criticality in a repository environment, assuming that these canisters become flooded in the long-term. So, what we're going to explore today is a number of issues that relate to that, most significant of which is the burnup credit question.

So, we have three people, we're going to run it as a panel, even though we don't have like a panel table, but the speakers are going to be right here available for you and us to question. We are going to follow the program as listed.

The first person speaking is going to be John Wagner from Oak Ridge National Laboratory. John, if you would come up, please, to the podium? John is going to talk about the role of burnup credit and how it can or perhaps should be excluded, or included, in the analysis of postclosure criticality. John?

WAGNER: Thank you, Dr. Kadak.

As he mentioned, I would like to give an overview of criticality and postclosure, the basis for the exclusion, and the role of burnup credit.

Next slide, please?

I’d like to start off, as the outline shows here, to give you a big picture perspective on postclosure criticality in the repository, talk a little bit about our control strategy for it, the considerations and the factors that are required to occur before you can have a criticality even. The features, events and processes that are relevant in how we do our screening.

Next slide, please?

So, this big picture perspective, you know, starts with the regulation, one of the requirements that we’re trying to satisfy. And, in the proposed 10 CFR 63.342, there’s a quote from it. And, essentially, it requires that the performance assessments that DOE performs shall not include very unlikely features, events and processes, where it defines very unlikely as those occurring with one chance, less than one chance in 10,000 over 10,000 years. And, so, this is our fundamental requirement that we're working from.

Can everybody hear me if I don't get up close to this? Can you hear me from here? Okay, I feel sort of funny leaning over this thing.

So, that's our requirement that we're fundamentally trying to meet. And, within this constraint, criticality is considered an event. And, a criticality event has been screened on the basis of low probability of occurrence. And, again, just to sort of put this all in one slide, our probability of criticality, which you will maybe often hear us call our POC, is determined to be 4.4 times 10^{-5}, which is obviously less than 1 in 10,000 by a factor of a little more than two.

Next slide, please?

The strategy, or control strategy in postclosure criticality is to use our NRC accepted methodology regarding features, events and processes, which is the title of that report is there. And, our reliance on engineered systems, natural systems, and the properties of the waste form to ensure that our probability of criticality is less than the screening threshold. So, it's a combination of these.

Now, with regards to how we do the in-package criticality control, it relies on neutron absorbers, these are absorber panels between the assemblies, a design feature, and the waste form in terms of burnup credit for the commercial spent nuclear fuel. So, that’s where--and, I’ll go into it a little bit more, but that’s the role of burnup credit in here, is for commercial spent nuclear fuel, to ensure that we can screen criticality.

How burnup credit is applied is through something called a burnup credit loading curve. And, I’ll show an example and kind of walk you through that in a moment.

These are developed such that, again, we preclude criticality from being a screened in FEP. And, the loading curves, our analyses are based on design basis conditions, conditions that we feel are justified and defendable within the environment. And, I’ll get into that a little bit further in the talk. And, they do involve fully flooded conditions.

I want to make a point, too, there’s a lot of discussion about the variation in the amount of burnup credit that can be taken. And, within postclosure, within our the way that we do things, the variation in the amount of burnup credit affects the amount of assemblies that can be loaded through our, of course our loading curve again, I’ll have an illustration in a second, but does not really affect our POC. Our probability of criticality does not depend on where our loading curve is.

Let me try to illustrate in my next slide, please?

Let me give some background, because not everybody is familiar with what a loading curve is, so, let me try to explain. And, let’s just focus on one of them right now. These are loading curves for PWR and BWR commercial spent fuel. On the horizontal axis, we have enrichment right now, fuel limited to 5 weight percent enrichment, if I can find the upper bound here. On the vertical axis, we have assembly burnup, and the plotted axis goes up to 80 gigawatt days. The red line is from our SAR. This is our loading curve. Superimposed, this loading curve is superimposed on our waste form inventory. Let me explain what these boxes mean.

Each one of these boxes represents a number of spent fuel assemblies in our waste inventory that we must try to accommodate. And, they are color coded based on how many it represents. So, actually, if you look at this little gray box, it corresponds to assemblies of 4 percent enrichment, and 40 gigawatt days. You will see that that box represents greater than 500 spent fuel assemblies. So, that’s just how to read the whole plot.

How much burnup credit we take affects where this curve is. If we were able to get more credit for burnup, it drops this curve, less credit, and it raises this curve.

Now, so what does this curve really mean? It defines the loading in terms of how much the minimum burnup is required of an assembly, given a certain initial enrichment. And, all points along this curve and above are burnup enrichment combinations that satisfy meeting our critical limit value for loading. Okay, is that all clear? Or, maybe we can get questions at the end. But, this is a pretty important slide in terms of our loading and how burnup credit is utilized.

In the case of PWRs, you will notice that we have assemblies that are below that curve, and we have to deal with those in one way or another. And, they also influence our evaluation in terms of probability of criticality because they represent potential assemblies for misloading.

In the case of BWRs over here, as I’m sure all of you recognize, BWR assemblies are much smaller, and, so, they have more interstitial poison plates. You’re able to put more interstitial poison plates per unit mass of fuel. So, the loading curve is down there. All BWR assemblies are acceptable for loading in the TAD package as it is. So, we don’t have any probability of misloading in the BWR case.

Next slide, please?

So, in the criticality evaluation, going back to that, we have to look at occurrences of all conditions necessary within the repository, and evaluate the probability of all of those.

And, in regards to that, we also have to look at all the various ranges of parameters and probabilities and probability distributions, where applicable, to determine and demonstrate compliance with the applicable regulations.

Next slide, please?

I’m sure all of you are pretty familiar with this. I won’t spend time on it. But, just to give you some context, particularly in terms of in-package and external criticality. In-package as you would expect, in the package. External is near field, is in the invert region, far field refers to criticality outside of the invert region.

Now, as all of you also recognize, during the long period of disposal, there’s a lot of different things that can happen, a lot of different conditions that need to be considered and evaluated for. We have our changing repository conditions, temperature, humidity, chemistry, the effect of degradation of the package, the basket, the waste form, all of those sort of things. And, we have water movement, which is very important to us in terms of criticality because water represents an effective moderator. It also enables transport of radionuclides within the package, or external to the package.

We have changing waste package conditions that we must consider, both material degradation in terms of the loss of the barrier, as well as changes in the basket configuration. Changes in geometry and material degradation in terms of the basket literally falling apart, our absorbers corroding and being reduced in effectiveness.

And, then, of course, changes in our spent fuel conditions themselves, in terms of cladding, assembly structure, as well as in terms of we have changing conditions in terms of the isotopic composition of the fuel through decay and build-up processes. And, I have an illustration of that.

It’s a rather complicated figure. Let’s sort of break it down to pay attention maybe just to the red line first. This is along the horizontal axis, we have cooling time on a log scale, and this is just a representation of reactivity along the vertical scale. And, I just really want to illustrate how things change over time.

The red line corresponds to what I would call sort of a best estimate, all the relevant actinides and fission products are included. And, what we see here is that as fuel is discharged out of a reactor because it can no longer sustain a criticality within the reactor, within the first 100 hours, reactivity increases quite dramatically. It increases due to the decay of short-lived fission products. And, so, there’s this process here where inside the reactor, those fission products are being built up and exist, shortly after shut-down, these all decay away very quickly, again, within the first 100 hours.

From about 100 hours on to about a year, reactivity is fairly stable. And, then, after that point, you’ve got basically the effect of these two decays, where you have Plutonium 241, which is a fissile isotope, decaying into Americium 241, which is an absorber. You also have the buildup of Gadolinium 155, which is a very strong thermal neutron absorber, from the decay of Europium. And, because of that, you have this drop off in reactivity that bottoms out around 100 years.

Shortly after that, you’ve got other decay and build-in processes that occur that actually turn this back around, and we see it peaks out in the 10 to 30,000 year range. Okay, these peaks, the exact location of where that occurs, does depend on burnup and a number of conditions within the analysis. But, the overall shapes of these things is really what I want to illustrate here.

Now, I’ll just say a few words about the other two lines on the curve. This brown line with the square boxes represents the majority of the principal actinides and fission products, things that we try to take credit for in criticality safety analyses. Now, we don’t see this build-up when we have those, because those short-lived fission products are not part of that set of nuclides that are used in those calculations. But, you see the mirror of the behavior. And, then, the blue line just shows the difference when fission products are not included in the analyses, something that’s referred to as actinide only.

Next slide?

I’ll just try to show you an illustration of how reactivity changes with time. This is just some illustrations of the different scenarios that we have to consider in terms of basket degradation. We cannot say with assurance that things stay in a pristine as manufactured condition. In fact, there’s reasons to believe a lot of these, or at least that these things can happen, and, so, we have to consider them in the overall analysis.

So, some of this may be sort of obvious, but in order to look at the probability of criticality in the repository, we need to first look at what is needed to actually support criticality. And, the first thing that’s needed is to have the waste package breached. Some initiating event is required, because if we don’t have any moderator within the waste package, the reactivity of the fuel is actually quite low.

So, the first thing, again, initiating events, waste package breached, barrier failure.

Then, we need to have a moderator, whether that’s human error or liquid water, they are necessary before we really even have the potential for criticality.

Then, on top of that, we need to have materials inside the package degrade or reconfigure in some way, whether that is a manufacturing error, where the absorber panels were not included, or whether that’s a degradation in the materials inside, and I’ll say a few more words about that in the following slide. That’s for in-package criticality.

For external criticality, we need all of these things to happen, but we also need sufficient accumulation of fissile material outside of the package. We need to form a critical mass which basically involves a fissile material in some small localized collection point to enable criticality external to the package.

So, first step, obviously, is to look at our initiating events that would result in waste package failure. These are where all of our potential event sequences must start. And, so, a large number of scenarios have been evaluated over the years. The initiating events that have been deemed and determined to be important and relevant for further consideration are something we refer to as early failure. You might hear us call it nominal situation. And, this is scenarios in which the drip shield and waste package fail through corrosion or misplacement, things like that. I’ll have a slide on each one of these. Seismic, if you have a seismic activity that causes a breach in the package, igneous and rockfall.

So, this correlates to 16 features, events and processes with relevance to criticality. And, they consider combinations of location and initiating conditions.

The criticality as a whole, as I said, is considered an event, and the event class, or the combination of all of these 16 FEPs is screened on low probability.

Here's just a summary chart to illustrate the role of burnup credit within these. Along this range, we have the locations. Here we have initiating events. And, we rely on burnup credit and neutron absorbers in these two conditions, well, on the in-package conditions, for both seismic and early failure.

Now, getting to the criticality calculations or the evaluations, I really didn't go into any of the details in this slide in there of how we do things. We use very accurate Monte Carlo calculations and state of the art in terms of transport calculations. I kept this at a fairly high level so we can, if people have detailed questions about how the analyses are performed at the end, I'd be happy to try to answer those.

But, at a high level, we maintain consistency with standard practices in criticality safety evaluations. We develop design basis configurations that we believe are justified and defendable based on all the differing configurations that can occur. And, those are used in the criticality evaluation to bound, in terms of reactivity, the relevant variations in each waste form.

You know, one of the big issues here, or one of the big things that affect everything, is the presence of water, and because it’s not possible to definitively rule out the presence of human error or water within the package, our design basis assumes fully flooded conditions.

And, the other thing we have to be concerned with, in addition to just the package flooding with water, is human error in the formation of the mineral called schoepite, which as you can see from the formulation here, has considerable hydrogen and oxygen, which are moderating materials, and so schoepite, the schoepite mineral can actually be quite reactive, and we also have to evaluate for it.

The common events that dominate our probability of criticality are mentioned here. And, basically, this is derived from the report here, the Configuration Generator Model Report, where over 50,000 event sequences were generated, and we looked for common themes.

Two themes that show up continually throughout these different scenarios are the improper manufacturing resulting in the absence of absorber panel or something that would cause a loss of effectiveness of the absorber panel. And, the other is, in commercial spent fuel because we have this loading curve that I showed, we have the potential for improper loading. And, that would be loaded an assembly that is not deemed acceptable based on where the loading curve resides.

Next slide, please?

So, we go through an event tree type of approach to determine the ultimate probability of criticality. And, this is a high-level illustration of that. First, we have probability for some initiating event that's going to result in a breach of the waste package.

The next, we must have water, as was already discussed. And, because we can't rule it out, we set the probability of water to 1. Then we have a design basis configuration that we must consider, so we develop that on what is defendable, and we assume the probability of getting to that design basis configuration is 1.

Now, each type of waste form and package has its own design basis configuration, and, so, if we had new design basis configurations, or things to consider, it would be different and we would go and we'd go down another path.

Now, as I mentioned, there's two common aspects of these sequences. We have neutron absorber misload or waste form misload. So, here, we've had breach, water enters, we have our design basis configuration, and then if we have a neutron absorber misload, we reach a probability of criticality associated with that absorber misload. If we don't, we go down an alternate path where we can have waste form misload. And, again, this is like assembly misload.

And, these ultimately drive then to our various probabilities of criticality. So, this is, again, just to illustrate the logic flow in a determination of probability of criticality. The probability of each end state is subsequently summed for our total probability of criticality.

I have a slide sort of on each of the initiating events that I'll go through fairly quickly. We, in postclosure criticality, are the users of information from a variety of the other disciplines, many of which you heard this morning. They develop data that we then ultimately use as our probabilities of the initiating events, and how we use them.

The early failure scenario is calculated based on the presence of weld flaws in the corrosion barrier, or early failure mechanisms, such as, in this case, another drip shield.

It assumes, as all of ours do, moderation is present immediately resulting in our design basis configuration. And, then, the probability for drip shield misplacement, I'd just note that the probability of localized corrosion is set to 1. So, if we have a drip shield misplacement, we assume that we've got corrosion, and probability of barrier breach is then 1.

Through all of these options within early failure, being flaws and misplacing the drip shield, we come up with a total sequence of criticality for the in-package case of 2.1 times $10^{-7.}$

Next slide, please?

For the seismic scenario, we have to consider a variety of things as well. We have to consider vibratory ground motion effects. And, this actually is the one that really drives our probability of criticality in terms of magnitude.

For the TAD packages, which are rather structurally solid, we result in a probability of criticality from vibratory ground motion on the order of 5 times 10^{-7}.

The same cannot be said for the DOE spent fuel packages. And, having said that reminds me that I need to be clear, that the entire scope of this talk is related to commercial spent fuel and DOE spent nuclear fuel. The Navy fuel, the analysis is classified, and it’s not a part of this talk. So, I just wanted to make that point clear.

KADAK: So, what was your reference to the DOE fuel? Did you say it was included or not included?

WAGNER: It is included in this talk.

KADAK: But, not the Navy fuel?

WAGNER: But, not the Navy fuel.

Just sort of as a reminder that I needed to make that clear.

Going back to my previous train of thought, the DOE spent fuel packages are, in our structural analysis, are not nearly as rigid or structurally sound, which results in a much higher probability of criticality associated with them than for the TAD package. And, as I said, the difference is due to the structural integrity of the two packages.

For fault displacement effects, we calculated another probability of criticality. And, then, there's multiple events that can result in a breach and significant rockfall on the drip shield, which results in a localized corrosion of the waste package and a subsequent probability of criticality.

All of these are put together for a total seismic probability of criticality, which is listed there, and, again, which is dominated by the DOE spent fuel packages.

For the igneous scenario, we screened out really based on the low probability of the initiating event relative to other contributors to probability of criticality. There was some discussion this morning about the probability of an igneous event. At the time of the License Application, it was 1.7 times 10^{-4}. So, the initiating event, in itself, is very near the threshold for screening, and much, much smaller, I think it's a factor of 1400 below the initiating event related to the codisposal package in seismic. And, so, we screened it based on its low initiating event, and subsequent contribution of probability of criticality.

This is just a schematic to basically bridge into external criticality discussion. Basically, we have water and chemistry aspects that go on to corrode the waste barrier, or some mechanism for breaching the barrier of the package. We have waste form degradation, and then we must have some kind of transport of fissile nuclides outside of the package to be able to support either a near field or far field critical mass.

So, analyses, geochemistry analyses are done for each of the waste forms to determine whether or not it’s possible to accumulate a critical mass outside of the package. And, they involve a number of conservative assumptions, which I will briefly go through here. Actually, all of those are predicated on what is a critical mass of material outside of the package, determination of how much mass is needed to become critical.

And, a variety of sensitivity studies were done to determine that in the materials in the nonlithophysal and lithophysal zones within the rock formation.

These analyses assume that any material that’s released from the package that is not a fissile material, uranium or plutonium, gets neglected. So, again, there’s a number of conservative assumptions that go into this. We’re assuming uranium and plutonium come out, fission products and absorbers stay behind.

Each fuel is modeled in its as manufactured condition, which means no burnup credit is utilized in these scenarios. And, then, in terms of determining the critical mass, the most reactive moderation and reflection conditions are assumed.

And, doing all these, taking these conservative assumptions, the result of the various waste form evaluations demonstrate that we have insufficient material in either the near field or far field locations to support an external criticality.

This is really sort of the summary slide taken directly from the License Application for the probabilities of criticality. It really just summarizes what I’ve been through. The in-package, the various contributions to probability of criticality from nominal or early failure, seismic, rockfall, igneous, for a total for the DOE and commercial spent nuclear fuel. And, for completeness, the probability of criticality for the Naval fuels is listed here. The total probability of criticality is the sum of those numbers.

So, in conclusion, you can sort of probably read this as well as I can, but the crux of the matter is that we do have numerous and significant conservative assumptions that are required and utilized in this analysis. Even with these, the probability of nuclear criticality during postclosure is able to be determined to be very unlikely and screened on low probability of occurrence. And, so, the entire event of criticality is screened from the performance assessment.

And, that’s really all I have.

KADAK: Okay, thank you. Questions? John?

GARRICK: Here, to the whole wide world, I’m about to reveal my ongoing bias against rules and regulations. But, if you were a criticality scientist and asked to look at the total waste management system, and maybe you have done that, what would you consider to be the most vulnerable phase of the system from a risk perspective, that is to say, from a standpoint of possibly injuring people? I doubt if it would be this.

WAGNER: I would not disagree with that.

GARRICK: Has anybody that analysis?

WAGNER: Not that I’m aware of. No.

GARRICK: Okay. I’m also a little surprised to see the NRC continue to hang onto this design basis concept, which has been demonstrated to be pretty archaic in reactor risk analysis. Do you know what the risk is of a fully flooded system?

WAGNER: Let me try to answer your question, but let me sort of make a comment first.

In the analysis, the criticality safety analysis, our design basis configuration really is a set of conditions and assumptions that are considered to be possible.

GARRICK: Right there, you depart from a risk perspective. You don’t consider anything to be possible. You indicate what you think that possibility is. But, go ahead.

WAGNER: Well, really, you’re going from a nominal situation to a bounding, you know, in terms of what you choose for your assumptions. Our criticality safety, or our design basis conditions are based on bounding values. If we know a parameter can vary from here to here, and this is more conservative in terms of reactivity, we use this value. So, from that standpoint, I guess we are conservative in bounding.

Now, I’ve already forgotten what your question was.

GARRICK: Well, I just am kind of amazed that the NRC, after the lessons they’ve learned about risk assessment, continue to invoke some rather archaic concepts in the regulations relative to criticality. And, I think that a classic example is this flooded system. Because, in the old days, in the reactors, they also invoked the design basis philosophy, and they still do to some extent, but it’s quite different, and the design basis accident was usually some sort of a guillotine break of the primary system.

And, of course, the risk assessments that eventually and convincingly demonstrated that that was essentially a low and no-never-mind risk. And, as a matter of fact, we took the attention off what was the real risk with respect to operating nuclear power plants. And, I see the same thing here. I don’t see a profile that gives me confidence that they understand what the risk issue is with respect to criticality over the total waste management system. And, I guess I’m raising the question is anybody looking at that?

And, the other thing is what is the threat of a criticality event in the repository? Has that been analyzed?

WAGNER: Well, I’m not sure what you mean by threat. I mean, we’re calculating--

GARRICK: I’m talking about we worry about nuclear facilities. We worry about nuclear facilities because of possible radiation damage to either the environment or people. And, I’m suggesting that--I’m asking, really, what is the radiation risk of a criticality event in the repository?

WAGNER: Well, what I think you’re asking is what are the consequences.

GARRICK: Yes, that’s exactly what I’m asking.

WAGNER: And, basically, we’re showing that we can screen it on probability without consideration further. And, in some cases, you might say that the license and regulations are risk informed, in the sense that they are basically telling us that if we have a very unlikely event, we do not need to include it in the performance assessment. And, so, that's what we're doing. Now, as far as what is the--

GARRICK: See, the question hasn't been answered. It's what is the criticality risk in the waste management system? And, that's what disturbs me. That's not been answered.

WAGNER: Let me try to understand what you're after, because the waste management system, from the time it comes out of the reactor, all the way to disposal, and what are the risks in that whole process; is that what you're asking about?

GARRICK: Right.

WAGNER: Okay.

GARRICK: Criticality risk. My guess is that the criticality risk is going to be much greater out of the repository than it is in the repository. And, so, all this messing around about the criticality risk in the repository is kind of an oxymoron compared to what the criticality risk for the waste management system is. And, I just think that's kind of strange.

WAGNER: And, when you speak of risk, you're speaking of risk to the population?

GARRICK: That’s the only reason the NRC exists.

WAGNER: Okay. I mean, the bottom line is when we look at the criticality, we screen below the threshold, and, therefore, the risk to people from a criticality accident is inconsequential, insignificant, and not being considered further.

As far as I was speaking, I remembered your other question, and that is what is the probability of water getting into the package.

GARRICK: Right.

WAGNER: And, I don’t know the answer to that. I do know that based on the various mechanisms, we cannot definitively say that water or human error cannot get into the package. And, while water is an obvious one we have to be concerned with, even human error through the formation of the schoepite mineral, is something, from a criticality perspective, we do have to evaluate and ensure that we consider and include it in this analysis.

GARRICK: I guess I don’t understand why that’s a more complicated problem than the problem of judging how water accesses the waste package and degrades the waste package. But, I understand what you’re saying. Well, I just think it’s sort of a paradoxical situation.

WAGNER: Well, I think, though--

GARRICK: We get so locked into the regulations that we stop thinking about the problem. You know, it’s like the pilot that took off from LaGuardia, if he had been locked into the regulations to go to the next airport, which he was told to do, there would be 155 people dead today. But, he was a risk manager, not a compliance manager.

WAGNER: But, if I may make one more comment about that, though? We have a set of regulations. We demonstrate that we can satisfy them. And, it’s screened out. So, I don’t really see that there’s a problem.

GARRICK: Yeah.

KADAK: I think we got John’s point.

GARRICK: It’s fun to be on this end.

KADAK: I know. It’s not fun to be on this end.

Any other questions from the other members of the Board? Because I’ve got a few.

In terms of this chart on Slide 5, are you saying you’re using that standard red-line curve as your loading curve that you’re using in all of the analyses?

WAGNER: The analyses generates this loading curve. The analyses and the calculations form the basis for that loading curve.

KADAK: So, what do you assume for the loading of a typical TAD, let’s just say, that you use to conclude certain criticality or lack of criticality events?

WAGNER: Okay, all points along this curve--

KADAK: Which curve? The red curve?

WAGNER: The red curve. --represent burnup enrichment combinations that define our design basis configuration. So, we are assuming in the whole process that we have assemblies loaded that have, for given enrichment, have at least that much burnup.

KADAK: All right. So, that’s not like the real loading that might occur in a TAD, for example? It’s just your reference design that says you’re going to try to meet that target, and that’s why you’ve got to assign certain probabilities for misloading using those points on the bottom?

WAGNER: Right, you bring up a good point. This defines then, or this feeds into our estimate of misload.

KADAK: So, that’s your analytical norm. Then, if we go to Slide Number 9, does your analysis basically assume full burnup credit and poison in the TAD to make the numbers work, neutron absorber poison, the TADs are required to have?

WAGNER: Our analysis assumes a 29 principal isotope, actinides and fission products.

KADAK: Okay.

WAGNER: And, a degraded absorber. Let me address a couple issues here. First of all, in terms of isotopics, we do assume 29 principal isotopes, which are a combination of the principal actinides and fission products with relevance to reactivity, and also have properties in terms of they stick around.

We assume five year decay on those isotopics, which bounds anything beyond it, and is defendable position in terms of it bounding any time beyond it.

We, as sort of a contrast to that, we assume degradation of our neutron absorber panels consistent with 10,000 years of corrosion.

KADAK: So, those are effectively gone?

WAGNER: No, they’re not gone. Actually, we have corrosion studies that will justify that we have at least 6 millimeters of the absorber panels still there after 10,000 years of corrosion.

KADAK: But, you’re taking credit for burnup and also neutron absorber poisons that have been put in the casks?

WAGNER: Yes.

KADAK: Your analysis.

WAGNER: Yes.

KADAK: That’s what I wanted to get. Now, if you go to Slide Number 10, can you tell me how you calculated the last bottom figure on your right?

WAGNER: This one here?

KADAK: Yes.

WAGNER: What we do here is, what we were just talking about is these kind of configurations, and actually, in any of our commercial fuel assemblies, the separation of rods and moderator in between are actually optimized, as you can imagine, for use in the reactor in terms of reactivity. So, then, we start looking at these degraded states, and we look at different mass fractions of the materials that are there, and the fuel.

And, actually, in a lot of cases, something like this is considerably less reactive than something like this, even if you throw the absorbers out, because there’s just not a lot of room for moderation. Basically, you want moderator interstitial with your fuel.

In scenarios like this, where they’re like this, though, this is another area where schoepite formation is something that we have to carefully look at.

KADAK: So, you attempt to analyze that?

WAGNER: Yes.

KADAK: Okay. In terms of the DOE spent fuel, and its apparently larger probability of criticality, how did that get through the system to allow it to be less robust?

WAGNER: I probably said those words, less robust.

KADAK: You did.

WAGNER: The real fact of the matter is that the way those are with sort of an inter-pack, inter-canister, and the webs that go between them, there really is not, the structural analysis is really not taking complete credit for the structural integrity of those designs. And, frankly, we haven't needed it. But, that's the main reason. I'm sorry, Peter, did you want to--

SWIFT: Peter Swift, Sandia.

The point there is that the DOE fuels are loaded into the codisposal packages together with the glass waste forms. The packages do not have a TAD container between the waste and the inner vessel. In our seismic analyses, those are the ones that are more vulnerable to cracking under lower ground motions.

KADAK: Okay. And, you mentioned the Navy nuclear fuel. Has anyone reviewed those analyses to see if the 10^{-6} number for criticality is valid? Because, as I recall, Navy nuclear fuel is much high enriched, and will be so for a longer time.

WAGNER: Of course, those analyses have gone under the same level of review, and so forth, as everything else. Those analyses are classified, even the details of the Navy fuels are classified, and, so, the short answer is yes, they have gone through review rigor.

KADAK: Do they take credit for burnup and also neutron absorbers?

WAGNER: I'd really rather not speak to that. Perhaps a member of the--

MC KENZIE: John McKenzie, Director of Regulatory Affairs for the Navy's Nuclear Propulsion Program.

I can’t really give you an accurate meaningful answer in an open forum. We’d have to have a closed meeting to discuss the characteristics of Naval fuel, and the criticality analyses were done. If you’d like to have us arrange that, we’ll work on it.

Relative to the question that immediately preceded it about the review, we did have OCRWM individuals and Sandia individuals with clearances look at the work that the Navy did as part of the License Application, and, of course, individuals at the NRC with clearances are reviewing those calculations now to verify their accuracy.

KADAK: Okay, thank you. Any other questions? Carl?

DI BELLA: Two quick questions. Carl DiBella, Board.

You left the impression that DOE owned spent--that you don’t use burnup credit for DOE owned spent fuel. Is that correct?

WAGNER: That’s correct.

DI BELLA: Okay. And, then, the other thing is the loading curve for PWR fuel. There was a small, but obviously finite population, below the curve that doesn’t meet the loading curve. What is the strategy for dealing with that fuel for disposal?

WAGNER: Are you talking about the PWR fuel?

DI BELLA: The PWR fuel.

WAGNER: Yes, just making sure. Actually, there are a variety of means that will have to be sort of explored on how to deal with that. I think we could speculate on various means to deal with that fuel, whether it involves short loading, whether it involves inserting of absorber rods into the guide tubes, or whether it involves multiple loading curves and preferential loading. There are a variety of ways that we can deal with that fuel.

KADAK: So, you will have criteria for TAD loading?

WAGNER: We already have criteria for TAD loading that deals with all those fuel assemblies above that curve. Once we have specific designs and we get into specific analyses, we will determine how best to deal with those assemblies that are below the curve.

KADAK: Bill?

MURPHY: Bill Murphy, Board.

Has anyone evaluated the duration of the criticality event if one got started? Wouldn’t there be a tendency to dry the system out and it would go away?

WAGNER: There have been some analyses performed to look at that and look at durations and what happens. So, that could happen. We are relying on screening based on probability.

KADAK: Gene?

ROWE: Rowe, Staff.

On your loading curve, the red curve, does that represent an effective of like .98 for the most critical geometry and fully flooded?

WAGNER: It represents a critical limit of 1.

ROWE: Okay. Does it assume that all of the assemblies in that package have the same characteristics?

WAGNER: It does.

ROWE: So, I cannot take assemblies with different characteristics and put them in the same waste package; is that what you’re saying?

WAGNER: No, you can. If we can go back to that slide, it was before this one, I believe, maybe two before.

This line defines the minimum burnup for a given enrichment. So, in the analyses, assumed that all assemblies are loaded with burnup enrichment combinations on this line. That’s the design basis. That’s worst case. This defines loading at a utility site. So, what this says is that they will have to, when loading, you have to have at least this minimum burnup to load.

ROWE: But, my original question was do all assemblies in a given waste package have to have the same burnup and enrichment?

WAGNER: No. They have to have a minimum burnup for a given enrichment.

ROWE: And, would it be possible to take some of those assemblies that are below that curve and do a waste package specific calculation, and see if you could mix one of those more reactivity, more reactive assemblies with some of--

WAGNER: Yes, and that’s what I sort of referred to as preferential loading as a possibility for dealing with these lower assemblies. And, without going into much detail, but just showing conceptually, one could come up with a loading curve that, or either some loading curve up here, where you’re basically mixing really highly burned fuel with really low burned fuel to meet the overall critical limit criteria.

ROWE: Okay, thank you.

KADAK: Just to follow up. What is your critical limit criteria, in other words, what are you looking for? .8?

WAGNER: 1.

KADAK: 1?

WAGNER: We’re looking at the probability of criticality.

KADAK: K effective one.

WAGNER: Yes.

KADAK: For your TAD design, with the overpack, the waste package?

WAGNER: Yes.

KADAK: What surprises me very much about all this, even if you take credit for burnup, is that you’re even close to 1, given that the spent fuel coming out of a reactor is spent fuel.

WAGNER: If we can go to Slide 9? What you see here is that the nuclear power, the reactors are driven by economics and generation of energy. And, so, when an assembly, reactivity is in one place that prevents it from continuing to maintain a critical cost effective power, as soon as you pull that thing out, within 100 hours in this particular example, you’ve jumped 6 percent in K effective.

KADAK: If you put it back in the reactor, and you’re still pretty bad.

WAGNER: Well, you put it back in the reactor and the short-lived fission products build right back in. And, so, that’s the--and, then, you have also differences in temperature, both in terms of the fuel and the moderator as well.

KADAK: Suppose you assume instead of five year old fuel as a starter, you’re talking maybe 30 year old fuel, which is probably more realistic for loading, how would that change your analysis?

WAGNER: Well, okay, the short answer is the loading curve will move downward, which will help acceptance, it will help accept more assemblies. The thing, though, that we need to be careful about there is that, let’s see, here’s ten years, and here’s 20 years, and, so, I guess that’s 30 years there, so that that doesn’t bound what can happen later in the repository in terms of reactivity.

KADAK: I don’t understand.

WAGNER: Well, reactivity builds back up, and, so, I need to be able to bound my reactivity for the duration of the postclosure period.

KADAK: That’s 10,000 years later.

WAGNER: So, five years is really selected because it bounds anything that happens at later times. Now, for transport and storage, I guess we’ll hear a little bit later, you know, those time frames don’t need to worry about 10,000 years. So, that’s more of an option in that environment.

KADAK: Okay. Any other questions? Carl? Okay, we need to move on.

Thank you very much, John. Sorry?

PARK: You consider fully--but some have assembly vibration because of 100 assemblies can be useless, and, so, in that case, the not so much, so, in that case, not so much, so in that case, that case the boroneutral absorber, has appeared accidentally. In that case, the criticality of some worst case, some--what is coming into that, in that case, the criticality can come up.

KADAK: I think that’s taken into account with his dots on that below that red line curve. So, I think it’s covered.

That’s Mr. Park from, I think, Korea?

PARK: Yes.

KADAK: Okay. Thank you, John.

MACHIELS: Good afternoon.

Just I'd like to thank John Wagner, because he made my task a little easier by introducing you to a number of complex processes, which are obviously involved in the analyses of a situation like doing a performance assessment for the geologic repository.

I'd like first of all to bring to your attention that I have a co-author. His name is Alan Wells, and sitting in the first row there. He is the one who actually did all the calculations that I'm going to report on. So, since I'm a sharing individual, all the tough questions will be referred to him.

Next slide, please?

What I would like to do is talk a little bit briefly about the background of this project, and then discuss the criticality evaluation in different framework, as well then the talks about some results that were obtained looking at variations about use of burnup credit methodology, and I will then finish by talking about some of the options which are available in terms of criticality control.

Next slide, please?

The background for this project is very simple. There were some contact and discussions between the TRB Staff and TVA, which resulted in some exchange of information coming from TVA, which provided to the staff basically a number of descriptions of different dual purpose casks for specific contents. And, there was then some expression of interest to EPRI in terms of calculations of what would be the figure of merit for nuclear criticality, which is the K effective for a couple of those dual purpose casks, assuming fully flooded conditions, neutron absorber, which is initially present in the canister, being dissolved away completely. Referring to the burnup approach, which is the purpose submitted for the Yucca Mountain project, and also specifying the date that such was needed.

And, we were very much involved in looking at some of those issues, but in a totally different context, in the transportation context, and so we thought that it would be indeed of value to do this type of calculation with some variations, if you will, understand later, because not only for the interest which we got, the potential applications, but also from what we could learn in the context of the transportation package.

Those calculations that we did are documented in the EPRI report, which is available to the public, and the website is indicated there.

Next slide, please?

So, what I would like to talk briefly about is criticality evaluations in the framework of three different applications. One is criticality calculations in support of reactor operation. The other one would be in the context of fissile material transport. And, the third one in the context of total system performance assessment.

And, in the first case for reactor operation, the purpose is for the obvious, obviously, is production of power. And, so, the objective is to be able to run your reactor and bring it to criticality in a safe manner, and the purpose there is to calculate the K effective, when the reactor is going to reach the value of 1. And, since this is for operation purpose, this has to be a best estimate approach. That means that you have to actually prove to yourself, as well as the regulator, that you are able to predict when the reactor is going to reach criticality for typical control conditions.

Tools that are typically relied upon by the industry involve some tool such as CASMO and SIMULATE, CASMO basically being focused on calculating the reactivity of each assembly, and then SIMULATE, putting them in the context such as the reactor context.

It’s awfully important to use the actual fuel parameters that you’re dealing with, obviously, to do the best estimate of calculations, and burnup is automatically, one of those parameters, and fully taken into account.

In a fissile material transportation scenario, the emphasis is somewhat different here. Now, we are entering the work of nuclear and criticality safety, so the objective is to make sure that there are a number of, a small number of scenarios that the material that you're transporting never reaches criticality conditions.

So, typically, you have to show that the K effective will not be over a certain value, which is below 1, and it's not unusual to basically have something like maintaining the K effective below something like .95, as a reference value.

The type of tools which are used are SAS2H or CASMO sometimes for calculating the reactivity of each assembly, and then putting them into the context of the fissile material transport package using a tool like KENO or MCNP, which are multiple types of calculations.

This is a highly conservative approach, even the safety implications, and as a result of that, that's what we discussed in the context of Yucca Mountain, there is reliance on a limited subset of design fuel parameters, which creates an envelope, and anything under that envelope is what's being allowed to be put in the transportation package.

Now, when we talk about burnup here, burnup is no longer the key. The key is how much credit you can get for the burnup. And, depending on guidance, practice and past history, it has changed and is still highly variable between absolutely no credit for any burnup, to credit for some amount of burnup. But, the burden clearly is to be able to justify in a fairly rigorous manner how much you can claim in terms of a credit for the burnup.

Those, therefore, rely, fissile material transportation, relies heavily on some kind of stylized type of analyses, and they have worked extremely well with regard to transportation of enriched material, plutonium and this type of thing. But, in our estimate, they don’t work as well when we talk about spent fuel, the reason being that spent fuel contains a very large amount of species, and during the analyses, according to the classic criticality safety approach, really has a tremendous burden in terms of using the approach which is typically used, and I’ll provide some detail later on that.

In a typical system performance assessment, the object then, as already extensively discussed, is what is the probability of having a K effective of 1. And, I think that John has spent quite a bit of time trying to explain that approach. And, again, now, from the type of tools which are used, they are very similar to what we use in the fissile material transportation. And, as John has indicated with regard to some differences, is that there is no arbitrary margin involved here. We’re not trying to show that the K effective remains below .95, or some similar value, but we are talking about probability of K, having a K effective of

1. And, the present Yucca Mountain approach relies on the use of a number of isotopes, which are referred to as the principal isotopes.

Next slide, please?

So, in our case, coming back into our original problem, is that we looked at a couple of dual purpose canisters, which are shown on this graph here on the left-hand side. There are 32 positions for a spent fuel assembly, and you all know the type of assembly that may go into this.

Next slide, please?

Now, in my presentation, I will basically present material which is essentially limited by what I’ve said before, is that we’re going to assume fully flooded conditions, for example, and we are going also to fix--indicated that we are going to take some kind of reference time, five years, understanding that this is only a small--this is a part of the story here.

In a truly probabilistic evaluation, you are going to have to take into account the principle that degradation processes are going to happen at some time, and that will be described eventually by a distribution of what may happen, and that will result in some partial filling of the cask to potentially full filling of the cask, and that the reactivity is going to change with time. And, so, you have to incorporate that into a--for an estimate, and you can see on the right-hand side, for example, that the reactivity is going to change tremendously as a function of the water amount in the cask. It makes obviously a lot of sense since the water provides moderation, and if you have no water, or very little water, you won’t have much of an effect.

That curve shows that you have to have about 5.8 to a little bit higher, maybe, of water inside the canister in order to have the K effective approach value. And, the other one is the one that the John basically spent quite a bit of time on it, justifying that five years bounds in our case, we need specific calculation for specific assembly. And, you can see that the equivalence is about, cooling for about 13 ½ years before, which basically would bound the burnup. But, as John indicated, this will change as a function of burnup enrichment, so there is some variation. This is a calculation of very specific assembly.

Next slide, please?

So, having said this, now, that the probabilistic approach is extremely important, but I’m going now to basically not talk much about it. These are the assumptions that were used. We have actual spent fuel assemblies discharged from Sequoyah reactors. There are 32 assemblies in every dual purpose canister. There is neutron absorbing material, METAMIC, in each of those dual purpose canisters. And, when I will be referring to as built, that means the neutron absorber is present. When I talk about disposal here, I assume the degraded conditions where all the neutron absorber is gone.

With regard to the rest of the geometry, simply assume that the canister, basket and the fuel assembly geometries remain unchanged. And, that we will also assume fully flooded conditions with water density equal to 1. And, I already mentioned the effect of partial flooding, which is very important.

We also did some sensitivity calculations, which means that now the temperature of the water could be greater than roughly 4 degrees room temperature, but it has a pretty minor effect as long as you assume that it’s present in its liquid form. And, we will stick with the cooling time of five years, understanding that the effect of cooling time is also significant. And, if we had chosen ten years instead of five years, all the results that we are presenting would have decreased essentially by a couple of percent.

Next slide, please?

So, this is one of the specific canisters. For each position, middle column, from 1 to 32, only showing 16 of them, we know the assembly ID, the initial enrichment, the burnup, and its exact position in the fuel assemblies. And, I have highlighted a couple of them, Assembly D64 in the red, Assembly N04 in blue, and their position in the dual purpose canister.

Next slide, please?

Now, those are results using best estimate, single assembly. Assume in an infinite geometry, which means that we can totally disregard the leakage from that assembly. So, that assembly is supposed to be present, and mixed with the exact same assembly in some kind of an infinite array. So, this is the maximum reactivity that you can obtain with that specific assembly.

For Assembly D64, the first one, the K infinity under disposal conditions is about .99. And, for Assembly N04, the K infinity over disposal conditions is about 1.02. That assumes values typical of room temperature. If you transpose the same assemblies under the same conditions, an infinite reactor, which only contains that assembly repeating itself in a reactor, then the values of the K infinity drops roughly by 5 percent, or so, going from .94 to .96.

There are two reasons for that. One is the density in the PWR environment is about 2/3rds of the density of the water at room temperature, and about 25 percent of the difference is due to difference in cross-sections.

So, this is using a best estimate calculation, where we have included the best we know how all the actinides, all the fission products, and that means well over 400 nuclides.

Next slide, please?

Now, this is now a calculation between the best estimate CASMO at the right-hand side of the column, which as I mentioned, has several hundreds of nuclides, compared to a slightly derivative of the Yucca Mountain methodology, which basically has one more fission product compared to the Yucca Mountain methodology, the Cesium 133. And, then, you can see how, for the same assembly in the same conditions, the K infinity, the sensitivity of the results using the best estimate compared to something when you limit yourself to 16 fission products. And, you can see that the .99 in the disposal conditions become 1.07, and the .02 become 1.08.

So, this is just to emphasize that when we talk assembly, the assembly doesn’t change, but obviously, the methodology that you’re going to use in terms of calculating the reactivity, the nuclear reactivity, is going to be strongly dependent on how much credit you can get for the actual burnup of the assembly.

Next slide, please?

So, now, I’m going to forget about this business of single assembly and I’m going to look at the cask, one of those Sequoyah casks, where we have 32 assemblies, each with their own characteristic and specific positions. And, now, since I have the geometry, I am able to calculate any potential leakage out of that cask, as well as take into account a number of structural material into the casks, which are also ruled into the neutron economy there, all the neutron balance. The thing that will change is really, again, the methodology to calculate the K effective.

Next slide, please?

One common element to the different options is the tough part. 14 uranium and transuranic isotopes plus the oxygen remains the same, and is also consistent with the approach used by the Yucca Mountain project. And, then, as John already indicated, you can do an actinide burnup. Actinide only burnup credit, that means you neglect all fission products. Or, you can do a five fission products, and typically, you will get the one that gives you the most bang in terms of their neutron capability, capturing neutrons.

There is a third option where you add one fission product, the Cesium 133, that Yucca Mountain doesn’t like, and then there is the 16 fission products. And, the next slide will show the variation between Option 1, 2 and 4.

Now, if you look at actinide only burnup credit, and first as the cask is as built, that means you taking credit for the neutron absorber, you can see that the K effective is .88. And, that obviously satisfied the licensing requirement for which the k effective would be greater than .95. So, this is a difference with regard to the actual content versus the design basis content if we had to impose a limit of .95 based on actinide only burnup credit.

Now, the disposal would be 1.06, when we neglected the near term absorption of the METAMIC. Now, as you add fission products now, you can see the difference is that five fission products would drop the value from .88 to .83, and for disposal conditions, from .06 to 1. As you go to the 16 fission products, the .83 becomes .80, and the disposal .97. And, now, if I take into account that I'm not limiting myself to 16 fission products, but I take credit for the several hundreds of nuclides, then the adding those several hundreds of nuclides, which individually have not much effect, but collectively, has some effect, then the K effective would drop from about .88 to .77. For as built, and to disposal from .97 to .92.

KADAK: Just to be clear, the disposal has no boron in it; correct?

MACHIELS: Right. The METAMIC, which is built into the cask is gone.

So, you can see the variations in the K effective in as built from .88 to roughly .77, and disposal conditions from 1.06 to about .92 in terms of the sensitivity of this type of calculation to the assumptions that you built.

Now, in a regulatory context, obviously, when you talk about transportation of fissile material, you have to be able to justify those values, taking into account that there’s some corrections that may be needed to those numerical results in terms of showing whether the methodology that you used has any barriers that would predict, under predict or over predict, as well as handling of uncertainty that may exist in the data.

Next slide, please?

Now, these calculations assume that we’re using the same burnup credit methodology relying on five fission products. I could have used 16, and I’ve been told many times you should use 16, but I have used five. Okay? Understand that. And, I like the five because the K effective is equal to 1 in these calculations. So, when I calculate 8 percent, it makes my life very simple.

What you can do is that those where I, as loaded by the utilities, but what you could do in principal, is put the least reactive fuel assembly in the middle of the cask where they matter the most from a reactivity point of view. And, that means that the one which has the least reactivity also has the highest burnup for a given enrichment, and that means that it also optimizes the situation from a shielding point of view, because if you put the one which basically, anything with the most radiation, everything being equal, and, so, from that point of view, you can basically minimize reactivity and shielding at the same time.

And, so, by simply changing the position of four fuel assemblies in that cask, or by changing 12 of them, as shown in blue here, you could drop the reactivity by roughly 1 percent if you rearrange four, or by 12 percent if you rearrange 16 of them. And, so, from that point of view, you can see that in the worst possible case, which was not the case of the way it was actually loaded, and the best possible case based on just rearranging four--you get variation of 2.3 percent, which is actually a fairly significant number in the reactivity evaluation.

Next slide, please?

KADAK: Albert, could you speed it up a little bit, because we're running a little bit behind? I mean, you're making very important points, I don't want to take those away from you.

MACHIELS: Almost done.

KADAK: Thank you.

MACHIELS: This is the effect of water, WABA or BAA. That means that if you leave in the guide tubes some material that will not allow water to be present, you will decrease the reactivity. This is called moderator displacement, and you get another effect, which are up to about 23 percent in addition to what we discussed before.

Next slide, please?

And, finally, this is the one that has the more potential if you wish to go there, and really, you could Spike, that means add some control rods in those tubes, and you can get pretty dramatic results. This option would be more only considering the context where you would have underburned assemblies, which have a significantly lower burnup than the design basis, and then you could go to this type of approach.

Next slide, please?

So, I’d just like to say a little bit about biases and uncertainties, understanding that when we talk to the regulator, we spent 95 percent of our time in this area. And, you may have some times, too. You will have to take into account that in any calculations you make, there are some biases, and when we talk about fissile material transportation, we do that in a very unscientific manner. Systematically, if you under predict, you will have to correct that, but if you over predict, too bad. You just have to live with it.

Uncertainties only work in one direction. It doesn’t matter whether the uncertainties can cancel when you do fissile material transportation, you basically assume the worst in terms of the effect of the uncertainty.

Now, in addition to that, as mentioned, there is an arbitrary safety margin which is being imposed on those calculations. Clearly, in the context of fissile material transportation, a lot of the challenges basically, when you want to take into account the larger share of fission products, is really the burden of proof to show that you can do it, and it has an effect on the final result.

In system performance assessment, in principle, it should be handled in a more rigorous manner, that under prediction and over prediction should be treated as such, and uncertainties should be allowed basically to be considered in both directions, given that in this case, we are trying to assess the probability of reaching a K effective of 1. And, so, from that point of view, building conservatisms into the calculation doesn’t give you the right answer if you don’t do it in some rigorous manner.

Next slide, please?

So, this is my concluding slide. The power of what we did, in a way, is to the fact that we can actually look at actual fuel inventory. We don’t have to rely on some kind of loading curve, which basically defines what is feasible or not. By defining the position of those loading curves, you have to make a number of assumptions about limiting fuel and that takes quite a bit out of the inventory. If you look at specific material, you can do a realistic calculation, which basically takes into account that none of those fuel assemblies typically are closed to the conditions which are defined in limiting a bounding curve.

Some casks have some used burnable rods, and then they could benefit from moderator displacement. With regard to future loading, you could add loading optimization, if you wanted to. You could put some material that would displace moderator, and you could also at the limit put some corrosion resistant control element.

With regard to analysis methodology, when we talk about the system performance, probabilistic approach rules. And, so, from that point of view, it basically can lead to the fact that we don't even have to consider consequence analysis, as was discussed extensively in the previous discussion.

And, with regard to burnup credit, the key is obviously to get enough, using appropriate treatment of the uncertainties associated with either fuel composition of the nuclide parameters.

Thank you.

KADAK: Thank you very much. Questions, please?

Apparently, I'm the only one interested in this topic on the Board. So, any questions from the Board members?

I will try to be brief. Thank you very much. I guess what I'm trying to distinguish is between your presentation and John Wagner's presentation. And, John's was pretty much focused on trying to justify FEPing out of postclosure criticality, based on a probabilistic argument. Your presentation was aimed at trying to see if we could justify direct disposal of DPCs, or MPCs, taking full burnup credit without worrying about whether boron survives or doesn’t survive. Is that the correct distinction?

MACHIELS: I was not that ambitious, because as John indicated in his presentation, when I looked at the very specific situation, which is fully flooding, full flooding and the absorber completely gone, and that’s a part of the analysis, but that’s not the whole analysis, the method of decision whether the DPCs are disposable or not. You have to, obviously, go to the full treatment.

KADAK: Which is your probabilistic treatment?

MACHIELS: Yes, right.

KADAK: Okay. The one thing that I, I read your report, and I found a number of conservatisms which you did take credit for in your analysis, including some of these uncertainties. And, I was trying to add up the total conservatisms, but it appears that the conservatisms were represented by the difference between your CASMO result and the SAS4H or other.

MACHIELS: Yes.

KADAK: So, could we go back to that slide, if you will, where the CASMO best estimate slide compares with--I think that’s the one, yes. Where we’re looking really at best estimate of .92, which is clearly fine.

MACHIELS: Yes.

KADAK: Compared to even with full burnup credit, and, say, 16 fission products, which is also fine at .97. Those numbers include uncertainties; is that correct, or not correct?

MACHIELS: Those numbers do not include the biases and the uncertainties, no.

KADAK: So, if you included those, they might, at least on the actinides, would those go up to over 1?

MACHIELS: In the context of fissile material transportation--

KADAK: No, I’m not talking transportation. I’m talking just disposal.

MACHIELS: Okay, disposal. Okay, I’m going to say something that I don’t think that Yucca Mountain is doing, is that, for example, uncertainties should be taking into account as such from the formal statistical analyses. The uncertainties can go one way or another, and some of them will cancel each other. Okay, so I haven’t done the calculations to assess that. And, the methodological bias is something that obviously the--is well aware in terms of taking it into account.

KADAK: But, you could compare that with MCNP and other tools, just to see if two independent methods, you can come close?

MACHIELS: That’s one way, is to use different tools, yes.

KADAK: Okay.

MACHIELS: Especially like in benchmarks.

KADAK: Right. Let me just ask one more question. The CASMO analysis approach is used to predict criticality in reactors?

MACHIELS: Yes, CASMO simulated approach, yes.

KADAK: And, the accuracy with which they do that is very important--

MACHIELS: Very high.

KADAK: --very high. So, would you conclude based on that, that the CASMO analysis of a spent fuel disposal option could also be as high?

MACHIELS: Maybe. The reason is that CASMO is going to individually identify some fission products, which are very important from a reactor operation, as well as a number of operations which follows that. When you involve geological disposal, there would a number of fission products which are going to become more important, after a fairly long time. And, CASMO runs a lot of the things together, and with time, obviously, a tool like CASMO is getting better and better. And, from that point of view, the translation of the time to which this tool is applicable to projecting now how far it may go, you know, there is some uncertainty involved in that. So, that’s why I’m saying maybe.

KADAK: Okay. And, is it realistic to assume that the fuel even in your disposal analysis is actually at 80 degrees centigrade for 10,000 years?

MACHIELS: I kind of tried to shy away from that by saying that if you don’t have water in liquid form there’s no purpose in doing that calculation, the K effective will be extremely low. So, whatever the time constant is, which it takes for the water to break into the package, for the water to flow to dissolve the boron, which is going to take some time, and then for the water to essentially repair the cask and flood the whole thing, I think is when my problems start. And, so, that’s why I assume liquid water, and I mentioned we get some sensitivity calculations assuming that the temperature of the water is 50 degrees, or 75 degrees, that doesn’t make much of a difference.

KADAK: Okay. All right, any other questions? Yes, John?

WAGNER: Albert, just one question about the first column to the left with numbers. Try to reconcile the values, for example, let’s pick two, the actinide plus 16 fission products for disposal at .97 compared to the best estimate CASMO of disposal at .92. It’s a 5 percent difference in K, and I’m trying to understand what’s different between those two cases.

MACHIELS: Well, the best system that CASMO is taking into account more than the 16 fission products which are being used in the column that it takes, basically everything into account, and the correction of 5 percent is based on the previous slide, which shows basically the calculation of the K infinity of a given assembly at five years.

KADAK: I think it’s your diamonds, I mean, your triangles.

WAGNER: Yes, my guess--we can talk offline. But, 5 percent for those additional nuclides is more than I’m used to seeing.

KADAK: Okay, thank you very much, Albert. Very good. I commend the report to your reading. I take from Albert’s presentation that direct disposal is possible with some additional analysis on proposed closure criticality without worrying about taking credit for boron.

Okay, now we have Andrew Barto.

BARTO: Yes, thanks. And, as the name of the office implies, the Division of Spent Fuel Storage and Transportation licenses commercial dry cask storage under 10 CFR Part 72, and transportation of all radioactive material, including spent nuclear fuel, under 10 CFR Part 71. And, I’m going to talk mostly about transportation, but I’ll touch on dry storage a little bit as well.

Can we go to the next slide?

So, I’m going to skip through a little bit of the basic background with respect to burnup credit, and then I’m going to talk about criticality safety in general for spent fuel transportation packages, a little bit of how this has been done in the past, and then I’m going to talk about our current guidance for burnup credit in transportation. And, then, I’m going to hit on computer code validation for burnup credit. This is kind of a particular issue for burnup credit criticality analyses. And, then, at the end, I’m going to talk about some things we are talking about internally at NRC that we may do to expand our guidance for burnup credit.

Next slide?

And, real basic, burnup is, obviously, the amount of energy released per mass of initial uranium.

Go on to the next slide.

And, burnup credit is taking credit for the reduction of reactivity that occurs with burnup, essentially the reduction of P235 and a buildup of neutron absorbing fission products due to fission process, and the buildup of actinides, including the fissile plutonium isotopes.

Next slide.

And, the primary goal for burnup credit in transportation is to increase the capacity of casks, and eventually, the ability to transport the entire inventory of commercial spent fuel when you need to. But, you could also use burnup credit to lower the neutron absorber boron content, or potentially to increase the initial enrichment of the fuel.

Next slide?

This is just a representation, kind of an example of what you might be able to achieve in terms of capacity increases for spent fuel transportation casks. If you're not taking any burnup credit, you're assuming the fuel was fresh, you have to have a more spaced out basket that typically includes flux traps, which are two neutrons over panels separated by typically about an inch and a half, makes for every effective neutron absorption in fresh water environment.

If you take burnup credit, then there's the potential to not have those flux traps. You can simply have one absorber panel between assemblies and get a much tighter packed basket. And, we have seen this in dry storage where people have moved from 24 assembly canister to a 32, and they're essentially the same volume.

Next slide, please?

This is to illustrate the current situation with dry storage. There's 55 independent spent fuel storage installations in 33 states. We expect about a dozen in the next few years. And, there’s over a thousand storage casks already loaded. I think the last figure I saw was over 1100 actually. And, none of these have been loaded, at least to my knowledge, none of these have been loaded with the burnup credit assumption.

So, they’ve either been loaded in a low capacity cask, or under fresh water assumption, or they’ve been loaded in a high capacity cask, taking credit for the boron that’s I the pool during loading. So, eventually, all of this fuel is going to need to be shipped, ideally, to Yucca Mountain, but from our perspective, it’s going to be shipped somewhere eventually. So, we’re going to have to deal with the waste that’s at all these sites.

Next slide, please?

This is the basic regulations in Part 71 that govern criticality safety for all fissile material, and also spent fuel casks. And, this 71.55(b) is the one that causes the most trouble, I think, for our analyses. As we’ve said before, if you don’t have water in the cask, you don’t even really need to do the calculation. You know it’s going to be very low K effective. But, 71.55(b) requires, for transportation packages containing fissile material, that you have to assume water and leakage.

Next slide?

KADAK: Is that fully flooded and leakage?

BARTO: It’s whatever is most reactive, but for spent fuel, it’s fully flooded. And, then, 71.83 requires that essentially if you don’t know all the properties of the fissile material that you’re shipping, then you assume that they are the maximum extent, the extent that caused the maximum neutron multiplication. In the past, this has been interpreted into what our guidance for burnup credit in transportation used to be, which was don’t take burnup credit. But, about ten years ago, we started issuing some guidance on how to do burnup credit analyses for transportation. We can go to the next slide?

KADAK: So, does that mean like fresh fuel, you’re shipping fresh fuel?

BARTO: What’s that?

KADAK: Does that basically mean you should be doing the analysis as if it were fresh fuel?

BARTO: Yes. And, this illustrates what that calculation would look like. The beauty of the fresh fuel assumption is that your analysis is very simple. You have essentially measured fresh fuel characteristics. You have a very high degree of certainty on the enrichment and the dimensions of the fuel. You can develop biases and uncertainties for the criticality, based on UO2 critical experiments, of which there are plenty. And, then, the spent fuel loading becomes very easy. You essentially don’t have to pay attention to burnup or cooling time with respect to criticality. Obviously, you would still have to do that for heat transfer considerations and radiation shielding.

Next slide?

So, about ten years ago, as I said, we started issuing some guidance with respect to burnup credit in transportation. Revision 2 of Interim Staff Guidance 8 was published in 2002. It was actinide only based on the extent of the isotopic depletion and criticality code validation data that existed at the time. And, given that we were coming from an environment where we had a very large margin with this fresh fuel assumption, and we were moving in an arena where you're going to take credit for the actinides--and, I'll talk about how much credit that represents on the upcoming slide--but, we had decided to retain the fission products as an additional margin. In other words, you know the fission products are there, representing an additional margin, so let's keep that, for the time being at least, as an additional margin.

And, also, the last bullet there is something I'm going to talk about toward the end of the presentation. But, the guidance recommends a confirmatory burnup measurement prior to loading in order to prevent misload.

Go on to the next slide?

And, actinides can represent roughly 75 percent of the reduction in K effective, and those are the major actinides we typically talk about, the uranium, plutoniums. And, fission products represent roughly the remaining 25 percent. These are the six major fission products. There's a, again, we talked about Yucca Mountain using, I think it was a list of 15 fission products. So, there's another set that contributes to the reduction of K effective. But, you know, as you get out past that number of nuclides, you start to reach a point of somewhat diminishing return for the effort it takes to model those nuclides.

ARNOLD: U236 is--

BARTO: I think we've--well, we've certainly seen a couple--we've seen a number of burnup credit applications already, and I think some have taken credit for U236. It's not in the list. I think it's not as large of an absorber as U234. I could be wrong about that, and it may not have the validation data that these other isotopes have. But, I believe some have tried to take credit for it.

Okay, next slide?

I want to thank John for introducing the concept of the burnup credit loading curve. It makes this a little bit easier. This is more of a qualitative illustration of what a burnup credit loading curve would look like. We're often, you know, we give that statistic about actinides representing 75 percent of the reduction K effective, and that sounds like they’re getting the bulk of the credit. In reality, the situation for high capacity casks, the 32 assembly casks that I showed earlier, you essentially need more than that to be able to ship the highest percentage of the existing spent fuel population.

So, this is kind of similar to what John showed earlier, and, obviously, there’s some outliers, but this shading there kind of represents what the discharge PWR fuel population looks like with respect to burnup versus enrichment. And, the red line here represents what you might expect to get if you just take actinide only credit, and even though the fission product credit is only 25 percent of the reduction in K effective, but it has the potential to move this line, you know, the line doesn’t move far, but it essentially moves across the bulk of the fuel population.

DUQUETTE: Is that the same red line that John showed?

BARTO: Well, it’s the same in concept, but this is for a higher capacity cask. I believe what John did was for the TADs, which are 21 PWRs.

KADAK: I think John’s was the green one. John, is that right? More or less? Okay.

BARTO: Yes, I guess the curve you showed for the TAD canister takes fission products into account. But, yours ended up being down here because you had a lower capacity cask. You know, 32 assemblies versus 21, you know, it’s a more challenging calculation. It’s more challenging to get that many fuel assemblies in there with respect to criticality.

Go on to the next slide.

And, I always like to put this EPRI quote up here when I know Albert is going to be in the audience. But, you know, the guidance we issued for burnup credit at the time, and this is to kind of show we’re not really that far out on the limb here with respect to transportation. At the time, it was considered a reasonably good estimate of what you could expect to get with respect to burnup credit. And, really, internationally, that’s the case as well. There’s nobody that’s really that far beyond what we allow for burnup credit, if at all, and that includes European countries who ship spent fuel pretty routinely.

And, as I stated, we already have looked at a couple of burnup credit applications, one of which has been approved, and three others are under consideration. I would note that two of the ones that we’re looking at are for canisters that are already loaded and sitting on some storage pads around the country.

KADAK: Is that full burnup credit or just actinide?

BARTO: Well, the guidance recommends actinide only, and three of the four that we’ve looked at have essentially gone beyond our guidance.

KADAK: They went full?

BARTO: Right. Well, there’s a lot of question about what full means, but they’ve asked for some degree of fission product credit.

Next slide?

And, as I said earlier, that code validation is a particular concern for burnup credit. It’s a more complicated analysis. It’s essentially two parts. You have to do the isotopic completion analysis to determine what the fuel composition looks like, and then put that composition into the criticality code. So, it’s a two-step calculation, essentially a two-step validation process.

And, we have started off here with the assumption that applicants should follow the well established guidance for out of reactor criticality safety that’s reflected in the ANS and international standards. And, these standards require comparison of calculated versus experimental data in order to get bias and uncertainty. And you would use a radiochemical assay measurements of actual spent fuel samples to validate the depletion code, and critical experiments for the validation of the criticality code.

KADAK: Could I just interrupt you there for a second?

BARTO: Sure.

KADAK: What do you hope to prove with this radiochemical assay that you don’t know now when you reload fuel, especially in the short-term?

BARTO: What you’re hoping to prove is that the depletion code gives you a good answer for what the fuel composition is.

KADAK: But, isn’t that proven every time you reload?

BARTO: I believe it’s proven for reactors, that they work well. But, I think it’s a little bit of a different scenario. You’re looking at a somewhat different set of nuclides when you get out to five years, and beyond, than you are for--

KADAK: But, you’re talking just transportation; right?

BARTO: Right.

KADAK: So, maybe 30 years. Is there something changing so fast in that spent fuel over 30 years that would invalidate your fundamental understanding of physics?

BARTO: No. No. But, I think, you know, like you say, we have a lot of experience with reactors, and we load them well and they operate well. But, I think in reactor operation, you’re focusing on a particular set of nuclides, and it’s a somewhat different set. So, these nuclides that you’re trying to take credit for in transportation aren’t necessarily as well characterized as they have been for, you know, the different set of nuclides has been characterized for reactor operation.

For example, the fission products we’re trying to take credit for are long-lived, while they’re essentially stable, if you want to credit that, and they are not the isotopes of concern when you’re reloading reactors. They’re pretty insignificant in reactor operation compared to the actinides and the short-lived fission products that are required when you’re reloading the core.

KADAK: Okay.

BARTO: Okay? Next slide?

And, this in comparison with the fresh fuel analysis, this is a flow chart that shows the additional steps that would be required for burnup credit analysis, and the left half there are the depletion analysis where you’d feed the fresh fuel characteristics and the radiation parameters into a depletion code. You would obtain the bias and uncertainty associated with that depletion analysis from comparison to chemical assay measurements.

And, on the right side, you would have a validation of criticality code, ideally against critical experiments that contain the nuclides that you’re attempting to credit. But, also, you could use a MOX critical experiments and UO2 critical experiments to obtain the bias and uncertainty for the criticality code.

And, then, you would perform the criticality analysis generally of loading curve for the spent fuel loading. And, then, there’s the additional step that, again, I’ll talk more about later, but current ISG recommends a burnup verification measurement prior to loading.

Go on to the next slide.

So, we have been doing some work to expand the technical basis for burnup credit. We’ve had actinide only up to this point. The question is we’ve had some new data and new experiments come to light in the past eight years, or so, so what can we do to try and get some credit for fission products into our guidance.

Back on the previous slide, I briefly talked about a criticality code validation involving critical experiments that have the nuclides that they’re trying to credit. There’s not a lot of those critical experiments about. The French had done some experiments back in the Nineties, with major actinides. And, DOE actually purchased the rights to that data, and had Oak Ridge do an analysis of that data for us, and determined that those critical experiments are very applicable to spent fuel casks.

The experiments were designed to look like spent fuel. They have kind of the right ratio of plutonium isotopes, and the right ratio of plutonium and uranium. And, that data is available to applicants. It’s not publicly available, but if they sign a non-disclosure agreement, they can use that data to support the burnup credit analysis.

We also published a NUREG. We had some sensitivity uncertainty analyses for the commercial reactor critical configurations done, with the idea being that, you know, a reactor core kind of looks like a spent fuel cask, so maybe these configurations are applicable for validation of spent fuel cask analyses.

We’re also looking at some higher burnup radiochemical assay data that may become available soon for depletion code validation. And, of particular interest, there’s a lot more fission product radiochemical assay measurements among this data, which has kind of been the hangup with the radiochemical assays. The older measurements have tended to focus on the actinides, and not so much on the fission products.

And, the French also did a series of critical experiments involving the six major fission products, which is also non-public, and which we have also not purchased, and we are kind of in negotiation right now with the French entities that own that data to try and at least negotiate the use of it, if not free release of it.

Go on to the next slide.

So, moving forward with our guidance, pretty much everything that I talked about on the last slide, we’re trying to use as an input to our potential revision to ISG-8. With respect to code validation, we have this French HTC actinide data, which we feel is very applicable to spent nuclear fuel, whereas before, all we had were the UO2 and MOX experiments. It wasn’t as good a feeling as to how well those were applicable to spent fuel given that the MOX, you didn’t really have the right ratio of uranium and plutonium, and obviously, the UO2 experiments don’t have any spent fuel compositions.

But, now that we have this additional data, we’re trying to look for ways that we can include the fission products, even though we don’t have that same kind of validation for them. And, then, again, all the issues I talked about on the last slide are going to be inputs to our potential revision of this guidance, as far as informing how much fission product credit we can grant.

Go on to the next slide.

Another big issue we’re looking at right now are these confirmatory burnup measurements. In the industry, we’ve interacted with them a number of times on this. This seems to be a real problem for loading a cask under a burnup credit assumption. The measurement systems that exist now require you to move the fuel in order to do these measurements, and, any time you move fuel, there’s additional risks. There’s additional dose to personnel in doing a measurement campaign. So, we’ve kind of stepped back and tried to look at it, you know, what are we trying to prevent and what are some alternatives to this measurement that we can do.

So, really, what we’re trying to prevent is a misloading of an assembly, not just any misload, but one that would have a significant reactivity effect. And, that would be loading an assembly that is severely underburned, or multiple assemblies that are underburned. So, with that in mind, we had a study done by Oak Ridge and, you know, what would be the consequences of a misload. And, worst case scenario, loading anywhere from one to four assemblies right in the middle of that high capacity cask, what are the reactivity effects that you would expect to see?

We also have a study ongoing about information related to spent fuel burnup confirmation. How are the reactor records generated? And, how good are these out of reactor measurements in confirming that value? We’re also having our Office of Research look at what really is misload probability, you know, the probability of having any misload versus the probability of having one that could affect reactivity significantly. So, that’s some ongoing work.

So, all these issues feed into some internal discussion we’re having right now about maybe the way to address the potential for misload is to have some sort of misload analysis as an option instead of this confirmatory measurement.

Go on to the next slide.

So, that's pretty much it. You know, in summary, there's more and more effort to get burnup credit approved for transportation. The analyses associated with it are much more complicated, but we're working to expand the technical basis for burnup credit so that transportation package applicants can essentially get the maximum amount of burnup credit that is supported by the data.

And, again, the goal is to increase the fraction of the discharged fuel population that can be transported in the high capacity transportation packages. And, that's pretty much it.

KADAK: Okay, thank you very much. Questions? David?

DUQUETTE: Duquette, Board.

This analysis you just presented is primarily for transportation, I understand?

BARTO: Yes.

DUQUETTE: Transportation casks, and so on and so forth. Is your office also doing the same thing for postclosure disposal?

BARTO: Not my office.

DUQUETTE: The NRC is?

BARTO: The NRC is. We're another office within Nuclear Material Safety and Safeguards, and High-Level Waste would be the office that's looking at postclosure criticality analysis.

DUQUETTE: Okay. Do you coordinate with them on a regular basis?

BARTO: We have. I mean, I coordinated with them on some of the preclosure criticality activities, but I haven’t been involved, since the application came in, I haven’t seen it, I participated in a couple of technical exchanges before that application came in.

DUQUETTE: And, your analysis primarily goes to high capacity casks. If they go to, say, 90 percent TADs, does that change anything in your analysis? Have you included TADs?

BARTO: It changes the need for burnup credit. Some of the early interactions we have had with the applicants that will potentially be submitting TAD applications is that they don’t really plan on needing that much burnup credit.

DUQUETTE: Okay, thank you.

BARTO: If they can get away with either no burnup credit or actinide only.

DUQUETTE: Yes, that what I would have thought.

KADAK: John?

GARRICK: Yes. I was very interested in what you’re doing to expand your technical basis for the guidance and allowance for burnup credit. But, I also noticed that most of what you’re doing is basically paper studies or purchasing it from somebody else, like the French.

BARTO: Pretty much.

GARRICK: There's some very interesting issues here that are very amenable to applied research, and there was a time when NRC had a very active and very effective R&D program. Is any of the R&D effort that either goes on at the Center or with the Research Group being directed towards this problem beyond kind of the paper level? The reason is is it's very amenable to it, and one of the reasons that the confidence is higher with respect to reactors is because the configuration and the conditions that exist are much more quantitatively understood.

But, with the assay methods and with the accountability methods that exist, and the kind of monitors that you could emplace in the handling operations, it would seem that you could really reduce the uncertainties here quite tremendously. And, I just am curious as to whether there's an effort beyond what you've just discussed here.

BARTO: Do you mean with respect to obtaining the data domestically that we're purchasing from overseas?

GARRICK: Right. Yes.

BARTO: There has been some discussion of that. Unfortunately, I think the current funding climate for such activities is limited.

GARRICK: Yes.

BARTO: When you talk about doing any kinds of critical experiments, they’re very difficult to do, especially when you start adding plutonium nuclides or some of these fission products.

As far as the chemical assay data, I think that seems to trickle in. I’m not sure how much of that is being done domestically, but we’ve, I know, certainly Oak Ridge has been involved in a couple of international studies. When I referenced the high burnup radiochemical assay data, most of that was obtained overseas, but it’s through programs that we have kind of a back end involvement in through our Office of Research and through Oak Ridge National Lab.

GARRICK: Okay.

KADAK: Okay. Ali?

MOSLEH: Yes, Mosleh, Board.

When you--the measurement, the work that you’ve asked the Office of Research to do regarding misload, is that basically a human reliability factor?

BARTO: Well, it’s pretty early in the--it’s kind of in the early stages of doing this right now. So, the scope is a little bit undefined. But, I don’t think we’re going to look that much at human reliability. I think the way we’re going to approach that is to look at, you know, a number of things, basically operating procedures at plants for fuel movement. We can look at some historical incidents of misloads.

And, another thing that we might e able to look at is the distribution of fuel, in that, you know, the fuel population, looking at, you know, if you can get a probability for any kind of misload, then you can kind of use that fuel population to determine all right, if I have a misload, what kind of misload is it going to be. If I were to pick an assembly at random, what are the odds that I'd pick one of those outliers as opposed to, you know, the bulk of the fuel population is up in that higher burnup range.

I mean, that's all very, again, very early in development, and that's just one of the things we're talking about. And, we just haven't really talked too much about human reliability yet. But, I think essentially the whole misload issue, I think, boils down to human reliability at its most basic. But, we can make some determination now that, you know, misloads will happen at some rate.

MOSLEH: But, that's also something that we do have some data on.

BARTO: Yes.

KADAK: Okay. Howard?

ARNOLD: I have a related question on the burnup measurement. But, first, let me ask another one. How do you visualize that being done, the burnup validation?

BARTO: You mean the confirmatory burnup measurement?

ARNOLD: Yes.

BARTO: That's funny, because I was having a discussion earlier about this. I think the way we envisioned it when we put the recommendation in there is that it would be an in-line measurement, i.e. you would pick up an assembly for loading, take it to a measurement device, confirm that burnup, and then put it in the cask.

Unfortunately, our guidance doesn’t really give too many specifics on that, so I think if you were to look at the way these measurement campaigns have been done in the past, it’s you bring the equipment into the pool, measure however many assemblies you want to measure, and put them right back where they were, take the equipment out, and then do the loading at some time later. So, you kind of still have the potential for misloading if you do that measurement.

ARNOLD: Yes, that was really where I was--you’re basically questioning the integrity of your ability to track that assembly back through its entire history. I mean, if you know its history, you know its burnup.

BARTO: Right.

ARNOLD: And, so, you’re assuming that somewhere along the line, you’ve screwed up in documenting its history?

BARTO: Correct.

ARNOLD: And, so, I guess I would first want to question whether there’s a way to improve your confidence in that history.

BARTO: Yes, I think that would have to be part of anything we would do with respect to having an alternative to this measurement, would be, and I don’t know exactly what it would be, but some kind of additional QA of the fuel that you’re going to load. It’s hard to say what that would be. I think there’s already pretty well established QA procedures for fuel movement and fuel loading in general. But, you know, we have records of some number of misloads.

ARNOLD: The fuel that’s already in the multi-purpose canisters, are you willing to trust it?

BARTO: I think that’s where we would specifically need some sort of misload analysis, because you can’t measure it, number one, without unloading it, and if it’s a welded canister, you destroy the canister. Yes, and, again, we have a couple applications in right now for canisters that are loaded. And, that’s one of the things we’re talking about for those particular designs, and I think whatever we decide upon for those particular designs would probably end up in some fashion in our guidance.

ARNOLD: Okay, thank you.

KADAK: Thank you. Just two quickies.

One, how far are you along in terms of confirming that the tools used by the utility to monitor fuel burnup for reload analysis, as an example, and their ability to maintain those records reliably enough for loading is good? Where are you on that?

BARTO: Oh, you know, I don’t think there’s much of a doubt that that is good.

KADAK: That’s okay? Okay.

BARTO: I think the issue is, you know, we have looked at assembly handling incidents in pools. We’ve got a couple incidents now of misloaded assemblies being loaded into--

KADAK: Yes, I’m off the misloading part, just being able to know that this fuel assembly with this number on it has this burnup history based on our core follow analysis.

BARTO: Oh, no, I think we feel pretty good about that.

KADAK: Let me ask you a question related to transportation.

BARTO: Sure.

KADAK: As you know, there’s, oh, gosh, I used to have a figure, maybe Rod McCallum can refresh me, say a couple thousand canisters out there already, some storage only, some multi-purpose.

BARTO: There’s over 1100. I mean, there’s a number of those that are already dual purpose.

KADAK: Right.

BARTO: That could be shipped. And, those are primarily all the low capacity casks.

KADAK: Now, for those storage only canisters, what are you planning to do to get those able to be shipped?

BARTO: We’ll kind of have to approach that as we receive applications for that, essentially. And, I think, you know, what we're doing right now for the couple of designs that we're looking at that are already loaded, it will be sort of precedence setting in that respect.

KADAK: But, there is a possibility that they don't have to reload, and those canisters can be shipped directly, you think?

BARTO: I think there's a good possibility of that. You know, in that loading curve that I was showing for high capacity casks, that's a, you know, a generalized loading curve. Again, I think we talked about this a little bit earlier, but that's not taking credit for assemblies that have higher burnups in that curve. And, in theory, you could generate some sort of regionalized or preferential loading scheme where you could get a--well, I guess that doesn't help you with the already loaded canisters.

But, you know, one of the already loaded canister applications that we're looking at is for a finite number of casks at a particular site, where they have looked at individual burnups for the assemblies, and, in each individual canister.

KADAK: And, done the analysis.

BARTO: And, done the analysis.

KADAK: Thank you.

I guess we're going to have to break it off here.

Thank you very much, Mr. Chairman. When would you like to reconvene?

GARRICK: Let’s see, it says here--let’s just take a 15 minute break. Okay? That means we come back at 10 past.

(Whereupon, a brief recess was taken.)

ARNOLD: Our next and last talk on the formal agenda, aside from the public comments, concerns the waste package closure, welding prototype testing.

I want to add a little context to this. In our last public meeting of this Board, we had a discussion on the surface facilities at Yucca, and we were concerned that the, among other things, that the welding stations might be a potential bottleneck. We have, as a Board and Board Staff, visited a couple times the prototyping at Idaho that Mr. White is going to discuss. So, I’m very interested in getting the latest on this.

Mr. White has been with DOE since 1990, and on this project I think since 2005, and they have been doing good work at Idaho and I want to hear about it.

WHITE: Thank you very much. Good afternoon, everyone.

This presentation is going to take a look at the waste package closure system prototype that’s being developed at the Idaho National Laboratory in Idaho Falls. At the end of this presentation, I’m going to show a three minute video, where you will be able to see the computer work stations and actual welding being performed. After that, we’ll have questions.

This slide shows an overall view of the equipment at the Idaho National Laboratory, which will be discussed in more detail as we progress through this presentation. Obviously, it looks quite a bit different than you saw it 18 months ago.

Just to get people familiar with what we’re talking about, this is the waste package configuration. The waste package consists of an Alloy 22 outer corrosion barrier, and a 316 stainless steel inner vessel. It is a two-piece waste package. And, the waste will be in canisters that go inside the waste package.

Next, please?

This enlarged view shows that the purge port cap is associated with the inner lid. And, the space between the waste container inner vessel is evacuated and backfilled with helium, and that’s why I’m showing you this, so you understand where the evacuation and backfill is being performed.

And, this slide shows the waste package closure and seal welds. The closure weld lid at the top is approximately a half an inch, and at the bottom between the end to the outer lid, and beginning of the radial bend is approximately a tenth of an inch. We’ll get into more of that detail. A few slides from now, you’ll see a blown-up portion of that.

You can see the cross-section of the outer corrosion barrier and the inner vessel, and the light red is the upper sleeve. The dark red is the outer corrosion barrier and outer lid. The lighter blue is the inner vessel and inner lid. The darker blue is a spread ring. And, the purple is the purge port plug.

Next, please?

In this enlarged view, you can see the spread ring seal welds. The spread ring, when installed, secures the inner lid, as you can see in this picture. The spread ring steel weld ensures that the spread ring is held in place and provides a seal so that the helium gas remains between the inner vessel and the waste container, or containers, depending on the waste package.

Next?

In this enlarged view, you can see the purge port plug, which goes in the inner lid. This is the opening through which the evacuation and backfill with helium is performed.

Next?

And, this view shows the configuration of the outer lid groove weld, which is a full thickness groove weld. And, this is the area where controlled plasticity burnishing is performed after the closure weld has been completed. And, the upper sleeve fabrication weld is performed in the shop during fabrication of the waste package.

Next?

The systems included in the waste package closure are the welding the inner lid, the spread ring, purge port cap and outer lid. Performing nondestructive examination of seal welds and the closure weld. Evacuating and backfilling with helium. And, stress mitigation is performed by controlled plasticity burnishing, and that’s designed to induce a layer of compressive stress in the completed weld area. Material handling is performed by remote handling system, and the two robots.

This is a simplified process flow for the waste package closure system. The estimated time to complete the closure weld process is approximately 37 hours from beginning to end. And, you have to understand that’s an estimate. We haven’t done it yet.

After the waste package moves into position, we insert the spread ring, weld it to the inner lid and waste package with a two pass seal weld, and then we do a visual inspection.

And, then, the spread ring leak test tool, and purge port tool are set in place, and evacuation and backfill of the inner vessel is performed. The leak test of the spread ring seal weld is performed. And, another evacuation and backfill of the inner vessel is performed. And, the purge port plug is tightened and leak tested. And, then, the purge port plug is seal welded.

Then, the outer lid is placed, and the closure weld is completed, which includes visual inspections after each weld pass. There will also be a pass by pass ultrasonic examination performed. Then, after a final weld pass is done, we’ll do a visual ultrasonic and eddy current inspection. And, then, once that is done, it’s time for the controlled plasticity burnishing to be performed. And, then, a final visual ultrasonic, eddy current inspections are performed at that time.

In this slide, you can see the two robots. It also shows the tool tray with source end effectors and other tools needed for the closure process. The grounding ring is attached to the lifting ring in the middle of the lids. It’s used on both the inner lid and the outer lid. And, the two robots move around on a rotating bearing, and they are 180 degrees apart to reduce distortion and increase throughput.

This slide shows the ultrasonic and eddy current inspection end effector. The loading end effector is mounted on a robot in this view. It includes the laser based visual inspection system, as well as a loading torch, weld wire guide and cameras to view the leading and trailing edges of the weld pool.

The dressing end effector includes a wire brush that is used after each weld pass to clean up the weld. It also includes a grinder that can be used to grind out any flaws that may occur, and can switch between the brush and grinder remotely.

In this slide, you see a telescoping mast of the remote handling system. The remote handling system can place objects within plus or minus a 16th of an inch. Also shown is a transfer cart for moving materials into and out of the room.

We use a gas tungsten arc welding process, which is a very well developed process used in the industry. And, we also show the spread ring which is seal welded, and purge port cap, which is seal welded. And, we discussed those earlier as well.

For our visual inspection, we have a laser based visual inspection. The equipment, it’s a red box mounted on the welding end effector, and it identifies surface flaws. And, this visual inspection is performed after each weld pass, and after controlled plasticity burnishing has been completed. It also performed an important function in being able to track the weld ports around the weld path.

This slide shows the purge port tool and the purge port plug. The purge port tool loosens the plug in the center of the inner lid. Then, we do the evacuation and backfill operation. And, then, we tighten the plug, and the opening is sealed with a crushable metal gasket. Then, the metal seal is then tested for helium leakage.

And, this is a view of the evacuation and helium backfill skid. We show the helium mass spectrometer, vacuum pump, control panels, chiller side of the skid. What you don’t see is that there’s a helium analyzer located between the helium mass spectrometer.

Next, please?

This is the spread ring leak test tool. And, in the blown-up portion, which I have labeled as vacuum area for spread ring leak test, I’m going to explain what some of those colors are. The blue is the inner lid. The red is the spread ring. The gray is the inner vessel. And, the white piece over the gray and blue area is the spread ring leak test tool. You will notice that there’s a couple of small rectangles in the white portion, and those are gaskets to provide a tight seal for the helium leak test.

And, the mass spectrometer on the evacuation and helium backfill skid detects the presence of helium in the vacuum area if it exists.

We use the same process for the closure weld as we did for the seal welds, gas tungsten arc welding. The inner lid weld is a full thickness groove weld, one inch thick, and it’s going to take approximately eight to ten passes to complete the closure weld.

This slide shows the dressing end effector and the tools that it uses. The wire brush is used to clean the weld surface after each pass. And, you can also see a grinding wheel that can be used to remove any flaw that needs to be repaired. And, the tools can be stored in the tool tray drawer.

This slide shows the ultrasonic top and side vessel probes, and the eddy current probe on this end effector. The ultrasonic inspection identifies flaws and is a full volumetric inspection. The eddy current probe identifies surface breaking flaws, and covers a large surface area, which helps increase inspection rates.

And, so, this is at the end of the process for stress mitigation. We do controlled plasticity burnishing in order to reduce residual tensile stresses in the outer lid weld.

This slide shows control and data management systems that are being used at the Idaho National Laboratory. The control room has six work stations with multiple views for a fully automated and remote system. One of the work stations, there’s one work station for each welder, that’s two; one for each inspector, that’s two more; and one for backfilling, leak detection, material handling; and, then, one for a supervisor who controls overall operations.

In the video, you’re going to see these work stations being used by Idaho National Laboratory personnel.

This slide shows other tools and systems that are part of the waste package closure system being developed at Idaho National Laboratory. From the upper left, going clockwise, you can see the bumpy bar code identification tool; the spread ring insertion tool; the lid handling tool and lid lifting ring, which will be seen on the upcoming video; the interior of the control cabinet; the utility control cabinet; and the cable management, which you will also see on the video. It moves around with the robot so the hoses and cables do not get tangled. And, then, there’s a machine vision system which determines locations of tools and lids, so it can be picked up remotely.

And, in March of 2009, in a couple months, hopefully, we will have a full demonstration of the waste package closure system prototype at Idaho. The demonstration is going to include all steps of the closure process, which include completion of seal and closure welds using two robots, use of a mock-up waste package that is whole diameter with a shortened height. And, we’re going to apply heat to simulate the heat of an 18 KW transportation, aging and disposal canister waste package. And, that’s going to help us evaluate the time it takes to meet inner pass temperature requirements for welding.

Non destructive examinations and controlled plasticity burnishing will also be performed, and we will demonstrate the ability to remotely repair defects. And, as I mentioned before, the length of time to complete the closure process is estimated to be approximately 37 hours, and we will be keeping close track of the time it takes to perform these steps, so we can see just how close we are in the estimate.

Now, for the next slide, we’re going to show a three minute video that I mentioned at the beginning. And, you’re going to see a remote handling system and lifting mechanism and how it twists to lock in place. Then, you will see the equipment used to evacuate backfill and perform leak tests. Then, you will see how the two robots move around the waste package on the rotating bearing, and how the cable management system moves around as well.

And, last, the video will show control room, operator work stations, and you will have a video of the actual welding being performed.

(Whereupon, the following video was played.)

The waste package closure system prototype is being installed and tested at the Idaho National Laboratory. Equipment and lids are moved within the closure room by the remote handling system, or RHS. Tools are attached to the base of the RHS with the tool interface plate.

Shown here is the lid lifting tool attached to the RHS, moving to a location over the mock-up inner lid. The tool used to insert the spread ring into a groove in the inner vessel is sitting on the lid.

The lifting tool approaches the lifting ring on the lid. To move the lid, the lifting ring would attach to the ring. When not in use, the RHS is staged in the home position.

The inner vessel is evacuated and backfilled with helium followed by a leak detection of the inner lid welds. Shown here are the tools used to perform the inerting and leak detection. The tools are connected via umbilicals to the skid, which hold the system support equipment.

The waste package closure operations of welding, grinding and nondestructive examination are performed by two robots mounted 180 degrees apart on a rotating bearing. The end effectors for each of these operations are staged on a tool tray mounted behind each robot. A grounding fixture for welding is placed over the lifting ring in the center of the lid. The motion shown here is much faster than when welding or inspecting is in progress.

The process control room has six work stations for controlling the closure operations. Up to six cameras or data views are available at each work station. These monitors show a trailing and leading arc viewing camera for each robot, a bearing and robot overview, and a data screen.

A camera view from the bearing shows movement of the robot and end effector. A trailing arc viewing camera shows the weld pool and tungsten torch from welding the groove joint of the outer corrosion barrier.

Successful automated welds have been completed from this control room.

(Whereupon, the video was concluded.)

Now, it’s time for questions.

ARNOLD: Okay, I have a couple, and then maybe some of the other Board members will, too.

I remember a discussion on our earlier visit of whether that inerting was even necessary, and there was, I think somebody said that there was a study going on as to whether it was needed. Could you tell me why we have to inert the ends?

WHITE: Yes. You could just do the vacuum drying, but the inerting is important for heat dissipation from the waste container out through the waste package.

ARNOLD: The helium is a heat transfer?

WHITE: Yes, it helps transfer the heat out of there, and that’s one of the major reasons we kept it.

ARNOLD: I see. Okay. Another question. Experience with welding of large metal objects is that they sometimes distort, get bent out of shape. And, this is being prototyped just with the lids.

When do you think you will have a complete inner and outer shell and be able to do the whole thing together and see if in fact it’s feasible in that form?

WHITE: Well, we believe we’re going to do that in March. We do have a shortened height. We had a gentleman, an engineer from our contractor, do calculations to verify that this would be adequate to properly demonstrate that the closer lid process is--

ARNOLD: But, you’re not doing a full--

WHITE: It’s not the full length.

ARNOLD: Yes, a full waste package.

WHITE: It’s shorter than full length.

ARNOLD: Yes.

WHITE: And, one of the main reasons for that was the practicality of having to dig a 20 foot hole in the floor at the Idaho National Laboratory facility.

ARNOLD: And, you’re convinced that you will be okay with a short one?

WHITE: Yes, I am. I have looked into it and discussed it in some detail with BSC and Dell Mecomb with BSC. And, that’s one of the reasons we argue in the 18 KW load for heat, because it is important that we demonstrate that we meet these inner pass temperature requirements that we have.

ARNOLD: Okay. Other questions? David?

DUQUETTE: Duquette, Board.

A number of questions that come up on the way this would actually operate in practice. The TADs are supposed to be filled with material at the sites. Would you then see moving this robotic arrangement to the reactor sites for loading the TADs and sealing the TADs at the sites?

WHITE: No. That’s a separate process. We also will have a separate TAD closure process at our site, at the wet handling facility. That is an entirely different process than this process, which is for waste packages. The TAD goes in the waste package.

DUQUETTE: I understand that. But, as I understand what you demonstrated here, it’s both a TAD and an outer waste package; is that correct?

WHITE: No. We’ve done the inner vessel and the outer corrosion barrier, which are two parts of the waste package.

DUQUETTE: So, the TAD would fit inside of both of those?

WHITE: Yes.

DUQUETTE: Okay. I had a question on this spread ring.

WHITE: Yes.

DUQUETTE: Is that simply a plastic deformation or are you simply expanding the ring out so that it conforms into the slot that surrounds it? Is it a split ring? How does it operate? Maybe go to Slide Number 4?

WHITE: You can go back to 2. It shows the spread ring already spread out in that view.

DUQUETTE: Okay.

WHITE: It comes--it’s a one piece spread ring. Go to the previous slide, please. You see the spread ring, and you might think that that’s already that way. It does not come that way. What it is is you have to push it out with a spread ring tool.

DUQUETTE: That’s what I meant.

WHITE: Which is shown in another slide.

DUQUETTE: You plastically deform it into place?

WHITE: No.

DUQUETTE: You don’t?

WHITE: We’re just pushing it into place. There’s no plastic deformation involved in that. It’s spring fit, and then you hold it in place, and you perform your seal welds, which helps hold it in place. And, that secures the inner lid in place.

DUQUETTE: To drop it past that wall that I see at the top of the figure, means it has to have a smaller diameter than that wall, and it drops down and becomes a larger diameter as it fits into that slot.

WHITE: I think the problem is that it’s kind of confusing because it’s not one piece like it looks like in that picture. It’s actually, I don’t know, I’ll try and do this so you can see it. Like this, and then you spread it out, and it fits in place. And, there’s a joint where it fits. And, so, it’s hard to explain without the video of it.

DUQUETTE: So, it’s not a split ring? It’s all one piece?

WHITE: It’s one piece. It has to be sprung into place. At one time, there were three pieces, and we went to a one piece.

DUQUETTE: So, you, when it’s in its retracted position before it gets put into place, what’s holding it in that retracted position? In other words, it’s got a smaller diameter. How does it retain that smaller diameter, and then expand out into the larger diameter?

WHITE: Well, there’s actually grooves in the side of the spread ring, and if you go to the--let’s see which slide will show it--Slide 20. There, we show the spread ring insertion tool.

DUQUETTE: Yes.

WHITE: And, you can’t really see it that well, because I didn’t include a picture of it operating. But, it will actually push out, and it pushes that spread ring out into that opening, and it actually fits very securely to itself. And, then, you seal weld it, and that’s what keeps it in place.

DUQUETTE: I understand that. I’m just trying to figure out how you get it onto the surface and then spread it out. I'm curious about what shape it is when it drops into place.

WHITE: I think what I need to do is try and get a video, or something like that to you, so you can visualize it, because it's really tough to explain. Colleen, do you want to explain it better?

DAVIS: Colleen Shelton Davis from Idaho National Laboratory. The spread ring is fabricated in this fashion. So, it sits on that lid like this, and then the tool pushes it into place.

DUQUETTE: So, it is split.

DAVIS: It's one piece, but it's like a--

DUQUETTE: But, it's split. Okay. That's the point that I was trying to make. It basically overlaps itself and you would expand it out.

DAVIS: Yes.

DUQUETTE: Okay. And, so, when it's in place, the two ends just about butt?

WHITE: Yes.

DUQUETTE: And, you go ahead and do your seal weld around it.

WHITE: Exactly.

DUQUETTE: You don't weld the butt?

WHITE: I'm sorry I didn't explain it better.

DUQUETTE: Okay. I understand. So, this whole operation would be done at the mountain after delivery of the TADs, it would slide inside the waste package; correct?

WHITE: Yes. The TAD would be in the waste package, which would be in the waste package transfer trolley.

DUQUETTE: The waste package is also amenable to other kinds of repackaging at the site if TADs are not 100 percent used?

WHITE: For TADs not being 100 percent used, talking about the 90 percent breakdown?

DUQUETTE: Well, there’s some concept that not all of the fuel will be delivered to the mountain in TADs, that some of it may have to be--

WHITE: Right. Then, that fuel would go to the wet handling facility.

DUQUETTE: Okay.

WHITE: And, then, we would handle it in our pool, and it would be put into a TAD at the site in the pool.

DUQUETTE: So, it would be the same configuration TAD.

WHITE: Yes.

DUQUETTE: So, it’s no longer a TAD. It’s just an AD?

WHITE: Yes. I guess you could say that.

DUQUETTE: Okay, thank you. I think that answers most of my questions.

ARNOLD: Ron?

LATANISION: I have two questions. Latanision, Board.

I did miss part of your comments, Chris, so you may have talked about this, but I would like to get a sense of the answers. With these prototype welds, is the plan to take sections of the weld after you've produced it in the prototype to examine for defects and porosity? And, I don't just mean surface breaking defects, I mean porosity and other defects that may be present.

WHITE: We'll be doing all the non-destructive examinations that we would normally do.

LATANISION: Why not destructive?

WHITE: And, so, we will be able to detect any defects as we're doing the prototype process in March.

LATANISION: Do you think you're going to find all of the defects that might be present in a weld with non-destructive testing?

WHITE: Yes.

LATANSION: Well, all right, maybe you're more confident than I am, but you don't have any plans to section any of these welds to look at the metallurgical microstructure in the prototyping phase of the development of this weld?

WHITE: We may, but I can't say for certain.

LATANISION: Do you have plans to look at the distribution of residual stresses?

WHITE: Again, that could be done in the future. I'm not certain right now. It depends on a number of things, including funding.

LATANISION: I'm interested in the broad issue of the assembly of this package. And, my recollection is that one of the early steps is to quench the package from high temperature. We, I think, saw one illustration--

WHITE: That's the annealing process.

LATANISION: Right. We saw one illustration of that, and as I remember, there was some pretty significant distortion. How are we going to accommodate that prior to welding? Do we have a solution in terms of the assembly process? Carl, you may know more, Carl, about this--

WHITE: You're talking about the annealing that was performed back in July of 2006?

LATANISION: Yes.

WHITE: In New Jersey?

LATANISION: Yes, that's right. I wasn't present, but I know some of the Board members were.

WHITE: Well, I was there that night.

DI BELLA: Put up Slide 3.

WHITE: Okay.

DI BELLA: What Ron is asking a question about is the process that makes the 3/16 stainless steel inner vessel and the Alloy 22 outer corrosion barrier, which is not part of what is being done at INL. The only part being done at INL is attaching those lids and seal ring and evacuating and testing to the already completed in another part of the country, 3/16 stainless steel inner vessel and Alloy 22 outer corrosion barrier.

LATANISION: That is the question, but I’m wondering what sort of integration there is to bring forward a complete package that isn’t distorted, from which we know something about the distribution of residual stresses, et cetera. I mean, maybe it’s a broader question than Chris is prepared to address.

WHITE: Well, actually, I can address it.

LATANISION: But, I’d like to hear about that at some point.

WHITE: We do have a waste package prototype program. The waste package that was performed back in 2006 was a first. There are plans to do five additional. And we learned lessons from the first one from the annealing process. And, as you noted, there were some distortions. And, so, some of the things that we did, determined that we needed to do, was to pre-anneal the sleeves. And, we did do that on the mock-up waste package that just got annealed this last weekend, and it did not appear to be distorted much at all. I don’t have results from it because they’re doing measurements today. So, I get those later.

LATANISION: Could I suggest that we include some discussion of this on our agenda for the next meeting? By that point, we will have more information. This is something that’s of great interest from the point of view of the--

WHITE: Oh, yes, definitely.

ARNOLD: Yes, the broader topic of the prototyping of the whole assembly.

WHITE: Yes.

ARNOLD: That was what my earlier question was addressed to.

WHITE: And, while I was the waste package engineer for a while, we do have a new waste package engineer, Michael Plenski.

ARNOLD: Bill Murphy?

MURPHY: Bill Murphy, Board.

What is the chemical composition of the weld metal?

WHITE: It’s a nickel alloy, and there are some four key areas of interest in determining the weld metals. Oh, I’m sorry, this is going off the wrong direction.

The weld metal is Alloy 22 for the outer corrosion barrier in the outer lid. And, it’s 3/16 stainless steel for the inner vessel welds.

MURPHY: Thank you.

ARNOLD: David?

DUQUETTE: I sort of had the same question. What you’ve shown us, I think, is the welding operations on the inner vessel with the inner lid. Have you also prototyped the welding of the outer vessel, the corrosion resistant vessel, with that Alloy 22 outer lid? There are two welds that go into that operation that I can see in your diagram on Figure 4.

WHITE: Yes.

DUQUETTE: Page 4. Your machine will do both of those in the same machine, that is, the red welds is what I'm talking about up there in the left-hand corner.

WHITE: Yes.

DUQUETTE: What you've shown us today, I think, is just the spread ring welds with the seal welds of the spread rings, along with the purge port plug. But, I don't think you talked about the welding of the outer vessel in this talk, did you?

WHITE: Yes, I did.

DUQUETTE: You did? Was that operation shown, or was it just the inner steel welds that were shown?

WHITE: It was a slide shown.

DUQUETTE: There was a slide showing.

WHITE: Yes.

DUQUETTE: Okay. So, you're doing both, and you're doing them--is the concept that you would weld up the inner vessel first, and then slide it into the outer vessel and then weld the outer vessel?

WHITE: No.

DUQUETTE: Or slide it in and do both welds, both sets of welds in the machine consecutively?

WHITE: The inner vessel is placed inside the outer corrosion barrier as part of the manufacturing process.

DUQUETTE: Right.

WHITE: It arrives in that condition.

DUQUETTE: It arrives with the lid already in place?

WHITE: It’s already inserted.

DUQUETTE: With the lid already in place?

WHITE: No, the lid we have to weld in place.

DUQUETTE: Yes, that’s what I’m asking. You now have a two step--well, you have a two step operation where you have to put the seal welds on the spread ring?

WHITE: Yes.

DUQUETTE: And, then, you have to move your machine up to the nickel area and do the final welds on the outer lid welds; right?

WHITE: Yes.

DUQUETTE: Okay. So, that’s done consecutively. You put it in the machine and do the inner lid, and then move out and do the outer lid?

WHITE: Yes. And, that will be demonstrated in March.

DUQUETTE: Okay.

ARNOLD: All right. Mark?

ABKOWITZ: Abkowitz, Board.

If I understand this correctly, you’ve been showing us a scenario that would involve putting a TAD into a disposal package, and then after welding it, be put in the mountain. There are other configurations, I imagine, for the DOE spent nuclear fuel?

WHITE: Yes.

ABKOWITZ: And, so, consequently, is this system agile enough to deal with the various configurations, or do we have to test them one at a time?

WHITE: No, this is adaptable for all of our six configurations that we currently have. The waste package transfer trolley will have the waste packages at the same height with respect to the closure room when they’re in place. The only difference will be the diameters, and the hole in the floor is approximately nine foot diameter, and the diameter of the widest waste packages, which are the DOE codisposal waste packages, are about seven feet. So, there’s about a foot clearance all the way around. And, these robots can easily move and adjust where they’re performing the welds.

ARNOLD: Okay, we have a question?

ROWE: Yes, Rowe, Staff.

Two quick questions. First, what are you plans for trying to develop a narrow groove weld in order to try to reduce the residual weld stresses, or do you have any plan for trying to develop a narrow groove technique?

WHITE: This is our technique for closure.

ROWE: No plan on doing narrow groove?

WHITE: What do you mean by that, reduced pressure electron beam welding technique?

ROWE: No, no, narrow groove is just another technique where you just increase the slope of the weld groove. It reduces the amount of weld passes, reduces the heat affected zone and reduces the amount of residual weld stresses. You might check with EPRI. EPRI has done a lot of work on that.

The second question is the fit of the outer lid to the waste package, fit up tolerance is on the order of 1.6 millimeters, I believe, and your positioning tool has a 1/16th inch positioning tolerance, and you're trying to do this over a six foot diameter disk.

WHITE: Yes.

ROWE: Do you see that as being a challenge?

WHITE: It's something that I'm very interested in. And, just to be fair, at the Idaho National Laboratory, they found that they can place much tighter than plus or minus a sixteenth of an inch.

ROWE: I'm just worried about the tolerance of a six foot diameter disk and a waste package that's been heated up, are you going to maintain the roundness of those two components to within 1.6 millimeters? 1.6 millimeters is less than two dimes.

WHITE: Well, the problem would be if that gap narrowed significantly. Because you can’t have, if you could move to the blow-up of the--keep going. Right there. It’s kind of hard to make out, but you can see the lid there on the right coming down, and then there’s a vertical portion at the end, and that’s about a tenth of an inch before where that radius starts up. We can’t have that lid on that radius. That would not be acceptable.

ROWE: That’s my concern. Either it’s too big, so it sits up on that radius, or it’s too small, and you have a gap. And, you’ve only got 1.6 millimeter tolerance over a disk that’s six feet in diameter. It just looks like it’s a challenge to me.

WHITE: I know it does, and one of the reasons we are heating it up is to see what the impact is, because the lid won’t be preheated, but the waste package will be that we’re going to be using. And, so, we’ll be able to tell then a whole lot by doing this prototype.

ARNOLD: Last question?

DIODATO: Thank you very much, Dr. Arnold.

ARNOLD: I will remind you that the boss is pretty nervous about our time.

DIODATO: I will be brief. Diodato, Staff.

Thank you for your presentation. As you know, this

Board has been a pretty consistent proponent of the value of incremental learning through engineering experience. So, I’m just wondering what have you learned so far, and what do you hope to learn in March when you do these tests?

WHITE: Well, one of the things we learned in December was that we did two brute pass welds about a week apart. After the first half was completed, we found that there was distortions on the order of a half to 1 millimeter. And, then, we did the second part of it, and it sort of stayed in the condition it was after that. That’s very small, and we actually did it in a manner by stopping that would create more of a distortion than you would if you had done a continuous weld.

And, that was also on stainless steel, and we would expect less distortion on the Alloy 22. But, we will be measuring the distortion, since we’re doing the closure weld process during the prototype demonstration in March.

DIODATO: So, then, that led you to modify your approach in terms of going with the continuous weld? Is that correct, or not?

WHITE: Well, no, our intent has always been to do a continuous weld. But, as this is a prototype and it’s being developed at Idaho National Laboratory, for their purposes, the lab personnel wanted to only do half at a time at that point, because they’re trying to develop this process so it’s workable in March.

DIODATO: Thank you.

ARNOLD: Thank you very much.

WHITE: Thank you.

GARRICK: Okay, thank you for keeping us right on schedule.

We now come to the public comment part of our meeting. I have five names here, and I've advised the desk that if they receive more, to bring them up to us. I see no reason to not just take them in the order that they were listed, and the first one is Steve Frishman.

FRISHMAN: I'm Steve Frishman representing the State of Nevada.

Listening to Mark Board this morning, it got me thinking about a topic that you've heard a little bit about in the past, and I know you have an interest in, and I just wanted to sort of make the observation that after waste emplacement in the drifts, there are three important safety elements that absolutely rely on being able to have free and unimpeded access in those drifts.

And, that includes first, drip shield installation. And, the gantry is going to have to be able to be able to move without having any debris causing it to not be able to move. And, also, it's going to have to be able to meet fairly tight tolerances for the operation that it's supposed to perform.

It also, the drip shield itself, will require that it have a clean and level surface to sit on. So, if there’s any debris at all, there is the possibility that in, I guess the worst case, you get a bad fit, and ultimately, what you’re up against is compliance with the EPA rule, because it’s pretty clear now that the drip shield is really carrying the load.

Another place that requires it, and still is going to take more than just a very clean access, is in Performance Confirmation, especially in trying to figure out how to actually inspect the containers. And, again, you’re going to have to have a debris-free system.

And, then, finally retrieval. If you have debris that develops around the container in some way, I don’t see any way to sort of get that debris out of the way so that you can operate the TEV essentially in reverse.

So, it occurred to me that maybe there, since I see little attention paid to this in the License Application, that there may be a need to do more than just sort of assume that the Swellex rock bolts and the Bernold sheets are actually going to be able to do the job for 100 years.

Well, I’ve talked to Mark about this, and he says that he believes that the, especially the Bernold sheets, are really overkill, and that he thinks the system will stay clean. But, you’re talking about 40 or 50 miles of a system that has to be done perfectly so that the operations can be done perfectly. Otherwise, you’re up against real safety issues that are having to be decided now.

So, it may be that it’s worth a little more inquiry into finding out just how perfect that system has to be, and also whether there are ways to mitigate the what I think are probably inevitable imperfections in the system. It just is a place that hasn’t been thought about very much, but it’s also right up against three very critical items in the safety case, and it relies on a, at this time, a very sweeping assumption.

So, just a point of information.

GARRICK: Thank you. Thank you very much. Yes?

KADAK: Could I follow up, please? Is Mark here?

BOARD: Yes.

KADAK: Kadak, Board.

I asked you a question this morning about the preferential, the value of, say, a cementitious grouting versus the Bernold sheets. In your professional opinion, which would be better to prevent the kind of problem that Steve just discussed?

BOARD: I don’t foresee the problem in what Steve is talking about being in the bolts themselves installed. The one advantage of those Swellex rock bolts, which I work with a lot, and it’s a standard product that’s used all over the world, is that you have a quality assurance check of the installation of the bolt when it’s installed, because it’s inflated in the hole, and you know exactly that it’s holding. When you put cementitious grout in, I agree with you that that’s a good system as well, and it would work I think.

The only difference there is is that you have--it’s difficult to have a quality assurance check of every one of those bolts, whereas, it’s an automated thing with this Swellex bolt.

But, having said that, I don’t think that the issue perhaps with what Steve is talking about is a bolt issue. It’s making sure that you have a proper installation of some surface covering to the material that prevents small particles from falling out. And the whole design, or specification of this sheet, was to use it essentially as a substitute for a spray-on lining of some sort, like shotcrete lining. And, so, the sheet was specifically specified so that it would last in that environment for that period of time.

I’m not a corrosion person, but that was the particular idea was that it would last over this period of time that Steve was referring to.

GARRICK: Okay. All right, next is Judy Treichel from the Nevada Nuclear Waste Task Force.

TREICHEL: First, I wanted to make a comment aside from the meeting. There has not been a meeting since the passing of Tom McGowan, who many of you may remember, certainly some of the staff would. And, Tom was a guy who was accused of being a gad fly, or sort of a comic figure, and that’s unfair and it’s inaccurate. But, he really did the heavy lifting for the public, and he was very often at these meetings.

He worked very hard on his presentations that he did in public comment, many times called me to ask about it, and he was everything that somebody who is concerned about the public should be, and, in fact, he’s run for office when he disagreed with office holders. And, he always wanted people to know that when they talked about community development, that the key word was not development, it was to worry about the community.

But, at any rate, Tom is gone and he is missed.

As far as the presentations today, it seems to me that there’s going to be problems when you have cans, canisters, whatever, the TAD holding 21 assemblies, many of the dry casks that are out there now holding 24, and a move to some 32 assembly baskets. And, we also heard about the problems or possibilities or risks that would come with misloads.

So, if you got these things that are all different sizes, and the repository is using strictly the TAD, which is the smallest of all of them, meaning that when things went for disposal into a TAD, you've got unloading problems, and there have been arguments whether these things would be unloaded at the reactor sites and made to go with the TAD so that it comes and everything works well at Yucca Mountain, or if there's going to be a shift of responsibility to Yucca Mountain.

But, for a complete non-scientist, this sounds complicated and sounds like it could very much add to the problem or the risk of the misload.

And, my one last thing, and I don't want to argue with anybody about it, and I've done this before, but when someone from the Nuclear Regulatory Commission stands up there and makes a statement like, "Eventually, all of this fuel is going to be shipped, ideally to Yucca Mountain," that something that should not be coming out of the NRC. I realize that people slip, but we always notice those things.

That's it.

GARRICK: Thank you. Irene Navis from Clark County?

NAVIS: Good afternoon. And, once again, welcome to Clark County, Nevada. My name is Irene Navis. I'm Clark County's Nuclear Waste Division Manager, and I want to thank you all for today's presentations and your insightful questions.

Based on today's presentations, and your questions and comments, I’d like to suggest a couple of agenda items for future meetings.

Based on some of the things that we heard today, I think it would be important for this Board to explore and identify some of the relationships between the potential impacts to Yucca Mountain resulting from some of the reports that DOE mentioned today, their interim storage report findings, their second repository report findings. There’s a document out there called the Gnet Programmatic EIS that I think has some interesting relationships to this program. The NRC’s updated waste confidence ruling that was alluded to today that kind of moves off that 2025 deadline that was part of that ruling for a long time.

I think it would also be useful to understand in a comprehensive fashion how those various documents and findings may impact repository facilities and operations, things like thermal load and transportation, as well as DOE’s stated goals of enhanced homeland security through consolidation of nuclear materials. I think looking at that in a comprehensive holistic viewpoint, showing how all of those relationships exist, and linkages exist, would be very important for this Board to understand.

The second item is to examine the relationship and correlation between the contentions and the RAIs. It was discussed a little bit today. Our understanding is that we’re probably going to see at least 200 RAI questions between now and May. That’s a lot of potential change and impact to how the License Application is going to ultimately look.

There’s also new information out there that may be integrated and should be integrated into the License Application, like the PVHA-U, for example.

So, based on all of that potential change and new information, the contentions, not coincidentally, I don’t think, track very nicely with the RAIs that we have seen out of the NRC so far, and I think it would be interesting for the Board to take a look at that relationship, not to look at the technical and scientific and legal merits of the application, just to understand the process and what that means to an amended License Application.

Final point. I just want to recognize the Professional Staff, under Bill’s leadership, and I look forward to continued positive relationship with Karyn and others on the Board, and the Staff.

So, thank you very much.

GARRICK: Thank you. Thank you very much.

Dr. Jacob?

PAZ: I will be very brief. The most recent publication in--I’ll give you the paper--on chromium 6, and radiation, either low LET, two dose, one at .5 and the other one is .05.

The first dose no synergism. The second dose showed additive effect. This issue has not been addressed, even at Yucca Mountain, and it probably will be challenged in court, and this is a significant issue. And, I just hope that the people, the next generation, God forbid that Yucca Mountain will be approved, that drink the Holy Water and get cured of their cancer.

Thank you.

GARRICK: All right. Okay, we have Jack Davis from the NRC.

DAVIS: Thank you. I just wanted to make a comment concerning a statement that was made earlier by the Board, suggesting that perhaps the regulator was a little bit too conservative with criticality safety when looking at it through systems risk approach.

I just wanted to point out that in a performance based rule, the applicant has many options that they can choose to address an issue, and they chose to address it by a probabilistic approach. They could have addressed it in consequence. They didn’t.

So, our job is not to, you know, look at it one way or the other. It’s to look at whatever they present to us, and then make the safety case from there.

So, I don’t think that it was fair that the NRC should be criticized of saying that we’re too conservative from a criticality standpoint.

And, just one other comment. With respect to Judy’s comment, you’re correct, Judy, no decision has been made yet with regard to the licensing of Yucca Mountain. So, that statement shouldn’t have been made.

GARRICK: Thank you. Thank you for your comment.

Okay, are there any other comments that anybody would care to make at this time?

(No response.)

GARRICK: I want to thank the presenters today. It’s a difficult assignment in front of the group, we realize that, and we appreciate the efforts that were made and the level of professionalism with all the presentations.

I want to also indicate that as we close this meeting, there’s another event in this area shortly, in about 45 minutes, and we need to clear the area. But, I think that if there’s corridor type discussions, and I would encourage them, there’s plenty of room downstairs, and there’s even a bar. So, I would encourage us to take advantage of that.

And, so, unless there’s further discussions or announcements that I should make, Karyn, is there anything that we need to say in closing?

Okay, then, we are adjourned. Thank you very much.

(Whereupon, at 5:15 p.m., the meeting was adjourned.)

CERTIFICATE

I certify that the foregoing is a correct transcript of the Nuclear Waste Technical Review Board’s Winter Board Meeting held on January 28, 2009 in Las Vegas, Nevada taken from the electronic recording of proceedings in the above-entitled matter.

February 9, 2009

Federal Reporting Service, Inc.
17454 East Asbury Place
Aurora, Colorado 80013
(303) 751-2777