UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING BOARD MEETING

May 29, 2008

Embassy Suites Convention Center Hotel
3600 Paradise Road
Las Vegas, Nevada 89169

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

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PROCEEDINGS

8:00 a.m.

GARRICK: All right. Good morning, All.

My name is John Garrick. I'm Chairman of the Nuclear Waste Technical Review Board. And, on behalf of the Board, I want to welcome all of you. We appreciate your attendance at the meeting.

We have a routine we go through each meeting. We introduce the members of the Board, and I'd like to do that. With respect to myself, my primary occupation right now is a consultant in the consulting world. I work in the field of trying to provide advisory services on the application of the risk sciences to different industries, such as nuclear, space, defense, transportation, off-shore platforms, chemical, et cetera. And, my background and areas of interest are risk assessment and nuclear science and engineering. And, my other assignment, aside from being the Board Chairman, is to have the technical lead on radiation dose assessment.

I'll introduce the Board, and I'll ask the Board member to raise their hand to identify themselves as I do so. I'll start with Mark Abkowitz. Mark is a Professor of Civil Engineering and Management Technology at Vanderbilt University, and Director of the Vanderbilt Center for Environmental Management Services. Mark chairs the Board's

Panel on System Integration, and is the Board’s technical lead on Transportation.

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

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

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

Andrew Kadak. Andy is Professor of the Practice in the Nuclear Engineering Department of the Massachusetts Institute of Technology. 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.

Ron Latanision. Ron is an Emeritus Professor at MIT and a principal and Director of Mechanics and Materials with the engineering and scientific consulting firm, Exponent. His areas of expertise include materials processing and corrosion of metals and other materials in different aqueous environments. Ron co-chairs the Board’s Panel on Postclosure 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 has performed numerous 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 and actually will be leading the discussion today on the TSPA.

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

Henry Petroski. Henry is the Aleksandar S. Vesic Professor of Civil Engineering and Professor of History at

Duke University. His current interests are in the areas of failure analysis and design theory. And, Henry is the Board’s technical lead on Surface Facilities.

We have one member of the Board absent today, Professor George Hornberger of the University of Virginia. George co-chairs the Panel on Postclosure Performance with Ron Latanision. Unfortunately, George has an unavoidable conflict that prevents him from attending today’s meeting.

Today’s meeting focuses on DOE’s Total System Performance Assessment, or more affectionately known as the TSPA. TSPA is the large computer model assembled from dozens of supporting computer models and data acquired over preceding decades. DOE uses TSPA to estimate radiological dose from Yucca Mountain to the public over the proposed one-million year regulatory period. TSPA is the central element of the collection of arguments that support DOE’s analysis of the safety of Yucca Mountain, and the Board has a long history of evaluating and commenting on the scientific and technical credibility of the TSPA model; as well as the input and output. The Board has reviewed and commented on all versions of the TSPA, through the 2002 TSPA for the Site Recommendation, to the TSPA for the License Application that we are examining today.

It is clear to the Board that DOE has made progress over the years in a number of areas where the Board had expressed concerns. As DOE has continued to refine and update its analyses, the Board has addressed some areas of ongoing technical concern. For example, the Board has asked DOE to provide a stronger technical basis for the exclusion of localized corrosion of the waste package due to dust deliquescence; has reviewed the revised DOE infiltration model; and has continued to investigate the extent to which DOE analyses present a realistic picture of repository performance.

Speaking of which, the Board tends to be guided by two underlying principles. One is to obtain a fundamental understanding, or at least be convinced that DOE has a fundamental understanding, of how the repository system will function both in the near and long-term. And, secondly, to push, as much as we can, toward realistic assessments of phenomena relevant to repository performance.

As to our future, the Board will continue to conduct technical evaluations of DOE’s progress in understanding how the engineered and natural systems of the repository work together to isolate radionuclides, and the extent to which DOE’s performance assessments are realistic.

The Board also will review DOE’s program for managing spent nuclear fuel and high-level waste before closure of the repository. In conducting its evaluation, the Board will not be constrained by judging the adequacy of compliance arguments or predictions. We leave that to the Nuclear Regulatory Commission.

As indicated earlier, Ali Mosleh, our technical lead on performance assessment, will chair the TSPA portion of the meeting. Before we get into the TSPA part of the agenda, we will have a project status report by the Chief Scientist from OCRWM.

As usual, following the technical presentations, we have scheduled time for public comment--an aspect of our meetings that we consider to be extremely important. And, if any of you would like to comment at that time, please enter your name on the sign-up sheet at the table near the entrance of the room. Of course, written copies of remarks can be submitted and they will be made part of the public record. Some of you have asked about questioning during the course of the presentations. Our preference is for you to write down your questions, submit them to either Davonya Barnes or Linda Coultry--they are in the back of the room. And, we will cover as many of such questions as time permits.

Before we get started, it’s important for us to remind everybody once again of how the Board operates. Board meetings are pretty much spontaneous by design. We express ourselves very freely, and we want to be able to continue to do that. When Board members speak extemporaneously, it is important to realize that this is the individual speaking, and we’re not speaking on behalf of the Board. We’ll do our best to make the distinction between individual views and Board positions. The Board positions are documents. They are usually expressed in our Reports to Congress and the Secretary of Energy, and these Reports, as most of you know, occur a couple times a year.

Finally, I would like to ask all of you, including members of the Board, to turn off your cell phones and put them on the silent mode.

And, I think we’re now ready to kick off the meeting, and in order to do that, we’ll ask Chief Scientist for OCRWM, Russ Dyer, to give us a project overview.

DYER: Thank you, Dr. Garrick.

First off, I’d like to pass on a message from Ward, who really would have liked to have been here, but got tied up in Washington, D.C.

Next slide, please.

What Ward asked me to do was to essentially hit some of the highlights of recent Congressional Appropriations testimony that he gave. What I’d like to start off with is the record for 2008. Last year in the springtime, in appearing before the Appropriations Committees for the House and Senate, he laid out five objectives that he had for 2008, and I’d just like to briefly review where we stand on those.

The first objective was to submit the License

Application for construction authorization by June 30, 2008. We are on track to meet or beat that date.

Second, was to certify the DOE’s Licensing Support Network Collection by December 21st of 2007. We beat that date. The Collection was certified on October 19th of 2007.

The third objection was to complete the Supplemental Environmental Impact Statement for the repository in 2008. We are on track to accomplish that.

Next slide, please.

The fourth was to deliver report to U.S. Congress on the need for a second repository. We are on track to complete and submit that report this fiscal year.

And, the last one was to complete the Final Environmental Impact Statement for the rail alignment in Nevada, and we are on track to complete that.

Now, let me move to the 2009 objectives that Ward laid out in the Appropriations Hearing. And, I picked what I consider the top five. There’s about eight or nine actually in the testimony, but first, let me note that the President’s request for FY ’09 for this program is $494.7 million. Now, how does that compare with the FY ’08 appropriations and request? The FY ’08 request, President’s request for this year was $494.5 million. The actual appropriation was $386.4 million. That’s $108 million less appropriated than was requested.

With that in mind, the five kind of top objectives that Ward laid out to the Appropriations Committee was first support the License Application for the repository review process before the United States Nuclear Regulatory Commission.

Second is to start detailed design of the repository facilities. I’ve got continue up there. It’s actually start, because there’s a DOE milestone CD-2 that must be approved before one can formally start detailed design.

Third is to continue essential interactions with State, Local and Tribal Governments needed to support the National Transportation planning. We will be doing a lot of talking this year.

Fourth is continue design and licensing work on the Transportation, Aging and Disposal canister system.

And, fifth, is to continue staffing and training the OCRWM organization.

Sometime ago, we talked about the four major strategic objectives that Ward had when he came in. The first one was the License Application. The second was to build an organization, build OCRWM into an organization capable of, or worthy of being a licensee with the Nuclear Regulatory Commission. And, this last bullet is directly tied to that objective.

With that, that’s all I have. I know you’ve been waiting a long time to hear about TSPA, and I don’t really want to stand in the way of all that. But, are there any questions from the Board?

GARRICK: Yes, Henry?

PETROSKI: Could you elaborate on the detailed design status? What is this that you said it has to start rather than continue?

DYER: Oh, yes. DOE has a series of decision milestones. There’s a formality to the project approval process within DOE. And, there is a decision called Critical Decision 2. And, Critical Decision 2 must be approved by the Secretary’s office before we can formally start what’s called detailed design in DOE parlance. The DOE system project management is not exactly the same as NRC parlance.

PETROSKI: So, what is the status as of today? Has it started, or are you still pursuing the CD-2?

DYER: It has not formally started. We are putting together the decision package for CD-2. That will be one of the major objectives that we have during FY ’09.

DUQUETTE: Duquette, Board.

I think I just saw a recent decision on the TAD to have at least some of the design work done by AREVA, the French company. Was that because there were no U.S. competitors for that who bid on it?

DYER: There were actually two contracts awarded. One was to AREVA Federal Services, and the second was to NAC International.

DUQUETTE: Again, I guess what I'm asking is was there some--was it strictly a financial issue to go off-shore for some of that design work, or was there some other reason for doing that?

DYER: I was not privy to the contract award process. I believe NAC is a U.S. corporation.

DUQUETTE: It is, but AREVA certainly is not.

DYER: Right.

ABKOWITZ: Abkowitz, Board.

Russ, are you familiar with a March 2008 document published by the Office of Environmental Management called Technology Readiness Assessment, Technology Maturation Plan Process Guide?

DYER: No, I don't think I am.

ABKOWITZ: This is a document that EM published, which is now their official protocol for determining whether a technology can be justified, or whether additional activities are required to do that. And, they were strongly urged to develop and adopt a policy by GAO and to follow some of the guidance that NASA and DOD had used in the past. I would strongly encourage your office to take a look at that document, because not only is it official protocol, but it's going to be a very important document as you get into your facility design process.

In fact, if you look at that document and you look at the Technology Readiness that should be in place in order to make a CD-1 decision, it’s Technology Readiness-4, which if I read from that document, it says this is equivalent to the component and/or system has been validated in the laboratory environment. That’s clearly not anywhere close to where the repository was when you had your CD-1 decision.

So, this is out there. Your own organization has formally adopted in another branch, and I believe that’s probably a standard that you might want to hold yourself to.

DYER: Okay, thank you. If I could, maybe I can get--come see you and write down that reference.

ABKOWITZ: Absolutely.

GARRICK: Yes, Andy?

KADAK: Kadak, Board.

Do you want to say anything more about when the License Application would be filed?

DYER: Not really. We will meet the June 30th date.

KADAK: Okay. Well, could you update us then on the status of the second repository work? What is actually going on there?

DYER: Well, that’s a report that is required by the Nuclear Waste Policy Act, a Report to Congress from the

Department of Energy regarding the recommendation about the need for a second repository.

KADAK: What have you done so far on that study?

DYER: The study is essentially complete.

KADAK: Will you be talking about this to the Board at all?

DYER: We will when we finish it.

KADAK: Okay. Do you feel that we have a different relationship than other agencies to review stuff like that before you finish it? Why don’t we get legal advice on that one.

DYER: Okay.

GARRICK: Any other questions?

(No response.)

GARRICK: Okay. Well, thank you very much, Russ.

DYER: Okay.

GARRICK: All right, Ali will take over the discussion lead on the TSPA.

MOSLEH: Thank you, John. Good morning.

As John said, I’m Ali Mosleh, and I’m the Board Technical Lead on Performance Assessment.

As John stated in his opening remarks, the Board is guided by two complementary central elements:

(1) Fundamental understanding of how the repository system will function both in the

near term, as well as over geological time; and

(2) Realistic representation of phenomena relevant to repository performance.

As you know, DOE has chosen to use probabilistic performance assessment to make estimates of the repository performance over the period of geologic stability, a period of on the order of one million years, according to the National Academy of Science, Environmental Protection Agency, and the Nuclear Regulatory Commission. Of course, probabilistic risk assessment, performance assessment, is an appropriate method to address the complex and challenging problem of geologic isolation of high-level radioactive waste, and spent nuclear fuel, in part, because it is a problem that involves phenomena from molecular to kilometer spatial scales, phenomena whose behavior is sometimes incompletely understood, and phenomena which operate over very long time frames, all with significant variabilities and uncertainties.

As a part of its ongoing assessment of the scientific and technical activities of DOE Yucca Mountain Project, the Board continuously examines the technical bases underlying DOE analyses, including assessment of data, assumptions, and models supporting those analyses. In December, 2006, the Board wrote OCRWM Director Ward Sproat with the following observations. And, I quote.

> “TSPA provides quantitative estimates of repository performance that are the core of the safety case. It is the primary tool for analyzing coupled interactions among multiple barriers that affect radionuclide transport, including the engineered barrier system, the unsaturated zone, and the saturated zone.
>
> To increase confidence in repository performance estimates, TSPA should include consideration of all credible and consequential phenomena that significantly affect dose over the period of regulatory compliance. Given the importance of TSPA, the Board is especially interested in the results of the new repository system performance assessments and how they affect the repository safety case.
>
> Assessing the realism of TSPA performance estimates can be challenging because some assumptions may be overly conservative while others may be nonconservative. The performance-margin analyses can be very valuable in assessing the magnitude and effects of conservative and nonconservative aspects of TSPA”

So, the purpose of today’s meeting is to provide DOE with the opportunity to present information that can be used to address the issues that the Board identified in that letter. To get started, today’s technical talks begin with a presentation by Dr. Peter Swift on the TSPA Modeling Approach with an Overview of the Results. In addition, Peter will provide some information on how water and radionuclides move out of the engineered barrier system following major disruptive events, including seismicity and volcanism.

You will notice that because of the timing and length of the presentation, we have scheduled a brief intermission between Peter’s talk and the discussion of his talk. So, please make sure that we’re all back in our seats after the break, because we plan to start promptly at 10:00.

After that discussion, we have a presentation by Dr. Cliff Hansen on the Performance Margin Analysis. The Performance Martin Analysis is a series of computer experiments designed to explore model output sensitivity to some of the particular elements of the TSPA. And, Cliff will describe how these elements were selected and how changing them affects the estimated dose.

After that talk, we will break for lunch. And, the first talk of the afternoon will be on Uncertainty and Sensitivity Analyses by Dr. Jon Helton. These analyses involve quantitative statistical investigations to identify the parameters in the TSPA model that are most significant to dose. Jon also will give examples of tracking two radionuclides through the repository, one that is highly soluble and non-sorbing, and one that is less soluble and slightly more sorbing.

We will then take a break, and after the break, we will hear a report from Dr. Ron Ballinger. Ron was a member of the team of external experts that conducted an independent review of the Performance Assessment for DOE. We are looking forward to hearing their findings.

Finally, as you know, DOE is not the only entity estimating the performance of the repository at Yucca Mountain. For comparison, Dr. John Kessler and his colleague Dr. Andrew Sowder, from the Electric Power Research Institute are here to present their analysis, which used a different approach to Performance Assessment.

And, obviously, we have a very ambitious agenda today, so we’d best get started without any delay. With that, it’s my pleasure to introduce Peter. Peter?

SWIFT: Thank you, and I’m happy to be here.

I’d like to start off with a couple of acknowledgements. This Performance Assessment, the Yucca Mountain TSPA has been going on quite a long time now, 15 years in a form that we would recognize today as being a direct ancestor of this work we’re doing today. And, I’d like to thank people who worked on it. There have been hundreds of people, and a bunch of them are here in the room now. Obviously, I’m just presenting other people’s work.

Don’t mistake me for a minute as the person who did all this. There’s a lot of other people. But, I’d like to start off just by thanking one person in particular, Bob Andrews, who is here. Bob was the manager of this PA and its predecessors throughout most of the mid and late 1990s, and through until two years ago when Sandia became the lead lab for the Project. Thank you, Bob.

Also, Jerry McNeish, who is with Sandia now, and has also been a manager on this work for many, many years. And, then, the technical leads, most of whom are here now, you will hear from them because when the Board asks me questions I can’t answer, I’ll be looking at people like Dave Sevugian or Bob McKinnon. So, thank you.

Can I have the next slide?

So, what I’m going to try to cover here, just in outline, a summary of the modeling approach. And, this will be a very brief summary. I’m not going to do a whole lot here with TSPA methodology. Probably have you take questions on it.

I’m going to spend some time on the scenarios and modeling cases. In the middle of this packet, you will find some slides that are pretty good reference points. They’re mostly words, but they lay out what I think are the key things you need to know about each of the modeling cases, things like how many packages were damaged and what the probability was for each of these modeling cases. Things that are important to keep in mind when you’re looking at results, or you want to know well, how come that one is there, that one is there, that information, what I think are the key points, are on those slides.

I’ll walk through results, I’ll try to hopefully leave you with an understanding of why the dose histories have the shape they do, the magnitude that they do, and what’s the uncertainty in that. I think those are important, you know, that’s the key to it. A little bit on the stability. I’ve got one slide on that. That’s something that I think is important to state, that we are aware that these are model results, and models may or may not have stable results. We believe these do, and we’ll show that.

And, then, the topics the Board specifically asked for, summaries of the behavior of both water and radionuclides in the Engineered Barrier System, that will be the drift environment, during the two modeling cases that dominate total performance, that will be, as you’ll see later here, the seismic ground motion case, and the igneous intrusion case.

Also, I’m happy to field questions all the way through, or a little ahead of schedule, and I think that’s fine. However, you’ll keep track of the time here, and we’ll get through it.

All right, next slide?

This is about all I’m going to have to say about TSPA methodology right here. But, basically, think of it as a series of steps, and that word “iterative” there, this is an iterative process. We go through each of these multiple times in the history of a project. I personally have been through this on the waste isolation pilot plant project previously, and you learn from each iteration of the Performance Assessment.

But, the first logical step, not always the first step actually done, but the first logical step is to identify the features, events, and processes, the FEPs--I’m sorry about the acronym--that are potentially relevant. And, that should be a very long list, an inclusive list. Anything that might be relevant, should be put on the list and evaluated. And, screen those, determine whether or not they really do affect performance, either in consequence or probability space, or there will be some that are simply outside the regulatory framework in which we work, things like, for the purposes of the Performance Assessment here is done for regulatory purpose, the regulation does not require, for example, evaluation of the consequences of acts of war, deliberate sabotage, that sort of thing. That’s of interest, but what we’re interested here in is the long-term evolution of a system that evolves basically in the natural environment. We do consider a human intrusion scenario. It’s a stylized one with a simple drill hole through the repository.

After you’ve identified those features, events, and processes that need to be analyzed because they do have a potential for impact in overall performance, you develop models, which are capable of calculating the consequences of those various events. You build abstractions of those models that allow the models to be linked together in a way that would permit rapid simulation. Ultimately, we’re going to want to do thousands and thousands of simulations. So, these models have to be both fairly comprehensive and very efficient. It’s a tough modeling job.

Then, there’s one phrase up here, “Develop models along with their scientific bases,” and there it is, second from a couple of commas. And, that, of course, is where 20 years of good work has been done, developing the scientific bases for this TSPA. But, from our perspective, these are the steps we follow, and the science program itself has provided the basis for the TSPA.

It’s a question I know the Board is interested in, how well linked is the TSPA to that scientific basis, and I welcome questions on it. And, I will come to it again in a minute here.

Once you have models, obviously, you have model input parameters. You evaluate the uncertainty in those, and you assign uncertainty distributions to them. And, those uncertainty distributions should reflect honestly our state of knowledge about where the true values might lie, because then you can sample values from those and run them through the full system model, and you get a range of results. So, when you see these things, they’re called horsetails, plots that have lots and lots of result histories on them, that is the result of the uncertainty in the model inputs.

Build the integrated TSPA model, and perform calculations. Okay, we know how to do that. And, then, the last step here, evaluate the performance, considering uncertainty through Monte Carlo simulation. That’s hundreds of individual simulations each using different sampled inputs, each, therefore, representing a possible future state of the system from which, for regulatory purposes, we focus on the mean or median performance. But, we display the full range of possible outcomes.

Next slide, please?

Now, I realize this is pretty hard to read, either from the room, or on the handout. But, the purpose of this is to show you the body of science that underlies the TSPA. That’s the point I want to make with this slide. We have a system model up here at the top. It has in it major model components, unsaturated zone, engineered barrier system, waste package and drip shield degradation, waste form degradation, and so on. Over here on the right-hand column, are the external events that may disrupt that system.

In each column below that are the sub-models, each one of which itself is a major piece of technical research and understanding. We have had people who have spent literally careers working in one of these boxes down here doing excellent work. And, each one of these sub-models, that's where the link to the underlying science occurs, and it's the numbers, for those who want to read the handout carefully, those refer to sections in the TSPA report where you go to understand, for example, the sub-model for drift wall condensation, yes, we have one, we take that into account. Go see Section 633, I think it says. And, there's a good summary discussion there, and that will refer you to the underlying technical reports that form the basis for that understand in that model.

It looks like a daunting amount of information that flows into the TSPA. It is. It's a lot. It also, in this figure, looks like it's very straightforward and linear. Everything flows out nicely. It's not straightforward and linear.

Can I have the next slide, please?

It looks to many of us more like this. This figure, what it shows here is information flow among the computational models that make up the TSPA. Over here on the left--I don’t expect anybody to follow this, but it’s okay--over here on the left are what we call the process models that are run outside our TSPA GoldSim simulator. These are typically Fortran type models. They’re large process models that run relatively slowly, have lots and lots of detail in them, and they simulate the major processes, such as, oh, unsaturated zone flow, unsaturated zone transport, the drift degradation, the response of the waste packages to ground motion, infiltration, and so on.

These models pass information back and forth, input and output, and they provide the primary input to the TSPA model across this interface here. Much of that comes in the form of lookup tables of results. So, we end up with a broad range of values of output from this set of models here that characterize the underlying processes. Then, that output can be run quite quickly, sampled as uncertainty, and run quite quickly in the TSPA model itself.

Many of the major processes are simulated directly in TSPA. For example, the flow and transport in the engineered barrier system is done directly in TSPA. Waste package degradation is done in TSPA. Unsaturated zone flow and transport. Saturated zone flow and transport is generated, they’re breakthrough histories generated external to TSPA, but then the actual transport is calculated through a convolution integral directly in TSPA.

The dose calculation is done external to TSPA over here, but the BDCFs, the biosphere dose conversion factors, are used directly in TSPA to produce the dose. Only one dose is shown there, but this is run thousands of times.

The point of this figure is (a) it is complicated, and (b) it is understandable. If you chose to try to work through all these little footnotes down here and tell you what’s being passed between what, it should all work. It’s there. So, I’m not sure where I go with that.

But next slide, please.

Now, the documentation. For those who want to actually read the TSPA, I highly recommend it, it’s gripping. It’s 4,272 pages. It’s in four volumes. I have a little story on that. The three volumes, basically, the first volume describes the model components. The second volume describes the test cases done to demonstrate our confidence in it, things like verification tests on components, system level tests. This basically is our--sorry, they’re in different order here, Volume I, III, and II. Volume II is basically a summary of the model runs we did that are not directly part of the compliance case. Volume III presents the compliance results, and also has many appendices that provide additional support.

We produced these three volumes internally. The work for them was done in the fall of 2007, and the results presented in Volume III here were the results that appeared in the draft supplement to the Environmental Impact Statement in October of 2007. In the fall of 2007, we found a couple of issues in those results that we were uncomfortable with. They were clearly at that time draft results. And, we decided we would update them. We would do a rerun of the TSPA to adjust for some--make some changes and corrections that we felt were things we would be more comfortable going into Licensing with, with a new set of results. And, that's this Addendum here, which each one of these is a big thick binder. They're roughly of equal size.

So, the results for the license application are contained in the Addendum. If you get it electronically, you will get it all as one file, but the pages aren't merged together. So, it takes all four volumes to understand the TSPA. The Addendum also includes the documentation of every change we made between the Rev 0 and the Addendum. I think that will come up again in Cliff Hansen's talk.

Okay, that was it for the background information. Let me move right on into what we actually did here now. We recognized four scenario classes. That's a term the NRC has asked for. And, within these four scenario classes, the nominal performance in which the system evolves in the absence of significant disruption, an early failure scenario class, a seismic class, and an igneous class.

There’s also, and I’m not going to talk any further about it, there’s a human intrusion scenario, which is not in NRC speak, a scenario class. It’s not part of the futures that all have to sum to a probability of one. It’s a required stylized analysis that assumes a drill hole goes through the repository at sometime in the future. And, that analysis was done. The results of that analysis show consequences are lower than these, therefore, I’m not presenting it here, but it is documented in the report.

All right, within each of these, the nominal case, only one modeling case within it, but we, for the purposes of calculation, divided the others, each one of them into two separate modeling cases, because that was the most efficient way to use the model.

So, in the early failure scenario class, we look at waste package early failures and drip shield early failures separately.

In the igneous class, intrusion and eruption are treated separately. Very different results of the same geologic process. If you were to have a volcano in the vicinity, or at Yucca Mountain, you could have the intrusion case in which magma floods the repository and/or the eruption case in which the conduit goes onto the surface and causes an eruption. In our set of assumptions, quite logically, you may have an intrusion without an eruption. You cannot have an eruption without an intrusion first, or at the same time.

And, the seismic scenario class, two modeling cases, a ground motion modeling case in which packages and drip shields are damaged by shaking, ground motion from seismic events in the region, not necessarily directly at the repository. You get rock fall from that case, and packages and drip shields actually moving from package to package, and so on. And, in the fault displacement modeling case, we look at the consequences and the probability of a fault rupture directly in the repository physically sharing packages.

Next?

These slides here, I don't want to spend too long on them, but this is the important stuff that will help you understand why results look the way they do. For each of the seven modeling cases, I've got one, or perhaps two, slides that outlines what I think are the key points here.

So, for the nominal case, first, in this case, there are no releases from packages until corrosion creates a pathway. And, that would be the undisturbed evolution of the system. The packages don't leak until something reaches them, and the nominal process is corrosion.

As modeled, waste package failures are rare before 100,000 years. And, those first failures when they do occur, they come from stress corrosion cracking of the closure welds that occur after general corrosion has removed the annealed layer upon the outer surface of the weld. So, general corrosion has to remove part of the--well, the thickness of Alloy 22 on the weld before you get a stress corrosion crack initiating in the unannealed material underneath. And, that type of cracking is common by 500,000 years in our model. But, releases through those cracks, these are tight cracks, releases occur by diffusion only. We have looked at the ability of water to flow through cracks that small in aperture, and we conclude that diffusion is the transport mechanism rather than advection. Advection is water flow.

In this nominal case, drip shield failures due to general corrosion of the drip shield, sort of uniform thinning of them, occur between 270,000 and 340,000 years. That basically is our uncertainty in the Titanium Grade 7 corrosion rate. So, drip shield do provide protection from seepage water until they fail, and then they do not.

Now, this last bullet down here, waste package, we call them “patch” failures, because our model is set up with patches, patch failures due to general corrosion rarely occur before 500,000 years. And, a large number of waste packages, a large majority of waste packages, still do not show patch failures at a million years. These would be full scale wholesale holes in the side of the package. Those patch failures, when they do occur, those do allow flowing water and advective releases of radionuclides out of the package. And, by the time we have patch failures occurring in the Alloy 22 waste packages, the drip shields have failed, so for those are in seeping environments, there is, in fact, water flowing out of the package, dripping out of the package--flowing is a strong word there--but there is dripping water that would allow transport out.

Next, please?

The early failure cases, and we look at both waste package and drip shield, for both cases we assume the failures occur at the time of the repository closure. That's simply an assumption. The failures conceptually are assumed to be due to manufacturing defects, weld flaws, undetectable and undetected weld flaws for the waste package case. Emplacement errors, manufacturing defects for the drip shield case, and I can refer to--Neil Brown is here and he can answer more questions on what the technical basis is for the early failures.

We did develop probability characterization of the probability of the early failures, and it's very low. This is based on industry data for comparable manufacturing processes, and it takes into account the ability to mitigate and repair--to mitigate detectable errors. These would be the undetected ones. So, here's a number you really want to pay attention to in here. The probability of one or more early failure waste package in the entire repository, that’s 11,629 packages in this model, I believe, roughly 12,000, .44. And, if we have that .44 probability of one or more failures, the expected number is two and a half. So, basically, it’s on the order of one package per repository may have an early failure in this model.

And, when that failure occurs, the package itself is assumed--I didn’t put that up there--the package is assumed to be completely failed, provides no further protection. But, the drip shield remains intact in this modeling case until it fails by general corrosion processes around 300,000 years. So, for the first 300,000 years, from the time the repository closure to 300,000 years, it’s still a diffusive environment on that failed waste package.

The drip shield case. Drip shield failures are considerably less likely, because they can be fully inspected. They don’t have that final weld after the material is in place, and they can be inspected after emplacement in the underground. So, we believe the probability of a drip shield failure is considerably lower. So, drip shield failure is a fairly rare event in this analysis.

For a simplifying assumption, the waste package under an early failed drip shield is simply assumed to also fail if we’re in a seeping environment. This was an assumption made to simplify the question about whether or not localized corrosion would occur on a waste package that did not have a drip shield over it. So, it’s a rare event, but has a fairly high consequence if it does occur, because you remove both the drip shield, and for those packages that are in seeping environment, you remove the waste package also. So, you have releases by both--transport out by both advection and diffusion in that case.

KADAK: Excuse me. This is Kadak.

SWIFT: Yes.

KADAK: I understand the numbers, probability of one or more failures, that’s this 44 percent chance of having one or more failures; is that what you’re saying?

SWIFT: Yes.

KADAK: Okay. Of all the waste packages?

SWIFT: Out of all of them, it’s .44 probability that there will be one or more.

KADAK: And, the other one is about a 2 percent chance that you will have early drip shield failure; is that correct?

SWIFT: Yes.

KADAK: Okay.

ABKOWITZ: While we’re on this slide, could you explain why you’re using the median rather than the mean probability in these cases?

SWIFT: We're actually using a full range of probabilities. Those are just presented as examples.

GARRICK: While we're still on that slide, on the expected number, if you viewed that as expected frequency, could you say anything about what the probability of frequency distribution looks like? In other words, how much does that number vary between something like the 5th and 95th?

SWIFT: Cliff or Jon, do you want to take that? Jon Helton and Cliff Hansen are better qualified than me to answer that one. Jon, if you do it, go to the microphone and introduce yourself.

HELTON: Jon Helton. The failure of the--early failure of waste packages is assumed to follow a binomial probability distribution. So, what you have is basically a probability that one randomly selected waste package will experience an early failure. But, we've got 11,629 waste packages, so what you have is a probability that there's no early waste package is a probability that there's exactly one, and probability that there's exactly two, and so on up. So, when we do the analysis, we consider the possibility and incorporate it into the final numeric results of no early waste package failures is exactly one, exactly two, and so on.

Also, in doing the analysis, we consider whether you have an early transportation aging and disposal fail, or a codisposed waste package fail. We also consider whether the failed waste package is in one of five different percolation bins in the repository, five different areas of the final hydrologic properties, and also whether or not the failed waste package experiences dripping conditions.

The probability that defines the binomial probability distribution for early waste package failure is itself treated as being uncertain. And, I think, Dr. Garrick, that was your question, yes, I can’t recall the exact numbers, I could look it up for you in a minute or two, but there is a fairly substantial range of uncertainty in the probability that defines the binomial probability distribution for early waste package failure, and that is incorporated into the analysis, and later on in my talk, and probably in Peter’s, you will see a range of results that is driven by that uncertainty and that probability.

GARRICK: Okay.

HELTON: And, drip shields are treated similarly.

GARRICK: Yes, thank you.

SWIFT: I’m going to move on. Next slide?

The igneous scenario class. And, the key information here for the intrusion case, there is an uncertain probability frequency for which the mean is 1.7 times 10 to the -8 per year, and that one comes out of the expert elicitation that was done in like the 1990s. And, the range on that is--it’s more than two orders of magnitude. I don’t have that number in my head, but since the--you will see that. Actually, it comes out later on a little bit. But, we’re focused on the mean, overall mean consequence, and the mean frequency is what drives that.

The key assumptions here, all the waste packages and drip shields in the repository are sufficiently damaged to provide no barrier to flow and transport. So, all 11,629 packages, the contents of their waste are exposed and ready for transport. And, because we have a--conceptually, the drifts are filled with magma and volcanic material, we have no capillary barrier, therefore, we have set seepage equal to the percolation flux above the repository horizon. So, the igneous case, conceptually, the magma fills the, from wherever the igneous event might intrude the repository, it fills the entire repository with magma, and all the drifts are sufficiently filled. There’s no capillary barrier. Those two assumptions are bounding assumptions. I don’t like to use conservative assumptions, but those clearly are. We do not know how conservative. We do know they are bounded. You cannot break more than all of the packages. And, that’s a good bounding position to be in.

However, it is a very rare, low frequency event in probability space, and just above the regulatory cutoff for consideration. So, that weighs heavily in how it appears in the total probability weighted dose estimates that you’ll see in a minute.

The eruption modeling case. The first point here, as I said earlier, you can have an intrusion without having an eruption. Once a volcanic dike--that’s the name for the tabular body of magma that rises through the earth--once it reaches the repository horizon, it is almost certain, and conceptually, we think it is certain to have an eruption somewhere, but the eruption need not be within the repository. It could be anywhere along the many kilometer length of a dike. It also could occur--these conduits are not all that large. They’re on a scale of meters to tens of meters. The conduct could easily be within a pillar space and not within a drift. So, we end up with a probability that an eruption that will intersect a waste filled drift, conditional that there was an igneous event that intersected the repository at all, it’s a .08 probability, conditional on this probability up here.

The mean number of packages that are intersected, and, therefore, available to be erupted, is 3.8. And, that’s a range that’s considered in the analysis. I’m just presenting the mean here. That is largely determined by the uncertainty in the diameter of the eruptive conduits. But, packages are on the order of five meters long, and we assumed any package partially intersected by a conduit is going to be fully available to be erupted. But, we do not assume--this is a change from previous analyses--all that waste is actually incorporated in the erupted plume. We acknowledge that some of it is likely to remain in a lava cone, or a scoria cone, lava flows or scoria cones directly at the point of the eruption, and will not be carried far downstream in the wind, in the air.

We sampled a--I forgot the name of that parameter, but we sampled a parameter that allows us to have only a fraction of the waste erupted, and the mean value of that parameter is .3. So, .3 of this number is actually available to be included in the plume transport. Once it lands, we’ve added this since the previous analyses, one a plume settles out downwind from an eruption, we do look at the possibility that it will be redistributed by stream processes, water erosion, carried down Forty-Mile Wash to the exposure point.

Next slide?

The seismic class. And, I did not include something important on here, but it will come up. First, we’ll talk about the ground motion, and then we’ll come back and talk about the fault displacement.

Ground motions result in stress corrosion cracking that allow diffusive releases. These are from package to package and package to pallet. I guess primarily package to pallet impacts as packages are actually shaken during ground motion. And, these are very approximate numbers. We consider a full spectrum of annual frequencies with ground motion events at different magnitude occurring at different annual frequencies. But, it turns out the annual frequency of events that are of sufficient magnitude to damage the codisposed packages--these are packages that have glass waste and DOE spent fuels in them--is on the order of 10 to the minus 5 per year.

The frequency of events that damage the TADs, transportation, aging, and disposal packages that have the commercial spent fuel in them, is considerably lower. This is because the TAD itself is a big piece of steel that adds a lot of strength to the package. So, it is much harder to damage a TAD package through ground motion. It happens much less frequently in the analysis.

KADAK: Peter?

SWIFT: Yes?

KADAK: Kadak, again. I’m just wondering, you went to a lot of work to define the criteria for the TADs, but we haven’t heard a lot about the criteria for the codisposal packages, particularly given the three orders of magnitude difference in failure probability. So, I’m just wondering why the effort wasn’t made in that area as well.

SWIFT: Okay. Neither package was engineered with its strength with respect to ground motion damage in the far future as a major driver. The TAD was there for transportation strength, and it was not--the realization that that strength actually played a role in the long-term performance didn’t come until we analyzed it last year.

Were the TAD to not be there, yes, we would see stress corrosion cracking as a result of the changes in the stress state of the CSNF packages also.

KADAK: Are the materials for the codisposal package Alloy 22 as well?

SWIFT: Yes. Both packages are essentially the same in the outer layer construction, in Alloy 22, two and a half centimeters of that, and four centimeters, five centimeters of stainless steel. Neil, is that right? Thank you. Neal is nodding.

KADAK: But, the waste package is the thing I think you’re talking about there.

SWIFT: Yes, but the TAD is inside the waste package for the CSNF, and it does have a structural function, whether it was intended or not. It’s there and it’s a good, strong piece of steel. Another way of thinking of it would be--and, maybe that’s your question--why don’t we have TADs inside the codisposed packages also just for the strength? Doses are very low here as we get to that point. The performance is excellent in the repository. I’m not eager to add to the cost of something that wasn’t planned for.

KADAK: Okay, thank you.

SWIFT: These stress corrosion cracks, if there are questions that may come up later as to where and how they form on the packages, Dave Sevugian and Neil Brown are probably the right people to answer those. I’m not going to try and get into that now. But, basically, the ground motion stresses, changes the stress state of the package, and stress corrosion cracks initiate after that.

The cracked area accumulates with additional seismic events. So, when we have a second ground motion event, we do increase the area that is cracked. And, basically, it’s the cross-sectional areas of the crack that are major controlling factor on the magnitude of the diffusive release.

The repeated damage may actually cause--this would be, for example, repeated ground motion events after cracks have allowed degradation of the internals--can cause rupture of the waste packages. That would be from internals moving around, actually breaking through the package, or from the package actually being broken by being banged around. Those are very rare in the analysis. These come out of the NAC analyses done at Lawrence Livermore primarily, and also at ITASCA. We account for these ruptures in the analysis, but they turn out to be pretty rare events.

The drip shield in this case does thin by general corrosion, and eventually will fail due to dynamic loading from accumulated rock fall on it. And, that typically happens before it would have happened in the nominal case in which we do not have the extra load from rock fall on the package. It happens after 200,000 years, though.

We do include nominal corrosion processes for the million year analyses for the seismic scenario class because the two are, corrosion and the package response to ground motion, are fully linked. You can’t separate them. Corrosion affects the response of the package and the drip shield because of material thinning, and, therefore, we look at response of thinner drip shields and waste packages. And, also, once we have stress corrosion cracks allowing diffusion into and out of the packages, we allow the corrosion of the internal steel components, which then has the effect of reducing the strength of the system. You remove the inner steel vessel that supports the Alloy 22, and in the case of the TAD, you remove the package also.

Next, please?

The fault displacement case, not a lot of time here. I’m not going to spend a lot of time here. But, first, the annual frequency of direct fault ruptures in the underground that might intersect waste packages--this comes out of the probabilistic seismic hazard assessment done in the late 1990s, and it is rare. We are offsetting packages from the active faults, the known active faults, the

Solitario Canyon and Bow Ridge, and the probability of an offset on other faults is very low. But, we include it.

And, if it happens, we rupture the waste package and the drip shield at the time of the event. That immediately allows advection and diffusion, both out of the ruptured package. The size of the rupture, we treat as an uncertain variable, and it ranges from zero at the low end to the cross-sectional area, as if we had sliced a package in half. And, we only damage packages that would be actually on a fault trace. That would be a mean of 47 packages. It’s uncertain.

You can see that compared to, for example, the igneous case where we damage all the packages in the repository, the probability of half that 7 to 10 minus 8, damaging only 47 packages, at roughly the very low probability, is going to be a small contributor of the total.

Okay, results. Next slide.

I’m going to come back to these. We just put them up so we can see the results first. Just a note here on referencing. This reference down here is the reference to the TSPA report, and the AD means to the addendum. If one were to look it up internally through our own record system, that’s the number you would search on. That is available on the LSN. It’s a bit of a nuisance to retrieve it that way. I believe the Board does have a copy of it, though, in its full form.

On the left here, 10,000 year results, and on the right, a million year results. We choose now to display these with linear time scales. You've probably seen them in the past with logarithmic time scales. The advantage of showing them on a linear scale is that it puts more resolution out here in late times, but it means you have to show two plots in order to get any resolution at the early times.

I focus almost entirely today on the million year plots. Now, what you see here are, first of all, these do take into account the probability of the events that contribute to them. I'll come back to the terminology we use for that in a minute. The red curve is the mean, and the blue curve is the median, 95th and 5th. There are 300 realizations of the uncertainty in the analysis shown around the mean, median, et cetera.

Quick points to note, that the maximum dose out in here for the mean does occur at a million year. It's about 2 millirem per year. The median actually peaks a little before that, just under a millirem.

Next slide, please?

These are each of the seven modeling cases. Actually, we only show six here, and I'll explain that in a second. Each of the modeling cases that contribute to the total, and all we’re showing now is the mean from each modeling case. We’re not showing the full horsetail. So, if you go back to the previous slide--don’t go back there, but in your mind--there was a red curve. Each one of these now is that equivalent red curve out of a horsetail from each of the modeling cases.

And, things to note here from the bottom up, in terms of what is contributing to the total, down at the very bottom is this drip shield failure event, which is very rare. When it does occur, you have both diffusion and advection out of the package immediately. Above that, we have volcanic eruption, which is also a very rare event, and affects relatively few packages. Above that, we have the waste package early failure, which is primarily a diffusive release until the drip shields fail, somewhere in there, it’s entirely a diffusive release until then. It has a higher probability of occurrence in the drip shield failure case. Therefore, it appears as a larger contributor. But, still, this is a log scale on the consequence axis here. It’s orders of magnitude off the road of main contributors.

There’s the fault displacement case, and what’s actually driving our total for both the 10,000 year and the million year analysis are the igneous intrusion, which affects all the packages, and the seismic ground motion case, which also affects all the packages. So, the message there is if you want to have a large consequence, affect all the packages. Makes sense.

I’ll come back and try to explain the shape and magnitude of those curves. I will not have much more to say about the other ones, except one comment on the igneous eruptive that I’ll make in a minute here.

Next slide, please?

A little bit of terminology here, and this is a slide of Cliff Hansen’s. I’m not a mathematician. Jon Helton, Cedric Sallaberry, Cliff Hansen, are other speakers today, are mathematicians. And, the formulism with notation is something that is pretty precise in this analysis. Appendix J is primarily Jon’s work in the formulism of how we constructed the modeling cases, and how we built total dose out of them.

But, just when we say dose, we simply mean the annual dose to the reasonably maximally exposed individual, the RMEI, that’s 18 kilometers away. And, if we don’t specify otherwise, dose means it includes all the radionuclides, summed over all of them, and it’s a function of both the aleatory uncertainty and the epistemic uncertainty. The aleatory uncertainty most prominently being the uncertainty associated with the time of events. But, there are other aleatory uncertainties, other uncertainties we treat as aleatory in the analysis.

The epistemic uncertainty being essentially the uncertainty associated with our incomplete knowledge of the right values for the input parameters in the Monte Carlo analysis, things like the rock properties of the transport pathways would be typical epistemic uncertainties. The epistemic uncertainties are the ones that produce the 300 plots in the Monte Carlo simulation.

When we talk about an expected dose, precisely, we mean that's an expectation taken over the aleatory quantities. I'll give an example of that in a second. And, it's typically conditional on a single sampling of the epistemic uncertainty in the analysis. So, if all the model parameters had those values, but we still had events occurring at uncertain times, that would give you the expected dose conditional on one epistemic uncertainty, one realization of epistemic uncertainty.

The mean dose is a mean--I just showed it in the previous slide for each of the modeling cases. It's the expectation over both the epistemic and aleatory uncertainty.

The total expected dose, that's those horsetails I showed two slides back. That's the expected dose summed over the modeling cases showing each epistemic vector. That's the 300 curves that contributed to the mean I showed a couple slides back.

And, the total mean is typically the, for 10,000 years anyway, is the regulatory performance measure. That was the red curve two slides back.

Now, here’s just an example from the eruptive case of how to put some of this in context. This also provides an opportunity to see what--and this is something that the Board has asked about in the past, and others are curious, what we call the conditional dose. In this case, I show volcanic eruption because it’s relatively straightforward.

Over here on the left, we have looked, these are 40 realizations sampling on aleatory uncertainty, other than the time of the event, primarily in this case, it’s wind direction and speed, also some of the properties associated with the power of volcano that we treat as aleatory. But, we forced an event to occur at time zero here, volcanic event, and we forced it to erupt a single waste package. And, time zero would be the year, in this analysis, would be the inventory decayed to the year 2117. So, a hundred and however many years from now.

The assumption is made here that humans are at that time, hundred years from now, unaware of the hazards posed by the repository. They do not evacuate. They sit there and continue to live in the area after a volcanic eruption.

Each one of these curves then represents the dose. A person, if an event happened in the year 2117, that by year 5000, this would be the spread of doses they’re getting.

And, basically, the lower ones come from cases where the wind is blowing away from them, and the higher ones come from where the wind is blowing towards them. No great magic there.

Take the mean of this, and you get the red curve over here. And, now we put in the uncertainty and the time when the event occurs. So, now we’ve got events at selected times out in the future. For the purpose of the analysis, we actually fill that in with a very high density of events, but it makes a plot hard to see, so, hard to understand.

The drop-off in the peak here is basically a radioactive decay function, inventory of the repository decay. So, if an event happens at, I guess that will be 4,000 years, that would be the initial one year dose consequence from one waste package being erupted, and it would drop off in the future as soil processes, radioactive decay, remove the radiation from the near surface area where the person was living.

Now, if you draw at any one time through here, you try and calculate the expected value for a person living at year 1000, 10,000. They could be getting a dose from an event that happened at year zero, or year 100, 5,000, and so on. They also could be getting no dose at all because an event never occurred. And, if you draw the--essentially, you sum all the consequences at any one time slice, and take an average of that. You end up with a curve that starts out low, because there may be one high contributor, but all the rest are zeros, because you've got a long future ahead of you and nothing has happened yet, and at later times, there's an increasing probability that an event already happened, but the consequences of events far in the past are greatly diminished.

So, take an average through that, and you get a curve that looks something like that, and that's what is shown down here. Now, here, we have corrected, or included, the probability of the event. And, each one of these is for a different sampling of the epistemic uncertainty in the system. It is a mean drawn through a family like this one here. Remember, these were means. So, you take, for each one of these, you construct a plot like that, and draw a mean through it, and you get this thing down here. These, we would call the expected doses, and then over here, you get the mean in red of this family here, and that would be the one that was shown two slides back as part of the summation.

So, I show that because we do believe it's important to show how we built those means the regulation asks for, and what the conditional consequences, if the event occurred, would be.

Next, please?

Now, I showed this one a minute ago. What is the uncertainty in this? What’s the shape of it? How do we interpret it? Now, I’ll be working on that from now on. Welcome to come back to this one.

Next slide, please?

This, again, I showed this one a minute ago. It’s the mean contributors to it. Again, from the bottom up, the drip shield early failure, the igneous eruption. That actually is the same curve I just showed previously, except that that was only for 20,000 years. This is out to a million. So, the whole previous plot that I worked through with showing how we built it was just the early time in this one.

Waste package early failure with diffusive releases until the drip shield fails. The fault displacement case. These two up here, the spiky one that dominates you see at the beginning, it’s actually larger until several tens of thousands of years. It’s the larger contributor. And, then, the igneous intrusion takes over. That’s the olive green one as opposed to the sort of green there. It’s green on the handout, I think. And, then, by the time you get out to a million years, the seismic case is dominant once again.

We don’t show a nominal case anywhere here, and the reason is because for the 10,000 year plot, not shown here, there were no releases from the nominal case. Remember, there were no releases possible until you’ve reached a package by corrosion.

For the million year case, we chose to include the nominal with the seismic ground motion, because we modeled all the nominal processes anyway in the seismic ground motion case. So, we call it seismic ground motion here, but it actually does include the nominal case. And, I'll say a little bit about that.

Next slide?

So, the total, that horsetail on the previous page, which is down here, is the sum of all the other modeling cases, and I only shown two of them here, the two that matter, plus volcanic eruption, because if you were to resolve the first few hundred years carefully, you discover volcanic eruption is the only contributor until 300 or 400 years out, because it takes that long for the very first arrivals of ground water. So, in the first few hundred years, the only way to get a dose from that repository is if there should be the improbable volcanic eruption.

All right, so, here is the horsetail associated with epistemic uncertainty in the igneous intrusion case. Here it is with the seismic ground motion case. That includes nominal. And, basically, this is the--there are treated curves on all four of these, but basically, the total here, each one of those curves is the sum of one of these curves, and it's matching--they all use the same sampling of the uncertain parameter. It's matching curve from this set here.

So, for example, this curve here is essentially that one, plus whatever the other component is here, but this one is big enough, it appears essentially undistorted there.

Next slide, please?

A little bit here about the seismic case. I think this is probably the one that is, to me anyway, that I find the most interesting. On the left here, we've taken out the seismic effects, and now I'm actually showing, and this is Cliff's plot, just the million year dose from nominal processes, if there were no ground motion damage, no rock fall, drift collapse due to ground motion, and no cracking of the packages. These releases come from stress corrosion cracks that occur after the thinning of the Alloy 22 has removed the annealed by air over the closure welds.

So, you see a large number of them. There are a handful that appear and are mostly early, but the large number of them don't start appearing until after 100,000 years.

These doses are then included in the actual modeling case we used for the dose summation. We ran this separately, but the processes are included in this one. This is the one that's part of the compliance case over here. But, this is now overlain with essentially the ground motion, the cracking from seismic ground motion. And, so, what do we see in this plot here? This is Cliff’s stylized decomposition, Cliff Hansen. But, you see there are three basic types of curves in here. There are curves that rise up early and then drop off, and then there is a family of curves that come in somewhere here, and level out. And, then, there are curves coming a little later and climb. You really can only see those out here at the very end.

So, what are they? The first type here in blue, these are releases primarily of Technetium, but they are releases from diffusion out of codisposed packages that cracked in ground motion. And, those cracks occur due to ground motion events beginning at any time, but they really start to add up by 100,000 years, as a significant factor in the--giving a high probability of already having occurred by 100,000 years.

The second family of curves coming in, these are diffusive releases out of the CSNF packages, the TAD packages, that occur after you have general corrosion. These are essentially a nominal release, general corrosion has thinned the CSNF packages, and you’re getting cracks in the closure welds.

The third type of curve here is when you actually have large scale failure of patches due to general corrosion, and both types of waste packages develop those cracks at the same time. And, those up there actually turn out to be what dominate the dose at 1 million years.

GARRICK: Why do you call them stylized?

SWIFT: Because--why is this stylized? Cliff Hansen drew it with a magic marker. Do not confuse these with actual calculational results. That would be those up there, that’s all. Maybe there was a better word than stylized.

Cliff, did you actually use a magic marker? No. Okay.

KADAK: But, these charts show something interesting that I was trying to get at earlier, and that is even though the numbers are quite low in terms of dose, the codisposed packages provide early dose versus the commercial nuclear spent fuel packages. And, I’m wondering as a design question, could that be reduced if needed, to meet your targets?

SWIFT: Well, we don’t need to reduce it to meet our regulatory target. But, I think your implication is straightforward, that if the codisposed packages were structurally more robust, you would remove much of this blue curve.

KADAK: Right.

SWIFT: Next slide, please?

So, now, the total, and I--the black curve, let me talk about what radionuclides are driving that total. And, I went back to the overall total that I showed several slides back. This is not just the seismic case. This is all the modeling cases put together. So, it’s both seismic ground motion and igneous intrusion are what’s driving it.

The black line here is the same total mean that you saw in red on the horsetails earlier. And, what nuclides are actually driving that? At relatively early times, and I’m a geologist so I can actually with a straight face say that early is 200,000 years from now, for the first several hundred thousand years, the major contributors are Technetium in blue here that is a diffusive release, and comes out of--well, it comes out of both the--it also is transported advectively as well in the igneous intrusion case, but we’re getting a large fraction of it out diffusively in the codisposed seismic ground motion case.

But, we’re also getting a big slug here of--slug is a poor word--a large amount of--a small amount really of Plutonium 239 that is coming out from the igneous intrusion ground water case. These are--we’ve damaged all the packages. Most of this is coming out of the CSNF commercial spent fuel, and if you go back earlier and look at what was driving that hump in the total, it was igneous intrusion case, it’s in fact Plutonium 239. This is basically the decay curve of Plutonium 239 dropping off here.

At later times, the main contributors beyond that are Plutonium 242 here in red, and Iodine 129 in green, and Neptunium 237, which is the pinkish color here, and there’s a Radium 226 which underlies the Neptunium curve. The slight uptick here after 900,000 years, and these are mean curves, that is related to the additional fraction entering transport from general corrosion patch failures in the nominal scenario. But, the bulk of it back in here is driven by the igneous intrusion scenario.

Sorry to do that to you. I want to go back now to Slide 18. That was the major contributor, just to make that point again for that. For most of the history here, it’s igneous intrusion event that’s driving the total. But, the seismic ground motion event, which basically reflects the sort of expected evolution of the underground, is not far below it, several--order of magnitude below almost in there.

But, I will also note that these little spikes here, they’re prominent and somebody is going to ask about them, so I’ll bring them up right now, those are a result of a choice made in time stepping in the general corrosion model in Wapdag (phonetic). We had very long time steps, 200,000 year time steps, and any time you’re looking at model results and somebody shows you something that happens in 300,000 and 500,000 and 700,000 years, you might want to be suspicious. That is, we had 200,000 year time steps in the general corrosion model there. Essentially, the model is reporting accumulated thinning, but then allows stress corrosion cracking of welds, and makes it more susceptible to ground motion damage in the subsequent event.

If we had better resolution in those time steps--the eruptive models internal to the TSPA model, the rest of the TSPA model had much shorter time steps, on the order of thousands of years in there, but the Wapdag model had those long time steps, had it had better resolution, the model would have run more slowly, and the curve would have been smooth through there. But, I'm okay as long as I understand why those are like that.

But, going back now to Slide 21, you will notice that those spikes are very predictable straight through, and they show up in the species that are non-sorbing, have very high solubilities, things like chlorine and Technetium and Iodine. And, that's because of their rapid diffusion through cracked packages.

Next slide, please?

Now, I had promised I would say something about the stability of the total dose, and this is all I'm going to say about it, this one slide. But, basically, what we did was we resampled our input uncertainty, and replicated the analysis three times. So, each one is--essentially everything is the same in the analysis, except we had different random size of the uncertainty in the input parameters, and these results here are actually reported from what we call Rev 0 of the model rather than the addendum result. After having demonstrated stability of the Rev 0, we did not feel it necessary to go back and rerun the whole analysis three more times in the final model. They're very similar.

The point is that here, we show the mean, median, 95th and 5th percentiles of those horsetails overlain for three different samplings of the input uncertainty, and yes, visually, this is a good stable model result. You would expect the mean to be the most stable. The 5th and 95th have the highest potential to be unstable. They're quite stable. That's a good sign.

We constructed confidence intervals about the three means in here, show them there. And, again, we have good high confidence that--this is really just a statistical test of the model. What it tells you is that the model provides a good estimate of the true mean one would get from an infinite sample size of this specified uncertainty. We have high confidence the model is performing its task correctly internally, and that increasing the sample size would not change the results.

KADAK: Peter?

SWIFT: Yes.

KADAK: Kadak again. This is all interesting analyses. Now, can you describe for me at 800,000 years, what do the waste packages look like in your model? In other words, are they all dissolved, or what is the physical nature at that point in time of the waste packages?

SWIFT: The drip shield is gone, there will be remnants of it, but general corrosion has largely removed the drip shield. There will be a significant amount of rubble in the lithophysal zones, and the drifts will have a lot of rubble in them. The packages will have rock line on them. They will have been banged around by ground motion events. But, once we have significant rock fall on top of them, they will be--the motions will be much smaller, relatively pinned by the motions.

The outer Alloy 22 layer will be thinned several millimeters from what it initially was. The codisposed packages will have networks of stress corrosion cracks in them. The TAD packages will be much less likely to be cracked, but they will have cracks in them from the general corrosion, being exposed to the unannealed areas of the welds. The environment will be humid. There will be areas where there are drips falling out of the packages. There will be a small number of packages that have general corrosion that’s actually corroded through patch sized openings and allow water to flow into the packages, but that will be rare. Is that it?

KADAK: And, there will be some obviously dissolution and migration?

SWIFT: Yes. Once--well, as we model it, once a package is breached by cracking, there is sufficient transport of oxygen and water through those cracks into the internal of the package to degrade the single steel in the internals, to degrade the waste form, and to create a water film that allows diffusion outward. So, the interior of the package is humid and corroding, degraded.

KADAK: So, if you look at the time period, say, from after roughly 100,000 years out to a million, nothing much changes in terms of the engineered barrier from what you just described?

SWIFT: The most significant change, say, from 200,000 to a million years would be the increasing number of these patch failures that is still increasing at a million years. The nominal--it’s the nominal processes that matter have several hundred thousand years, I think. That was sort of the point of the--Cliff Hansen’s stylized decomposition that I showed earlier.

Now, this is in the absence of the igneous event. I think you weren’t asking about that.

Can I have the next slide?

Now, these are the parts of the talk here where I wanted to try to get to the Board specific questions about what assumptions we have made, what we believe about a few different things, about how water and radionuclides are moving in those two important modeling cases. And, again, I apologize, these are mostly words. I didn’t have pictures to show these. And, then, there’s a lot of--it’s fairly dense with information here.

Okay, first of all, water movement following ground motion, what do we think about how water moves in the drift after there’s been ground motion? First of all, I’ve got to say something about the nominal seepage model. This would be in a drift that has not been significantly degraded by ground motion events. The nominal model shows a range of--an uncertain range of locations that will show seepage, dripping. But, the mean of that, about 40 percent of waste package locations nominally, in the absence of ground motion, will see seepage in the TSPA model. And, I will have a slide to quantify the seepage coming up here.

Those seepage rates are depending on where you are in the thermal history in the first few thousand years are very low. As the repository approaches ambient temperatures, they rise to about 11 percent of the percolation flux. So, the capillary barrier is removing--is producing the water flux to about 11 percent of what’s moving through undisturbed rock in the nominal case. But, those are means. There’s uncertainty around those.

We do adjust that seepage model to account for rock fall accumulation. And, in particular, we built a separate, and this was done by a team at Berkeley Lab, built a separate seepage model for rubble-filled drifts. And, for a rubble-filled drift, as modeled in the seismic case, we get up to a mean of about 70 percent of waste package locations now see seepage, and the seepage rates are now up to a mean of 48 percent, almost half of the percolation flux. So, it’s quite a lot more water. This basically reflects the degradation of the capillary barrier through time. Keep in mind that the igneous case goes all the way to 100 percent of percolation flux.

Now, in lithophysal rock, that’s the rock that has the gas cavities in it from the cooling of the tuft, that’s about 85 percent of the emplacement area. We use the nominal seepage model, this one here, up until we have a rubble density--we actually have a drift degradation model that calculates how much rubble comes down from rock fall from ground motion events. When the rubble density is less than five cubic meters per meter in the tunnel, we use this model. When it’s above 60 cubic meters per meter, we use this model, and in between, we use a linear interpolation on the seepage flux.

For the non-lithophysal, which is about 15 percent of the emplacement area, our drift stability people indicate that the collapse events will be very rare, but will have larger blocks when it does happen. But, they are very rare, much rarer. And, we go directly to the percolation flux, essentially uncertainty in how to handle that. For that 15 percent of the repository, we go directly to a percolation flux when rubble is above half a cubic meter per meter.

GARRICK: Peter, for all of these scenarios or cases, what I call the funneling effect, is it even invoked? That is to say everything that comes into the drift ends up into the invert more or less?

SWIFT: Yes. The water into the drift does end up in the--and, go out of the drift. But, that water that is this number here, that water is diverted around. It rejoins the model effectively in the unsaturated zone and transport model below the drift, but it is not available for transport in the drift. But, yeah, the 11 percent, or whatever gets into the drift, ends up in the invert and goes out the bottom.

GARRICK: And, all of this is available for transporting through the UZ?

SWIFT: Yes.

GARRICK: Okay.

SWIFT: Next slide?

This is just a graphic that shows what I was talking about about how we adjust the seepage rate to account for rock fall. The curves shown on here, the black dotted curve is not seepage. On the left, it’s actually spatially averaged net infiltration over the repository footprint. On the right, it is the percolation flux. These are mean values. This is a log scale in time here now. And, just note that these steps here reflect--they should reflect the climate changes. They do. This step here, transitioned from infiltration to percolation flux, reflects the specification in the NRC’s proposed final rule, that we should, rather than attempting to specifically model climate change over a million years, we should use a specified constant percolation flux over the repository footprint, uncertain in magnitude, but you sample a value and then you hold it constant through time. That’s the mean of the NRC’s proposed specification there.

And, these are the actual seepage fluxes, mean seepage fluxes, expressed on a per package basis for packages that see seeps. And, at early times, there is no seepage while we’re above boiling there, and then some seepage in some parts of the repository, and then after we cool down. The red curve here would be the nominal value that I believe hopefully is 11 percent of the percolation flux from the slide previously. And, this increase through time, these are the actual calculated TSPA results, that reflects the result of accumulated rubble in the seismic case that degrades the capillary barrier.

KADAK: Just another question. Kadak again.

So, you’re saying on a per waste package, say at 1000 years, you’re going to get about 90 kilograms of water per waste package impending on that waste package; is that what you said?

SWIFT: Yes, per year.

KADAK: Per year.

SWIFT: Uh-huh. Does somebody want to comment more on that? Okay.

All right, more words, and I apologize. Now, that was seepage. Now, what about the flow of water through the engineered barriers, the drip shields and the waste packages? First of all, for the drip shields, we conclude that flow of water through any stress corrosion cracks that might form in drip shields is negligible. Those cracks are tight and do not support water flow, and we don’t care about the diffusion of water through the drip shield, since it’s essentially the same relative humidity on either side of it.

So, there is a discussion of that in our FEP, features, events, and processes, there’s an entire several thousand page document that describes the screening and the justification for screen decisions for individual FEPs. You would go to that FEP and see our discussion of why we believe that flow through cracks and drip shields is negligible.

But, after general corrosion thinning and failure due to the accumulated rock fall at approximately 300,000 years, it’s earlier than that, it’s slightly earlier than it would have been without the rock fall basically, and that was that figure of 270 to 340,000 earlier, the drip shield provides no flow barrier at all in the model.

For the waste packages, a series of statements and assumptions here. The diffusion of water through stress corrosion cracks in waste packages is assumed to be sufficient to degrade the single steel internal components. We have--I think Bob McKinnon could speak to that if we need to--we have looked at rates of water and oxygen diffusion through cracks, and we believe that yes, it will support that statement. But, as actually implemented in the TSPA, it’s an assumption.

Flow through waste packages occurs only when we have either general corrosion patch failures that don’t occur until 500,000 years and later, or the rare ruptures or punctures due to extreme ground motion events. We do include those, but they’re rare enough they’re not really a contributing factor.

Once you have a patch failure, the flow fraction entering the waste package is proportional to the ratio of the patch failure length to the waste package length. Conceptually, what we’ve done there is to place the patch failures along the crown of the waste package, and that is the effect of allowing all the available flow to enter a waste package after a very small fraction, a mean of 4 percent of the patches have failed. If you line all the packages up on the crown, and then put the water--drip the seeping water directly on the crown, you don’t need to fail all the waste packages before you’ve allowed all the water to flow in.

LATANISION: Ron Latanision. Just a point of clarification.

What does degrade in terms of stainless steel mean? What modes of corrosion are being considered?

SWIFT: The question was what’s the mode of corrosion for stainless steel?

LATANISION: Yes, what does that mean where it says degrade, what are the modes that are being considered?

SWIFT: I’ll let probably Neil Brown field that one. But, the--all I meant by that was that the rate of stainless steel corrosion is not limited by water or oxygen availability. Neil?

BROWN: Stainless steel would corrode both through stress corrosion cracking and general corrosion and perhaps localized corrosion.

LATANISION: Okay.

SWIFT: Flow is allowed to leave the waste package at the same rate that it enters. That was, to me, a simpler statement than saying that we assume that after the water flows in through a patch at the top, the patch then moves to the bottom to allow the water to get out. But, conceptually, that is what we do. We do not limit the rate of flow by assuming that the waste package becomes a bathtub and holds water, and then leaks out the bottom later.

We have, in the past, looked at that, and you can get small pulses of release by assuming that patches form in the top first, and then in the bottom. We didn’t really--that’s a secondary effect. We didn’t see any point of chasing that one further. We simply let the water go out at the same rate that it comes in.

KADAK: Kadak again.

With that assumption, how do you address this post-closure criticality issue?

SWIFT: That is done separately. And, they do consider the possibility of bathtubbing for that. A fair question, yes. This was an assumption made simply for the purposes of a system level transport calculation.

KADAK: And, does that system level transport calculation also take into account the fuel degradation in that process, or is that also a separate calculation? In other words, water to solution of the spent fuel.

SWIFT: Yes, we do include--I think I may have that coming up. I’m not sure. But, yes, we do actually model the degradation of both the spent fuel and the glass. We do not limit that by water and oxygen availability, but we do--it is not assumed to degrade instantly. The spent fuel lasts on the order of thousands of years, and the glass on the order of tens of thousands of years in a humid environment.

KADAK: Is that in a bathtub environment or is that in sort of a wet environment? In other words, I'm trying to figure out do you take, this assumption says the water just simply flows through the failed cask, or TAD, or whatever it is, waste package, is that assumption consistent with the analysis of the fuel degradation?

SWIFT: Conceptually, in this case, it's a humid environment. Is Dave Sassani here? Dave, do you want to field that one? Do we have a difference in our waste package degradation rates for saturated and humid environments?

SASSANI: Well, for the waste form degradation, there is separate cases for humid air versus aqueous environments. But, unless you're about 100 degrees, the differences are not that large. But, I think the next question is more about having a package full of water, and the waste being immersed, and maybe having a much lower oxidation rate under those conditions if you are in a saturated system. In the previous cases, they looked at--to those bathtub events form. They don't last very long, because you do get more than one hole in a package, and then you end up draining the package. So, you don't stay in that bathtub long enough for that to matter too much in terms of the degradation rates of the waste forms.

SWIFT: The last point I wanted to make here was that we do not modify our model for the invert for the seismic case. Flow in the invert remains the same.

I didn’t mention it here, but I hope I got this one right, we also treat condensation in the ground motion case the same. Condensation is really only a factor in the first few thousand years, though. Even then, it’s a minor factor.

Radionuclide transport following ground motion. We already covered some of these points here, but before you have patch failure, diffusion is the only release mechanism in the ground motion case, patch failure or rupture/puncture. We do assume this diffusion of water into the waste packages allows both waste form degradation, which Dave Sassani can talk more on, also we assume continuous water films exist on the waste form and the internals. There’s continuous water film pathway when relative humidity is above 95 percent, and that means temperatures have to be below boiling also.

Again, that’s probably a bounding assumption, but it will be--it’s humid in those environments, and there will be a water film on much of that. So, it’s not an unreasonable assumption to allow a water film--allow diffusion.

Because it’s diffusive pathway, it’s those high solubility, non-sorbing nuclides that dominate the dose, Technetium, Iodine, and a lesser inventory and lesser contribution would be Chlorine 36 behaving the same way. And, the rate of diffusion is controlled by the--it’s controlled diffusion in the cross-sectional area. In this case, it’s the cross-sectional area of the network of stress corrosion cracks, the path length and the concentration gradient.

After patch failure, you have seeping water that flows through the waste, and you have advective releases, and then you see the long-lived actinides dominating the dose, specifically Plutonium, Neptunium, et cetera. And, in that case, the things that matter are the water flux, how much seepage do you have, what are the solubility limits assigned to those actinides. Again, there’s uncertainty in all these factors. And, the sorption processes. For the sorbing species, the sorption processes in the waste package become potentially important. We do have a lot of corrosion products forming in the waste package, and we do allow nuclides to sorb on them.

Next slide, please?

Moving to igneous intrusion. For water movement, I mentioned this earlier, because of uncertainty in what a drift filled with magma and volcanic debris will look like, we simply removed the capillary barriers of the drift completely, and we used percolation flux to bound that uncertainty. And, we assume that the percolation flux seepage equivalent to it occurs at all waste package locations, so we go ahead and get them all wet. We allow water to re-enter the drift when temperatures drop below boiling. The reason I make this point is just to make it clear that in a--the main thermal effect in the repository, even following the igneous event, is radioactive decay heat from the waste. It's not the heat of the magmatic event.

In a cool repository, hundreds of thousands of years from now, if you filled it with magma, you'd get back down to ambient temperatures within a few hundred years. And, you drop below boiling very quickly. There just isn't that--the drifts are relatively small in volume compared to the mass of the mountain, and the heat of that magma will dissipate into the rock fairly quickly.

So, when we say we allow the water to re-enter when temperatures drop below boiling, in many cases, that essentially is immediately after the event once the repository has cooled down.

All water entering the drifts is assumed to reach the waste. Again, that's a bounding assumption. And, we use, for purposes of calculating that flow, we assume the magmatic materials have the same properties as the fractured tuff that's the host rock around the drift. No barrier to flow from the drip shield and the waste package. As I'll mention in a minute, we don't simply assume they're gone, though, it's just in terms of the flow properties, they have no role.

Again, we do not modify the invert, although one could argue something would have happened in the invert if it's buried in magma, but we chose to keep the invert the same.

Next slide, please?

Radionuclide transport. We assume both waste forms--well, actually, there are three waste forms, but all three waste forms, commercial spent nuclear fuel, DOE spent fuel, and high-level waste glass forms. We assume they are fully degraded by the high temperatures of the intrusion. I didn't mention the DOE spent fuels because we made the bounding assumption that for all cases, not just igneous intrusion, we allow that waste form to be degraded immediately. That's due to uncertainty in the condition of some of that waste.

Those are probably not terribly conservative assumptions. Once you've made the assumption that all those waste packages, all 11,000 plus of them, are going to see a magmatic temperature of 1100 degrees, if you make that assumption, it's probably not unreasonable to go ahead and assume that the waste will degrade, if not instantly, at least very rapidly at 1100 degrees. So, we do that.

For the purposes of calculating water chemistry inside the package, looking at radionuclide transport, the sorption processes, the materials of the waste package and their basic geometry remain intact. We don’t think the waste--the Alloy 22, the steel, the other materials in the package, are not going to be swept away and leave bare waste sitting in the tunnel. They will be there and they play their chemical roles.

We use the same water chemistry, and then this bullet here derives from that one, we use the same water chemistry model inside the package that we use in the nominal case. This is because that water chemistry is dominated by the waste form itself, and the steel and other materials inside the package, they’re still there. So, we use that water chemistry model.

And, the transport pathways are essentially the same from the waste to the invert and on into the unsaturated zone. They’re the same as we would use in the nominal case, or the seismic case, for patch failures. You have water flowing out of the waste, and waste being dissolved and mobilized in water and moved out. And, the contents of all the waste packages are available for advective transport.

KADAK: Just a quick question.

SWIFT: Yes.

KADAK: Let me see if I understand the scenario. The scenario is you have this igneous melted rock impinging on the waste package and the fuel that's inside, the spent fuel, and you're saying--what is going to happen to the fuel itself? Will it melt? And, if it does or does not, then the pathway is water?

SWIFT: Yes.

KADAK: And, you're assuming sometime in the future after this igneous stuff cools down, you will then have an apparently solidified rock embedded in--which embedded is the spent fuel, either deteriorated or not deteriorated, but the pathway is the same water pathway as you previously analyzed, with the same chemistry?

SWIFT: Yes. Now, conceptually, what do I think will really be there? I think that you will have remnants of packages, large pieces of metal, and not in very good shape after it's been hit by magma, but it's there, and there will be, the packages won't be uniformly filled with magma. I don't think you'll have the spent fuel actually encased in magma. I think it will be in--some of it will be looking surprisingly intact. But, it will have been heated to a point where certainly cladding will have failed, certainly. I wouldn't want to defend that it wouldn't have anyway. And, once the magma cools, it will fracture. You get cooling joints in it. So, it's difficult to argue that it will have hydrologic properties that will restrict water flow any more than the natural host rock would. So, that’s how we end up conceptually with percolation flux.

KADAK: The temperature of the magma is what?

SWIFT: On the order of 1100 degrees C.

The assumption, I think, in my mind that is worth thinking about there is that it flows through the entire repository. The magma will be cooling as it flows down drifts. I wouldn’t want to claim that our model is conservative with respect to the behavior of those waste packages closest to the point at which magma might flow in. It’s bounding, but it might be realistic for those packages. It might be quite conservative for those at the far end of the repository, which may never see magma at all. But, this is a case where further realism may be difficult to achieve in how magma behaves in the underground.

KADAK: But, you’re not taking credit for the magma as another barrier?

SWIFT: Correct. And, we have some geologists here on the Board, there will be cooling fractures in it. It will not be an unfractured medium after it cools.

All right, it’s 10 o’clock, and I’m on my summary slide. So, I apologize for using up whatever slack we had in the schedule.

GARRICK: Yes, you very successfully used all the--

speaking of margin--all the margin that Russ provided you.

SWIFT: We’ll hear more about margin from Cliff later. Maybe he’ll get us some back.

I think there may be nothing on this slide I haven’t already said. But, it’s worth reiterating. The total mean dose, and that is the one of the regulatory interest, determined by the occurrence of igneous--these are the major factors--determine is too strong a word. These are the largest factors influencing it. The processes associated with igneous events, particularly igneous intrusion--only igneous intrusion, seismic damage, and that will be ground motion damage, and general corrosion does indeed turn out to be an important contributor of the total as you approach a million years.

List of the major contributors there. And, for those who are comparing our calculated results to a regulatory standard, at 10,000 years, we’re at .24 millirem per year. The largest contributor is Technetium 99 from the codisposed waste. And, that’s a diffusion pathway from the ground motion modeling case.

At a million years, the medium, which is the proposed regulatory standard, is just under 1 millirem, and the mean is at roughly 2 millirem. And, again, the largest contributors here are long-lived actinides and Iodine coming from the diffusive pathways. Technetium would be there, but radioactive decay has reduced its importance at a million years.

And, the very last point on the page I mentioned a couple of times. By the time you get to a million years, it is indeed the nominal processes that are of most interest.

And, I think the--I’m done here. Do you want--

MOSLEH: Yes, thank you, Peter. I think we’d like to take a break now. And, then, after that, we have time for more questions and answers. So, let’s take like about 12 minutes of break. 10:15, we get together.

(Whereupon, a brief recess was taken.)

MOSLEH: We’d like to start. And, because of the fact that we interrupted Peter with many questions, I think we can shorten the question and answer session by 15 minutes. We had 45 minutes originally. Let’s see how that works, and we can catch up. All right, questions by the Board?

GARRICK: Peter, I have a couple of questions, and some of them you maybe have heard before. But, one of the questions I have is on the million year dose calculation, the last time we heard a discussion of this, it was pretty much a runout of the 20,000 year model to a million years. What’s different about this one, particularly between 20,000 years and a million years?

SWIFT: The model is still essentially a runout of the model we built for the 10,000 year analyses. That part is consistent with the NRC’s proposed rule-making, if you go through and read proposed 63, Part 342, how to treat features, events, and processes beyond 10,000 years. Basically, we do continue the same model on out. So, if a process was included in 10,000, it’s included for a million. If it was excludes, it remains excluded.

Was there a question about changes in the magnitude of dose from previous analyses?

GARRICK: No, it was primarily phenomenological, whether there were issues introduced as a result of the million year time duration, things that were peculiar to that calculation versus the 10,000.

SWIFT: On that one, we’re following the lead of the EPA and the NRC that--they have, in their proposed rules, chosen to limit speculation about processes that might become important over a very long period of time, or more important.

I’ll offer an example of one that--the processes of gradual erosion on the surface of the mountain. No, even over a million years, it’s not going to exhume the waste or move the mountain. But, the processes of erosion over a million years might change the upper boundary surface of our infiltration and UZ flow maps somewhat. We don’t consider that change. We hold those upper boundaries constant. That’s a good assumption for 10,000 years. Beyond that, it’s simply, we do not attempt to round off ridges, or deepen gullies over a million years.

GARRICK: One other--well, I’ve got a couple of other questions, but I may hold those off. At least let me get one more question.

There’s several ways of presenting the risk information. That’s for sure. And, the way you presented it is in a probability weighted fashion. And, I know that in the bowels of your analyses, you have the information to present it in different forms.

One form that would be very interesting would be to answer the question of what’s the risk, not the unweighted risk, but what’s the risk of a particular consequence, like 15 millirem per year? Is that something if we wanted to get CCDFs with consequences of variable, that’s something we could do without the weighting?

SWIFT: It is not something the NRC has asked for, so we don’t highlight it. Yes, actually, Jon Helton’s work has gone through that. Jon, do you have anything you’re going to show later this afternoon, or not?

HELTON: I do have a--well, first--Jon Helton here.

We, in Appendix J of the AMR, we show complimentary cumulative distribution functions for unweighted dose of the type you ask for. So, for the different modeling cases, the six modeling cases that Peter has been describing, we have the CCDFs that are actually reduced down to get the expected doses that you’ve been seeing.

GARRICK: I only got down to Section 6.

HELTON: Okay. I have one backup slide that shows CCDFs that we could pull up later on if you wanted to.

GARRICK: Okay, thank you. I have other questions, but I think I’ll pass.

LATANISION: Latanision, Board.

First of all, Peter, I just want to say I did appreciate that talk, very comprehensive, and actually very well done. So, my applause on that presentation. But--

SWIFT: Yeah, there’s always a but.

LATANISION: I do have one suggestion, and a couple of short questions. It just seems to me that it would be--and this may be a corollary to what John was just talking about. But, on all of the plots where you show the dose measurements or calculations, wouldn’t it be useful to show the dose at which there is concern about human health, just as a means of perspective? I mean, there’s a lot of mis-impression, misunderstanding, and I think it would just be a useful addition to something like that, maybe not necessarily for the NRC’s benefit, but for the public’s benefit, and maybe even for some political benefit. It’s just a suggestion. I don’t think I need to tell you folks how to--but, it’s a suggestion.

SWIFT: Okay, thank you.

GARRICK: Well, on that point, this is Garrick, of the Board. I think Ward Sproat, in his last talk with us, indicated there’s going to be a public document prepared for release, and that would be the ideal place for some of those kinds of comparisons.

SWIFT: The simplest would be noting where our estimates are, where the regulatory limits are, and where, say, natural background lies.

GARRICK: Yes.

SWIFT: That would be a useful comparison.

LATANISION: Yes, that’s my suggestion.

Two questions. If we could turn to Slide 20? Whether we call this stylized, or whatever, the message here is very clear, and it’s also troublesome, in a way, in that I don’t recall hearing a lot about codisposal packages in the past, not a whole lot. But, what fraction of the total waste package population, what fraction is codisposal packages?

SWIFT: Dave Sevugian or Cliff Hansen? Neil Brown has already jumped up.

BROWN: It’s approximately a third.

LATANISION: That’s stunning. I really had never--

BROWN: That’s by volume or number.

LATANISION: That’s certainly not by activity.

SWIFT: By activity, it’s quite a bit less.

LATANISION: Yes. But, I mean, it really does suggest that perhaps with some engineering, you could change that blue curve dramatically, if you were so inclined. Is that on the radar screen in terms of following up?

SWIFT: Not at this time, no. I mean, these are already low numbers.

LATANISION: Oh, I know that, but you’re going to show people this graph, and they’re going to recognize that you can do something about that. Why not? Okay, that’s the first question.

If we could next turn to Slide 9? The early failure modeling case, you know, we haven’t heard a lot about the experience at fabrication of the packages, the prototyping, and so on. So, in my mind, there’s always concern about how effectively welds can be made, the closure welds, for example, whether there are other sort of fabrication defects that might arise during the processing that we haven’t even thought about. And, so, I wonder how do we go from the necessity of a model and the probability assessments you are making here, to the reality of fabricating packages, recognizing that there may be, in that process, there may be issues that will arise that aren’t being considered, and which will make those numbers totally unrealistic, and obviously will have impact on the dose calculations. How do you deal with that?

SWIFT: Well, Neil Brown was, actually before he joined the lead lab, he was the lead of the Engineering Group at BSC that worked on the long-term performance issues. But, just the consequences, the risk, the probability weighted dose associated with the early failure cases will essentially scale linearly with the probability of the event. If you have more packages, more drip shields failing, those curves would rise up proportionately. So, you can see where they are now, and if you doubled the number of failures, so on.

Neil, do you want to comment on the question?

BROWN: From the waste package prototyping, we have completed the first waste package prototype. We took the waste package fabrication specification and fabrication sequence, went through standard human failure probabilities, considering probability of non-detection of flaws, et cetera, to arrive at these numbers. We compared that as well to industry failure rates, such as for spent nuclear fuel, pressure vessels, et cetera, and these values are right in line with those sorts of early failures.

And, when anybody looked at boiler and pressure vessels, you find that most of those failures are from operator failure, running at too high a temperature, running at too high a pressure, which aren’t really applicable to what we’re talking about here.

The other nuance I’ve put in here is we talked about the closure welds and probability of non-detection of flaws in those welds. That is not included in this place. It is included in the nominal scenario. So, weld flaws of the closure weld that are not detected are accounted for in the nominal scenario, not in the early failure scenario. So, this is flaw on the package everywhere except the closure weld.

LATANISION: Latanision, Board.

Just to come to closure on this. My point is, and I recognize what you're saying, Neil, but unlike, for example, igneous events where you clearly don't have any choice but to look at this in a probabilistic sense, in terms of these fabrication issues, you will get experience with fabrication as these packages are assembled. And, I'm just asking the simple question is there some thought that in addition to the modeling and the probabilistic approaches taken here, there will be some methodology incorporated into the plans to accommodate for what you learn during the actual fabrication?

BROWN: Yes, of course. We have ongoing work at Idaho National Labs on the closure weld system. You were there not that long ago.

LATANISION: Yeah, I wasn't, but the Board was.

BROWN: Okay, the Board was. Yes, okay.

LATANISION: Okay, all right, thank you.

ABKOWITZ: Abkowitz, Board.

I'd like to pick up on my colleague's comments here about this whole business of engineering this system, and it gets back to the question I raised with Russ Dyer earlier, which is it's all about engineering a system that has a certain degree of technology readiness associated with it, and I don't believe that what has been described for us to date, in terms of the design process, is anywhere close to showing the fidelity and technology readiness that allows you to be so definitive with some of these numbers.

I get back to the statement I made earlier about the way that EM is now adopting its definition of technology readiness, and in order to move forward with getting a positive CD-1 design decision, they need to demonstrate, that's a technology readiness Level 4, they need to demonstrate that the component and/or the system has been validated in a laboratory environment. Your system has not been validated in a laboratory environment. There is many components of it that have not even gotten off the drawing board yet. And, so, to have gotten a positive CD-1 decision, I don't know how that happened, but if it had happened the way that this protocol suggests, you wouldn't have to be in a position right now to try to guesstimate at this scale what these problems are. And, that, to me, is a very significant disconnect, and a recipe for dealing with some very difficult engineering failure problems downstream that I'm sure my colleague, Dr. Petroski, can elaborate on.

SWIFT: I’ll take that as a comment rather than a question, I think, and thank you.

The way in an analysis like this to accommodate that concern, and it’s a valid one and should be accommodated, is through the treatment of uncertainty. The question should not be are we certain that that is the right number for the probability of an early waste package failure. Rather, the question should be does our uncertainty in the probability of an early waste package failure span a reasonable range of belief. And, that’s a question which I welcome--I believe I heard your answer on that, and I appreciate it.

ABKOWITZ: Well, let me just ask one other question while I have the floor. If we could go to Slide Number 5?

If I understand this correctly, we have a variety of external process models that have been developed at various laboratories and with other contractors that feed into the GoldSim model, which then takes all this and grinds it along. Do those turquoise balls, or whatever color you want to call them, they’ve been essentially getting developed in parallel in different places; correct?

SWIFT: Correct.

ABKOWITZ: But, yet, they’re very interdependent on one another in terms of the assumptions that are made in one place, and how they’re picked up in another place. Could you comment on the quality control process, or the validation process that’s in place to be sure that there’s, for lack of a better term, a mass balance between all these different pieces that depend on one another, but were developed independently at the same time?

SWIFT: Sure. They have been developed independently, but through many iterations. Some of them are relatively new to the family. The MASSIF model for infiltration is the newest one, but the rest of them have been through this process quite a few times. So, yes, they’re developed independently. Yes, they are capable of running in essentially stand alone environments where the inputs and outputs are not necessarily linked to each other. But, for the purposes of this problem, there’s been good communication for quite a few years on linkage.

And, with respect to sort of literal mass balance, we don’t expect, nor do we require, mass balance between these models here on things like water flow. They’re balanced internally, but their purpose is to produce specific outputs that become inputs to the next model, that are feature, you know, the parameter of interest for that transfer. So, these are not--this is not a fully linked system of models over here.

Internal to the GoldSim model, we do have internal to any one modeling case, we have pretty good mass balance on radionuclides. We can talk more about that if you want to. We have quite good in EBS. It’s excellent mass balance on nuclides, essentially perfect. And, the water balance, with the exception of assumptions about the vapor phase content of water, which we don’t attempt to balance, the rest of it does balance.

DUQUETTE: Duquette, Board.

Several, I think, very short questions, and a couple I think you can’t answer, but I’ll try anyway. The first one is I was interested to see that the concept of the TAD was introduced into the TSPA, and the TAD is relatively new. It’s not brand new, but it’s relatively new, and you added it last year, I think.

I think this Board, or at least I was under the impression that the TSPA had sort of ossified, that it was frozen in space some time back. The reason I’m bringing it up is there has been discussion within the Board on whether or not we came up with a scenario that we thought was interesting, whether or not TSPA was viable to adding something new to it and getting a result reasonably quickly. What’s the turnaround time? I mean, obviously, you introduced the TAD last year and you have a result.

Now, how long did it take to introduce the TAD and then have a result on what its effect was on the release rate? Was it a month? Was it a day?

SWIFT: The time required to do a full run of TSPA, a full TSPA of, we call it 300 realizations, because there are many modeling cases, actually, there are many, many thousands, it’s on the order of several weeks. Actually, just of sort of analyst sit down and run the computers, and Cliff, you can correct me on this if I’m wrong on that, but I think that’s a reasonable number.

The rate limiting step on the TAD, introducing that, getting a conceptual design that all the various people who were players in this had to agree on was an appropriate basis to go forward with, and each of the various process model groups had to look at what would this introduction do to their particular piece of it, we’ve added a lot of steel, that changes the in-package chemistry model, and so on. So, it was on the order of several months to sort out which of the component models were impacted, what those impacts were likely to be. And, at that point, the TSPA people could start planning how to fit it all back together well in advance of knowing what the specific values might be.

We were producing our first system level test results about a year ago right now, with the TAD in them. So, then, getting the whole set--suite of documents through the review process, getting essentially qualified inputs for the analysis, several more months. But, ten months maybe, something like that might be the answer to your question.

DUQUETTE: I think so, which brings me to the next question. If you could bring up Slide 8, please? This is my ignorance of how--because I’m not a mathematician, and I understand what TSPA is supposed to do, but how did you arrive--there are several other slides like this one, but how did you arrive at these various scenarios? Is this a bunch of guys sitting around a table saying well, what do we think is the best scenario? Let’s take stress corrosion cracking common by 500,000 years. Why not 300,000 years or 800,000 years, or some other number, or something like that? I guess I’m trying to figure out how these assumptions are arrived at in order to put them into the TSPA model.

SWIFT: Okay. I probably should have been more careful in distinguishing what, on these slides, are assumptions, and what are model outcomes.

The stress corrosion cracking common by 500,000 years is a model result, not an assumption. We didn’t say, well, we think that’s when it’s going to happen. Instead, we said, well, it will happen after you’ve removed so much of the annealed surface above the closure welds, and then let the model calculate that.

But, the basic decision to come up with the six modeling cases, once we realized we had the major classes of destructive events, igneous and seismic activity in a nominal system, we need to account for rare but potentially significant early failures. Then, the modeling cases sort of fell out as computational convenience, how best to structure the analysis so we had good coverage, and all the things that might matter.

DUQUETTE: Thank you. I wasn't criticizing the way you had presented the data. I'm just trying to understand. I know you didn't have time to go into detail on that.

SWIFT: Right. But, most of the quantitative things on these slides are actually model results, not assumptions. So, when I say that a mean of 9 percent of waste packages show patch failures at a million years, that's the system level output. That's what happens when you convolve our general corrosion rate with the way we distributed patches on packages in the environment in the repository, that sort of thing.

DUQUETTE: Which links my two questions together to some extent. As you know, at least a couple of us on this Board are very interested in pursuing the concept of localized corrosion being a very important parameter in the lifetime of these canisters, or containers. And, I saw nothing in any of your presentation that addressed localized corrosion at all. I'm sure there are reasons for that, and I know that in the early period, it stepped out, but there's still nothing at all about localized corrosion.

SWIFT: Okay. I have a question for Ali. Then, I have three or four slides that are available as backups that I’m prepared to actually talk about localized corrosion in the TSPA, but it will take five minutes, ten minutes to go through them. Would you like that, or not?

MOSLEH: Go ahead.

SWIFT: That will be backup slide Number 33 in the file. Yes, we do have localized corrosion in the TSPA, and this slide, and these would be available, I don’t have them in the handouts, but I’m showing them and they’ll be available. On the drip shield, we do have screened out localized corrosion, and we go to our FEP analyses for both deliquescent salt environments and localized corrosion in seeping environments. On the Titanium drip shields, we have screened that out. I won’t say more about that, because I’m here to talk about the TSPA.

On the Alloy 22 surfaces, indeed, as the Board has seen before, and I’m not going to go into detail on that, we do screen out localized corrosion in dust deliquescent environments.

However, localized corrosion in seepage water actually is included in the TSPA, and if you go look at the FEP discussion for that, it will tell you yes, this one is included.

Next slide, please?

And, how do we actually include it? All right, the--we do calculate environmental conditions suitable for localized corrosion initiation on Alloy 22, and we do it using the TSPA model and a localized corrosion sub-model within it, and we take the drip shield out. We do it without a drip shield present. So, we allow seepage water to encounter Alloy 22. And, the actual localized corrosion model itself, and I can put the equations up if you want to see them, but it’s a function of temperature, pH, and the nitrate/chloride ratio, basically.

And, we do calculate those through the thermal hydrology and chemistry models for 3,264 nodes in the repository, and modelers like to say, and what we discover is that the potential for localized corrosion peaks in the first few hundred years when temperatures are high. And, the actual results, if there were not a drip shield--these, by the way, are documented in an appendix to the TSPA report, Appendix O--localized corrosion conditions could exist without a drip shield at approximately 10 percent of modeled waste package locations in the first few hundred years.

By 5,000 years, these conditions up here, temperature, pH and nitrate/chloride ratios, are no longer favorable for localized corrosion, and less than 1 percent of the waste packages--I’m sorry--less than a tenth of a percent of waste package locations still have initiation conditions.

And, by 12,000 years, as modeled, the potential for localized corrosion in Alloy 22 in seepage water is over.

Now, how do we actually implement that, recognizing that this was a relatively rare event in our TSPA model, that you would have conditions without a drip shield prior to 12,000 years, we didn’t bother to attempt to--once we had calculated this information, we did not go forward and then propagate that directly into the modeling cases. We did it by assumption. So, that’s where why in the drip shield early failure case, we took the bounding assumption that regardless of the actual environment, we just let localized corrosion occur. Drip shield early failure is quite rare, so, basically, only 10 percent of the location to see seeping should see localized corrosion. We made it 100 percent.

In seismic fault displacement, we’ve already broken the package by faulting. We concluded that was sufficient, did not go further to implement localized corrosion there.

In the igneous modeling case, which we could have igneous intrusion prior to 12,000 years, we did not--the waste package was already fully compromised.

Next slide, please?

And, this is just a figure from Appendix O, Figure O-2, that shows what I just said. We have broken it up by waste package type, and percolation sub-region. This basically is the wetter and drier portions of the repository, as modeled. And, there, at a couple hundred years, we just peak above 10 percent of the locations for the total that have the potential for showing unfavorable conditions that would initiate localized corrosion.

And, that, by the way, turns out to be driven by an outlier realization. Most of the realizations show this tail out here, going all the way out past 10,000 years, with a very small probability of localized corrosion. That’s driven by an outlier and most realizations show no localized corrosion at all after a few hundred years.

Next slide, please?

And, just so I can say it was up there, those are the assumptions we used in the localized corrosion initiation model, and the actual equations that I would have to ask Neil Brown to explain how we parameterized those.

DUQUETTE: Well, if you were one of my students, I would ask you to derive them, but I’m not going to do that.

SWIFT: I would then ask Neil to.

LATANISION: Well, I’d like to ask a question about this, but go ahead.

DUQUETTE: The last question I had is on Slide Number 14, and it’s a very short one. Doesn’t this imply that if you took the drip shield out, it wouldn’t make any difference? Because the drip shield is that red curve down there with super low rates, and so on and so forth?

SWIFT: Well, the drip shield is gone in all of our modeling cases by around 300,000 years. So, the main function of the drip shield then would be on the two slides back, where I noted that 10 percent of the waste packages in seeping environments, which is only 40 percent of the waste packages, might see localized corrosion if there were no drip shield.

DUQUETTE: That’s true. But, you take the drip shield out for that modeling anyway, right?

SWIFT: But, that model, in order to calculate the potential for localized corrosion, yes, we took the drip shield out. But, in the system level results that we show, the drip shield is still there. We do not initiate localized corrosion unless we had another event in the model that removed the drip shield, i.e. igneous, seismic, et cetera.

DUQUETTE: Thank you.

MOSLEH: Ron, do you have a question?

LATANISION: Yes. Latanision, Board.

If we could go back to the previous slide?

SWIFT: The equations?

LATANISION: Yes, the equations.

SWIFT: That was 36.

LATANISION: I want to make sure I understand. This refers to the entire period of time during the thermal transfer from zero to a thousand years out?

SWIFT: Yes. Maybe Dave Sevugian might be the person to ask. How long did we run the localized corrosion initiation model? Out to 100,000 years? Or, Cliff would know.

HANSEN: This is Cliff Hansen with Sandia. We run that analysis for a million years.

LATANISION: A million?

HANSEN: Yes.

LATANISION: The point that leaps out in the equation is the tremendous dependence on the nitrate/chloride ratio. And, we know, based on some of the work in fact that was reported at the last of the public meetings, and which is continuing at the USGS, that the question of the nitrate/chloride ratio seems to be right now very ambiguous in terms of what it’s telling us, and where it’s moving, based on dust analyses, based on other types of things that are being done. It just emphasizes the point that I think Dave was getting at that to FEP out localized corrosion, when we know there are processes occurring today, these don’t have to be modeled, we have data that shows what’s happening in some tests that are being performed that raises question about the concept of FEPing out localized corrosion, just based on that input.

SWIFT: Let me reiterate, we did not screen out localized corrosion. It is actually included in the TSPA for all modeling cases in which the drip shield is removed.

LATANISION: For the seepage instance?

SWIFT: Yes, where we have seepage striking the--it's included by the bounding assumption that it occurs for those cases where we have no drip shield.

LATANISION: But, my point is that you have FEPed out deliquescence, and this presumably would just as well impact deliquescence as it would seepage, this calculation, this formulation. Is that correct?

SWIFT: No, this is intended for seepage water. Neil, do you want to field that one?

LATANISION: Critical potentials don't know whether it's seepage water or deliquescence.

BROWN: Well, we've had detailed discussions on localized corrosion due to dust deliquescence in the past. And, yes, this equation does rely upon a nitrate/chloride ratio. But, as we said, that for seepage environments, we have--I'm sorry--for dust deliquescent environments, we have a multi-layer argument, including small quantities of chloride availability, the low water volumes where we're looking at, what, 18 microns, or so, of water in thickness, and salt dryout, and other similar arguments, and we're standing behind our argument that dust deliquescence does not occur, or will not result in localized corrosion.

LATANISION: Latanision, Board.

I'm not trying to revisit that argument. I'm simply making the observation that you have a formulaic expression here, which depends on the nitrate/chloride ratio, which is measurable, and, in fact, is being measured today, probably as we speak.

SWIFT: It is.

LATANISION: And, it’s raising doubt about some of the bases for excluding localized corrosion from the point of view of deliquescence. I would love to believe that you’re right, but you haven’t made a case.

BROWN: Just to, we have heard you, and the dust deliquescent work that we’re continuing to perform at Sandia National Labs, we are looking at salt assemblages with low or no nitrate, and, to date, we have not seen any in that environmental chamber that you went and visited. And, so, that work is continuing.

SWIFT: Also, I want to make a point. Look at the time scale on this, and note that essentially what we’re seeing here is the potential for aggressive conditions in which localized corrosion can initiate. It is also very dependent on temperature, as well as nitrate and chloride ratio. We do, in our model, have a drip shield that’s effective to--this is 10,000 years, 20,000 years--the drip shield is providing seepage protection out to 200,000 years.

So, the uncertainty in seepage water, as opposed to deliquescence, that you’re concerned about, there’s a margin on the order of 200,000 years here between the time at which--this curve would have to move 200,000 years to the right before we would see localized corrosion. The temperature term is--

LATANISION: I understand. I'll buy that.

SWIFT: Okay.

LATANISION: Thank you.

MOSLEH: Andy?

KADAK: Kadak.

I have just a few questions. One of the things I think John was trying to get at was the difference in these results from results of the TSPA, say, two or three years ago. They're significantly lower, probably by an order of magnitude. And, what I'd like to try to get a feel for is why the changes--what changes occurred in you modeling or understanding that allowed you to feel comfortable about such low numbers? That's the number one question.

SWIFT: Can we have Slide 13? I wanted to put up some results. In past Pas, we have shown doses that were on the order of tens, to in some cases over a hundred millirems per years, out beyond several hundred thousand years. The differences here, one that gets mentioned relatively infrequently, but it's worth noting, the ICRP, International Commission on Radiological Protection, revised its dose conversion factors, and the dose conversion factors actually went up a little bit for Technetium and down for most of the actinides. And, that produced an effect on the total. It’s a factor of perhaps four to five.

We were aware of this at the time of the EIS in 2002, but at that time, the EPA and the NRC were still requesting the older dose conversion factors. And, in the proposed rules now, the U.S. standard have gone to the current international standards.

Moving from--let’s say we moved from 50 millirem per year, or 60 millirem per year, to two, a factor of five is important there. The other most significant change here is that in previous analyses, we did have most waste packages showing patch failures well prior to a million years. Now, we’re seeing a small fraction of them. We believe we have better understanding of the general corrosion rates that allow us to say with confidence that most waste packages are going to be intact, cracked, but intact at a million years. So, we see a lower contribution from the advective release.

KADAK: So, this is the corrosion question again, or what?

SWIFT: General corrosion.

KADAK: General corrosion.

SWIFT: Yes, not localized, general.

KADAK: So, those are the two big differences?

SWIFT: Dave or Cliff? Dave Sevugian, do you want to add something to that? There are many, many other smaller differences, but those are my two first order ones.

SEVUGIAN: Dave Sevugian.

Maybe from the last results you saw, the new TAD design also added a lot of strength to the CSNF package. Before doses were, you know, dominated by the inventory in the CSNF, that has been reduced considerably with the addition of the TAD. So, that’s another factor.

SWIFT: Neil Brown?

BROWN: We also revised the criterion for stress corrosion cracking. That number has been, as we’ve got more data on lack of seeing stress corrosion cracking on Alloy 22, we have raised that threshold upwards, which has reduced the quantity of damage on both types of waste packages.

SWIFT: I’ll add one more comment to that, too, that if you go all the way back to 2002, which those were actually the last full published results that appeared in the Environmental Impact Statement, 2002, at that time, we were simulating the major future climate change associated with full glaciation as if there were no uncertainty in the time of their occurrence. And, that produced sort of a sawtooth spikiness to the results we published at that time. And, it’s those spikes that went up over 100 millirem per year in 2002. That was probably not realistic to imagine that we know the times of the future glacial cycles precisely. But, nonetheless, we went with that assumption, and were we to do that again, we would have included uncertainty in the time of the event.

However, the EPA and the NRC took that question off the table by specifying that we use a long-term average climate, essentially acknowledging that was a very difficult uncertainty to work with in regulatory space.

MURPHY: Bill Murphy, Board.

You addressed some of my questions in response to Andy’s question just a moment ago, but I’m also quite interested in the very long-term, and specifically, the effects of the assumptions like the constant climate and the assumption like the constant dilution volume at the accessible environment, and whether or not one can predict when glaciations will occur. They will occur at a precise time, and won’t there be a spike associated with that, or could there be a spike associated with that? And, have sensitivity studies been done to evaluate the consequences of those very long-term assumptions?

SWIFT: The answer to your last question, sensitivity analyses have been done to evaluate those consequences, not specifically so to this analysis. This analysis is done to address the regulatory requirements that actually are fairly prescriptive, proposed ones anyway, with respect to how we treat the period between 10,000 and a million years.

However, you can look back at past analyses, and, yes, a dramatic glacial climate change, if it occurs--it will occur at some time in the future, it won’t be instantaneous, although we did used to model it that way. But, it will be relatively rapid compared to long time steps in the model, and it’s probably not unreasonable to think that there will be a fairly rapid response in the model.

In that case, though, it would look much like, although on a much smaller amplitude, much like those igneous eruptions that I showed earlier, where you would treat it as an aleatory uncertainty, and the expected value over that would be, the event could happen at almost any time, and the mean would end up actually being smooth again. But, you would want to display the consequences of the--the conditional consequences if it happened at 128,000 years from now. Yes, there would be a small spike due to increased ground water flow and rising water table. And, in fact, that is what we did in the 2002 analyses.

MURPHY: I have one other question concerning Slide 21. I’ve been very curious since Radium 226 has started to appear at the end of your dose calculations, and I presume that it’s in-growth from decay, from Uranium 238, and possibly Thorium 230, or other--

SWIFT: It’s Uranium 234, it’s in that decay chain, I believe. Somebody correct me if I got that wrong. But, yes, correct, Radium should not transport freely itself, and the Radium dose should come from what grew in near the end of the physical transport path. And, at the decay chain, it’s Uranium, Thorium, and then Radium.

MURPHY: And, my followup question in my own mind is what about radon as the decay product of radium? In that full system, where you have a lot of Uranium, radon is the predominant ionizing radiation dose that people experience, and has the radon hazard been evaluated?

SWIFT: In this relative to background?

MURPHY: Well, yes, in some respects, this reflects how low these doses really are.

SWIFT: Yes, there would be probably two radon pathways of interest. I wish I had Marilyn Lasiolak here to answer the question better. She’s the person who did the dose pathway work for us. But, one would be the atmospheric pathway directly up from the repository and out, and radon decay during diffusion through hundreds of meters of rock will knock most of that down. And, then, you end up with a very small radon anomaly at the surface than is much dispersed in the air before it ever reaches a human dose point.

The more interesting pathway would then be the exposure from ground water itself, the radon entering homes through water, just the dose you get in the shower, that sort of thing. I believe that one is actually included. That’s why I wish I had Marilyn here for that.

Does anyone know the answer to that? Bob, do you know? Bob Andrews? I’ll look it up for you. I apologize.

MURPHY: Thank you.

ARNOLD: First, I wanted to comment on the discussion between Mark and Russ. I think I would characterize the whole design of the waste package as a conceptual design only. The TAD, for example, is on its--now contractually on a path to get two separate specific designs. But, as I see it, the whole waste package is simply a conceptual design.

The other thing I wanted to mention, or ask you about, you mentioned in passing failure of the clad at something like a thousand years, and the fuel itself--could you expand on that a little bit?

SWIFT: The clad in the seismic ground motion case, the clad does fail from, as modeled, from ground motion shaking. And, that happens at an annual frequency that is slightly above the frequency at which you produce cracking of the package, or produce conditions that lead to cracking of the package. So, effectively, the cladding is not there as a barrier in the seismic ground motion case.

ARNOLD: Are you using the real data to get that seismic failure?

SWIFT: I believe yes is the answer to that. These would be from conventional drop tests and real fuel rods, I believe. Obviously, we don’t have data on how fuel rods behave in ground motion at Yucca Mountain, but I believe those are based on real data from real accelerations of clad.

Neil Brown, are you the person to answer that? Dave Sevugian maybe?

BROWN: At one time, we did explicitly model the accelerations and the probability of failure of the cladding, which was based upon some work that was done for transportation analysis. But, we got to the point where the cladding was unlikely to help us because of the issue of it failing during seismic and igneous, of course, it would fail, so, we don’t take any credit for the cladding as a barrier. We recognize that it is a barrier, but we do not take credit for it in any of our scenarios. But, the data that led us to that was modeling similar to that done for transportation with some actual experiments.

ARNOLD: It doesn’t just disappear, though. You know, you end up with maybe a crack in it, or something like that?

BROWN: Right. But, we do not take credit for that preventing water from impinging upon the fuel.

SWIFT: It’s a simplification that bounds the problem probably without introducing much additional conservatism. There’s no impact on the overall total, if you believe that cladding will fail from ground motion and igneous activity anyway.

ARNOLD: Suppose you believed it didn’t?

SWIFT: Then, one would have to develop a basis for that belief. We don’t have one.

BROWN: This is Neil Brown again. When we did take credit for cladding, we were using essentially failure rate of about .1 percent of the fuel being failed at receipt of the repository, and about 1 percent as a result of the stainless steel cladding. And, so, if you take that, you would say okay, you could lower the doses in all except the igneous case by somewhere around an order of magnitude.

CERLING: Cerling, Board.

Two questions, and they can probably be dealt with fairly quickly. But, one has to do with the overall infiltration that you are using in this, because several years ago, we were using the infiltration estimates based on the USGS, and then that was reevaluated, and I’m just wondering whether or not the infiltration that you’re using in this model is essentially the same as you were some years ago, which was on the order of the present day scenario of on the order of 4 millimeters a year?

SWIFT: The new results allow for a sort of a lower probability, a lower weight assigned to considerably higher infiltrations than what we saw before. The MASSIF results extend the range of uncertainty in spatially averaged infiltration to much higher levels. However, there are lower weights assigned to those in the PA. So, we can’t share those effects--

CERLING: So, it’s essentially--I mean, it’s essentially the same as the previous?

Swift: No, overall, it’s a little higher. Ming Zhu, are you there? Ming is the person who can field that question. Ming is one of our leads.

ZHU: You are right. For the present day climate, the mean infiltration estimated was 13 millimeters per year. But, after the weighting factor is applied to the whole distribution, I don’t have the specific number, it’s a slightly bit higher than 5 millimeter per year, than, you know, the number that was used before.

SWIFT: Okay. I would also point out that after 10,000 years, we don’t use the spatially average infiltration. Instead, we use the NRC’s prescribed, proposed prescribed rule for a log uniform distribution, the percolation flux from 13 to 64 millimeters per year.

CERLING: And, then, second, and I’m glad Andy had asked that question about the changes in the results of I guess Slide 13, if we could go to that?

Now, as I understood what you said is that part of it was based on a better, or let’s just say a change in the way that you did corrosion modeling, and there was a change in engineering, which was kind of a TAD, and then the other important one, which came up, was the rule change in which things were converted to the dose.

So, my question is if the original guidelines by EPA, and so on, were based on the former rules, would you want to speculate on whether or not their limit will change based on the new rules, or the new method for calculating dose?

SWIFT: No. I can’t speculate on the EPA rule. I don’t have any insight at all on that. What I know is what was published in 2005.

CERLING: Thank you.

MOSLEH: John?

GARRICK: Just a couple of very specific and hopefully short ones. If you look at the dose at a million years, what fraction of the waste packages are contributing to that dose? And, can you resolve that between cracks and patches?

SWIFT: At what time? I’m sorry.

GARRICK: At the million years.

SWIFT: At the million year mark? Well, you have to mentally take out the igneous intrusion case, because that’s 100 percent there. But, take that one out of it, and--the number was in there, about 9 percent of the packages have patch failures, and that’s evenly split between the TAD and the codisposed, I believe, or split according to their abundance in the repository. And, Cliff, by then most of the packages, a large majority are showing stress corrosion cracking. Is that correct?

GARRICK: What?

SWIFT: I'm asking Cliff to answer the question here about stress corrosion cracking.

HANSEN: Cliff Hansen.

Yes, the fraction of packages that have cracks is going to be uncertain among these realization, because it depends pretty strongly on the rate of general corrosion. But, the mean of that fraction would be on the order of 60 percent.

GARRICK: 60 percent on the cracks, and 9 percent on the patches?

HANSEN: Yes.

GARRICK: And, one other thing, just maybe a comment. On this issue of things like localized corrosion due to dust deliquescence, or even the last thing where we were talking about cladding, given your high level of skill with respect to dealing with--embracing issues probabilistically, wouldn't that be, based on the supporting evidence, wouldn't that be a way to go on these cases where you're being challenged with respect to the lack of compelling evidence? It sounds like such a simple approach. If it doesn't make any difference, then you've got what--it's there, it's visible, and we know what the result is.

SWIFT: Thank you for the comment. Yes, of course, if you can show something doesn’t matter, it would be good to do so. In the case of deliquescent localized corrosion, we do believe we have a strong case for saying it will not occur, and needs no further analysis. But, your point is well taken.

GARRICK: Cladding is an excellent example, especially for the near-term doses. It isn’t going to just disappear. It’s going to be--

ARNOLD: Yes, and I would point out that the drop tests occur--when fuel assemblies are dropped, they’re dropped in an open space, and the pellets can spread out. If it’s entrapped in a TAD, it just rattles, I’m not sure much of anything happens.

MOSLEH: I think there was a question by Dave. Go ahead.

DIODATO: Yes, thank you. Diodato, Staff.

Peter, thank you for the presentation. I’ll try to be brief and just focus on one point. I mean, first of all, the TSPA is an impressive piece of work produced by a large group of talented individuals, and they’re to be commended for that.

What it says to me, it provides stark testimony to the triumph of really engineered systems over nature, basically. You have very robust systems there. You have a waste package, a drip shield, the drift tunnel. I mean, when I look at the TSPA analyses, it seems like the drift tunnel is half of all and stay open longer than 200,000 years. So, that’s impressive, to me.

So, the question I asked myself is really where does the DOE’s confidence in these systems come from, the engineering systems? And, so, for example, if I look at Slide 24, just to try to understand this slide, the lack of change in seepage prior to 10,000 years, is that because the drip shield is still there? Do you see how for 10,000 years, the green and the red curve track each other, and then beyond that, are they both the same source of seepage?

SWIFT: Yes, the green and the red curves here, this is seepage water that enters the drift. If the drip shield is there, it strikes the drip shield. If the drip shield is not there, it strikes the waste package. But, this is independent of the engineered system, except for the fact that we have a tunnel itself. The small climb of the green curve here reflects some rubble accumulation due to ground motion events prior to 10,000 years. The long--remember, this is a log time scale--the long, slow accumulation is just more and more ground motion events, probably have more rubble. The step function here is the switch from an infiltration model to a prescribed percolation flux model.

DIODATO: Okay. So, this reflects maybe thermal constraints on seepage at the beginning?

SWIFT: This is the thermal period down here, yes.

DIODATO: Okay. But, you still do have some seepage during that thermal period in some areas in the repository?

SWIFT: Yes, cooler areas.

DIODATO: All right, so, that’s interesting. Can we go to Slide 11? My question here is you talk about the seismic ground motion, and the frequency of events that damage the codisposal packages is 10 to the minus 5, and then 10 to the minus 8 for the spent nuclear fuel. And, you have a peak ground velocity I guess on the order of 4 meters per second in these calculations. And, there is now some suggestion that maybe these peak ground velocities might be non-conservative and there could be reasons to believe that the peak ground velocities might be higher, maybe as much as twice as much. So, has the Department kind of assessed the impact of the higher peak ground velocities on the frequency of these damaging events?

SWIFT: The damage tends to come from--that the risk associated with this damage tends to come from the more frequent but lower peak velocities, rather than the rare extreme ones. It’s a function of the probability and consequence argument there. But, I’m going to ask Dave Sevugian to talk directly to that one. Dave, are you prepared on that one? Did you follow the question? I’m not going to ask Dave Sevugian to say one word about this.

SEVUGIAN: We’re not aware of new information you’re talking about. Recall, we bounded the hazard curve based on site data. We had the unbounded hazard curve, and strength of the rock at the site, data that we should bound the hazard curve and rescale it. So, are you talking about something we don’t know about?

DIODATO: There’s discussions that are occurring at extreme ground motion--

SWIFT: Yeah, my point was that when we--originally now, this is unpublished work maybe three years ago, we were looking at unbounded ground motion curves where we had extreme motions associated with extrapolation of the expert elicitation beyond the range for which they had intended it to be used. And, what we saw was that those extreme ground motions extrapolated to very low annual frequencies, were not driving the total risk. The total risk was coming out of motions that seemed reasonable and were occurring at higher--they were modest motions occurring at higher annual frequencies.

GARRICK: Yes, and that’s a very typical process that takes place in risk assessments. For example, in the risk of Category 5 hurricanes at a particular location, you will find that it isn’t the Category 5 hurricane that’s the principal risk, it’s usually the frequency associated with the Category 3s and the Category 4s. This is a pattern that manifests itself in most risk assessments.

SEVUGIAN: Yes, and even when we had the unbounded hazard curve, we deconvolve the dose results, and showed exactly what Dr. Garrick is talking about. Most of the risk is from the more frequent events, smaller events.

DIODATO: Yes, that’s interesting to note. Sticking with the seismic, I just have one more question on that, and that’s on Slide 12. The seismic fault displacement modeling case. Over the years, and you mentioned that there’s an offset from the Solitario Canyon Fault, so we’re trying to peel the onion on that and just kind of get some statement on the record about fault offset, because there has been a dogma at least over the years that there is some specific offset from faults, and yet when our consultant tried to push on that a little bit and find out what is this fault offset specification, the information he got back was that that didn’t exist.

So, what I’m trying to find out is what is your understanding of this fault offset criteria for waste emplacement in the repository?

SWIFT: Given that--somebody here--Dave is up. Bob Andrews from BSC might want to comment. This is an operational issue that is sort of an initial condition transfer to the--

MOSLEH: If we could make this a short response, because we’re running out of time?

SWIFT: Yes, we’re out of time.

SEVUGIAN: We sent a response back to Leon on that, and there’s a criterion for offset of quaternary faults, which I don’t think there are any in the repository. Somebody can correct me. The other ones, there is no criterion for offset, except for Navy packages. It’s just that the dose, you know, you saw the probability is--

DIODATO: Right.

SEVUGIAN: So, the effect of not offsetting is low.

DIODATO: Okay. I just wanted to be clear on that. So, there is no offset criterion.

SEVUGIAN: Except for Navy.

DIODATO: For the Navy fuel, yes.

SWIFT: Well, and if the footprint doesn’t extend over the Solitario Canyon Fault. That’s an offset.

MOSLEH: With that, I’d like to thank you very much.

SWIFT: I apologize for running long. I’d like to apologize.

MOSLEH: And, it’s time to move onto the next presentation by Dr. Cliff Hansen on the Performance Margin Analysis.

HANSEN: Good morning, Gentlemen. I’m Cliff Hansen with

Sandia National Labs. I’m here to present you a summary of a Performance Margin Analysis that was conducted using the TSPA model.

The purpose of this analysis is to quantify the effect of some of the conservatisms that we used in developing the TSPA model. Judicious use of conservatisms is appropriate when you’re modeling a complex system. I’m not trying to give the Board a lecture here, just explain some of our rationale. We use conservatisms on occasion to simplify the model for purposes of reducing its complexity, to improve its computational efficiency, and we also use conservatisms where the data, frankly, doesn’t support a more detailed treatment. In many cases, the conservatism replaces or substitutes for an explicit treatment of uncertainty in a matter in which we feel we won’t under-estimate the performance of the system.

We do not, in this analysis, address all the assumptions and simplifications in the model that could be viewed as conservative. Rather, when we planned this analysis, we took a risk-informed approach, selected a set of conservatisms for which we felt had the potential to effect the system performance results, for which we had some basis for proposing an alternative treatment.

You will find that all this is documented in an appendix to the TSPA AMR. And, we feel that the inclusion of this analysis with the TSPA model results should enhance confidence in the compliance case.

Next slide, please?

So, an outline of my presentation. What I will not do is I will not walk through the conservatisms and our basis for an alternative treatment in any exhaustive detail. Rather, and thankfully in the interest of time, I chose a bottom up approach. I will show you the results of the analysis, and use those results to identify and point you to a few of the conservative treatments that were changed, and the changes to which had an effect on the system results.

I should note that I’m going to focus primarily on the 10,000 year calculations. I’ve got a few slides at the end to show you the effect on the million year. The conclusions will be similar. And, that these results of the Performance Margin Analysis, or PMA, are compared to this Version 5.00 results from the TSPA model. The 5.00 results are reported in the draft Supplemental Environmental Impact Statement and in Volume I through III of the TSPA AMR. The final results from the TSPA model are from Version 5.005. Those are documented in the addendum to the TSPA AMR, and as Peter pointed out, are the results that are used--proposed to compare for compliance.

Next slide, please?

A list of the areas of the TSPA that were addressed by changes in the Performance Margin Analysis. Like I said, I won’t go through these and describe in any detail what those changes are. You can read about them in the Appendix C. I can answer some questions about them. But, to steal my own thunder, the conclusions will be that it’s these last two sets that have the largest effects on the performance of the system, treatment of fractured matrix diffusion in the unsaturated zone, the treatment of heterogeneity in colloid transport in the saturated zone, and then the modeling of the occurrence of damage from seismic events. And, that’s primarily, you track that back to representing the material properties of Alloy 22.

Next slide, please?

So, here are the bottom line results for the TSPA model from Version 5, Rev 05. If you recall from Peter’s charts, this looks very similar to what you saw Peter present. At the end of the presentation, I’ve got a chart to compare Version 5, Rev 00 and Version 5, Rev 05 of TSPA with the PMA and show that the conclusions don’t matter which version of the TSPA model you use.

On the left, are the expected dose horsetails from the TSPA model. On the right, the expected dose horsetails from the Performance Margin Analysis. Generally, by replacing these conservatisms with alternative treatments, you can observe about an order of magnitude reduction in the mean. Really, no change in the overall uncertainty about that mean.

There is a little bit of difference in the timing at which the dose increases to these values. I'm not going to discuss that further. I only wanted to note it for your interest.

KADAK: So, this includes the igneous stuff?

HANSEN: Yes, this is the total, including all the modeling cases. To explain what changes caused this reduction in the total dose, I'm going to go ahead and decompose these totals into the contribution by each modeling case, and then for the modeling cases that are making contribution, I'll decompose that further by radionuclide.

Next slide, please?

So, here's the decomposition of the PMA and the TSPA model by modeling case. As you saw from Peter's presentation, in the TSPA model, it's the green dashed is the seismic ground motion. And, these are just the means. This yellow covered line is the contribution from igneous intrusion, and the other cases make a minor contribution to the total.

In the PMA analysis, the contribution from the igneous intrusion is the dominant source of the mean dose, and it's similar but somewhat reduced from the TSPA model. The seismic ground motion is greatly reduced in the PMA, and as you will see on a later slide, that’s due to the change in the way we represent the material properties of the Alloy 22 barrier.

Next slide, please?

Just for clarity, I pulled the means of these two cases for the TSPA, those are the red lines, and the PMA, which are the black lines, onto two separate charts so that you could see them without the addition of the other modeling cases.

KADAK: Just a clarifying question? It seems like the--what are you assuming about the frequency of igneous events that apparently has no real impact on the change in the more realistic model? That’s surprising.

HANSEN: If we back up to Slide 5, the frequency of igneous events is uncertain. It’s sampled from the distribution.

KADAK: Is that a realistic--I mean, within 10,000 years, is there a credible expectation that there will be an igneous event in this area?

HANSEN: Credible or probable? I mean, the occurrence of igneous events is modeled with a distribution--

KADAK: Well, I’m--

HANSEN: --so, there’s an annual frequency at which they occur. It’s just very low probability.

KADAK: Is there data for that? Where are you getting this number?

HANSEN: Peter, would you like to take that question for me?

SWIFT: Sure. This is Peter Swift, Sandia.

The probability of an igneous event does come from expert elicitation done back in the 1990s, with a panel of about a dozen, or so, mostly volcanologists, but at simplest, imagine drawing a circle around Yucca Mountain, counting the volcanoes, volcanic centers, determining their age, and developing an aerial frequency for the area inside the circle you drew, and we had experts do this in tremendous detail, and you end up with a fairly robust answer that the annual frequency is somewhere between 10 to the minus 10, and 10 to the minus 7 per year, three orders of magnitude.

HANSEN: We did not change that treatment in the Performance Margin Analysis.

KADAK: What I’m surprised at is looking at the, given those frequencies, between 2,000 years, they’re expecting apparently events.

HANSEN: There is a probability that those events will occur. Because these are expected values, that probability is accounted for when you calculate these expectations. So, the uncertainty in the occurrence of the igneous events is primarily driving this broad uncertainty in the total dose. That’s the point I wanted to make by backtracking to this slide. The change in the magnitude between these two models is determined by other changes we made in the transport from the events.

KADAK: But, it appears your igneous still dominates pretty much everything, and it doesn’t affect your Performance Margin Analysis.

HANSEN: That’s true.

KADAK: The frequency of the event that you’ve assumed, and its consequences.

HANSEN: We did not alter all the component models that could be viewed as conservative. One of those is the extent of damage if an intrusion occurs, and the other could be that, you know, the frequency of the igneous events themselves. So, those were unchanged.

GARRICK: These are very, very small--

HANSEN: Can you go forward to Slide 7, please?

GARRICK: Dominance doesn’t have a heck of a lot of meaning when you--

HANSEN: We’re still estimating mean values on the order of .1 to .01 millirem in 10,000 years. Just for your convenience, I pulled out those individual means so you could see the magnitude of the effect on the seismic case and the igneous intrusion.

Next slide, please?

To look at the seismic modeling case, in particular, the chart on the left shows the contribution to the overall mean from each radionuclide, and the chart on the right for the PMA, similar. You can see that this heavy blue line is the contribution to the mean from Technetium, the heavy green is Iodine. This dashed pink is the contribution from Carbon 14, which the transport of Carbon 14 is modeled as if it was all in aqueous, not any gas phase at all, which is, in itself, highly conservative, but we didn’t alter it because we didn’t have much of a basis to propose a better model at this point.

You can see that the PMA dose, although it’s greatly reduced, is still being determined by those same radionuclides. And, the discharge scale that I chose to make it easy to compare the left to right, doesn’t show these other contributions, which are Plutonium and Neptunium. They’re present in the PMA dose, but pretty far down off the scale, about the same relative contribution as you see in the TSPA results.

This change in the magnitude of the total from the TSPA to the PMA can be traced back through the analysis and attributed to the change in the way that we modeled the residual stress threshold in the Alloy 22 outer barrier. This stress threshold, once it’s exceeded, allows the stress corrosion cracking to occur. And, if you go to the next slide, please?

This chart on the lower left is an illustration of the relationship between residual stress threshold and the probability that seismic events would have caused damage to waste packages. In this case, it’s the intact codisposed waste packages within 10,000 years.

There’s some mechanistic models that model vibrations from ground movements in packs of waste packages on the pallets, the drip shields, et cetera. And, from that suite of models, we estimate this kind of a distribution for the occurrence of seismic damage. And, you can observe the very strong dependence on that residual stress threshold.

In the TSPA model, this full range from 90 to 105 percent of the yield strength is used. In the PMA, it was truncated at 100 percent to 105 percent. So, you’re seeing only this tail part. This is relevant to some of the questions that were asked of Peter about the first 200,000 years. It looks like most of the risk comes from the damage from seismic events to codisposed waste packages, and the subsequent releases.

And, the suggestion was made that perhaps better engineering could reduce that risk. I would suggest that much of that risk probably results from what some view as a very conservative treatment of the engineered design that’s already been proposed. So, this would be an alternate path to explore if you were interested in reducing that risk further.

Next slide, please?

This is the results from the igneous intrusion case for 10,000 years. The same chart showing the contribution by radionuclide to the total. In the TSPA model, you observe that the total is determined primarily by Technetium, which is the heavy blue line, and then the dashed orange is Plutonium 239. These blue dots are Plutonium 240, and the green is Iodine again.

In the PMA analysis, the total is determined by just the Technetium and Iodine, and you will observe the Technetium is somewhat reduced. Comparing the contributions from Plutonium, you know, one of the changes has made a marked reduction in the contribution of Plutonium following the intrusion.

The reduction in Technetium, I will discuss first. On the next slide, is due to the introduction of--or accounting for the possibility of reducing environments within the waters in the saturated zone. The reduction in Plutonium is accounted for by our treatment of the matrix diffusion within the UZ, as well as the change to the colloid model.

Next slide, please? Back up one, please.

KADAK: It seems like that was the dominant one earlier.

HANSEN: Radium is an important contributor in the million years. This is a 10,000 year, so what we don't observe is that the Uranium/Thorium--you know, transport is rapidly enough to contribute.

Next slide, please? Thank you.

A few remarks about the model change that caused a reduction in the Technetium that also affects Neptunium in a similar manner. There are measurements of waters taken from wells around the site, which suggest that you might find reducing conditions in those waters. I'm not the chemist, so if you have questions about these, I will direct them to my colleague, Dr. Sassani.

But, the way that we captured the effects of these reducing environments is to assign a sorption coefficient for both Technetium, and a much larger sorption coefficient for Neptunium. It's indicated here by the sub-bullet that we sample those coefficients from a normal distribution with a mean of 1000 milliliters per gram.

In the base model, Technetium is modeled as non-sorbing, and Neptunium is assigned a quite low Kd of on the order of 10 to 13 milliliters per gram.

The hypothesis is that these reducing conditions in some, but not all of the wells, could be attributed to the presence of pyrite in that rock unit.

Next slide, please?

Here is a map of the site. On the left, the dots represent wells for which water analysis has been done. The wells covered with red are those which showed reducing conditions. The wells covered with blue did not. And, then, the red shaded area that I've outlined is the largest extent, spatial extent of this reducing zone that's captured in the PMA analysis.

We did perhaps a little bit of overkill, but we modeled the extent of that zone as uncertain, and this square as, or this rectangle is as large as it would be. It could be quite a bit smaller.

On the right, the red traces through that chart are the transport paths estimated by the saturated zone transport model. So, if you can imagine overlying these two, you would see that most of those paths would pass through some or part of this reducing zone, which would result in retardation of both Technetium and additional retardation of Neptunium, subsequently reducing the concentrations that arrive at the boundary.

Next chart, please?

This figure compares the dose, the mean dose from Plutonium 239, observed in the TSPA model with that observed in the Performance Margin Analysis. I've shown four curves on each chart. The black curve is the total overall radionuclides. The red solid curve is the total dose due to Plutonium 239 arriving by all transport mechanisms.

Now, that dose can be subdivided into transport of what we call aqueous, which is plutonium in solution, as well as plutonium that reversibly sorbs to colloids. And, then, that’s shown by the green dashed. The blue dashed is the contribution from plutonium that’s irreversibly sorbed to colloids. The colloids themselves might experience retardation during transport, but the ion sorbed to them don’t leave.

You can see by comparing the left to the right that both contributions are greatly reduced in the PMA analysis. And, that could be attributed to two model changes. One is the treatment of fracture matrix diffusion in the UZ, and the other is the treatment of the properties of the colloids themselves.

Next slide, please?

A summary of what we did differently in the UZ. In the TSPA model, the fracture matrix diffusion and the coefficients involved are determined from laboratory measurements of rock samples. And, this is published in many literature articles, and typically, what you observe when you try and match predictions based on those laboratory measurements with tracer tests performed in the field, is the need for an enhancement to this diffusion coefficient to account for fractures, small scale kind of features that may not be present in your simulation.

The chart on the right compares--let’s see, let me walk you through this. The dots on the chart are measurements of two tracers that were conducted, tests that were conducted in the facility. The skinny lines that peak quite a bit above the data are simulation results, using the diffusion coefficients that are predicted from the laboratory measurements on the rock samples. And, you can see there’s quite a bit of mismatch there.

And, then, the heavier lines, which more closely match the data, are simulation results that result when you apply an enhancement factor to the diffusion coefficient, which could be interpreted as representing the effect of small scale fractures or rougher surfaces, meaning a larger surface area.

We do not feel that we had a sufficient basis to use this enhancement factor in the baseline TSPA model. But, we proposed it, applied it in the PMA to determine whether or not, if it was introduced, it would have a large effect. When it’s introduced, it has the effect of reducing the--I’m sorry--decreasing the rate of transport of the aqueous component of the Plutonium 239. So, the dissolved and the ions sorb reversibly the colloids, just travel quite slowly through the UZ, and much more of them spend time in the matrix rather than in the fractures. And, that accounts for the large reduction in that component of the 239 dose.

Next slide, please?

The other change that we made that affected the Plutonium 239 is in the representation of colloid properties. This was applied primarily in the saturated zone. In the TSPA model, colloids are represented heterogeneously regardless--I’m sorry--as a homogeneous set of properties, regardless of the chemistry or the size or, you know, other things which do vary across the whole population of the colloids.

So, in the PMA, we used what’s referred to as a colloid diversity model. And, here’s a reference to an article describing some of the thinking behind it. And, essentially represented the variability in these colloid properties. In general, that has the effect of increasing the time required to travel through the saturated zone for the different kinds of colloid species, both the irreversible and the reversibly sorbed, and, thus, reduces the quantity of Plutonium 239 that arrives at the boundary and reduces the dose.

Next slide, please?

So, to summarize the results of this 10,000 year analysis, we found that by replacing some of the conservative treatments with a more representative model, that you would observe an overall reduction in the mean by about a factor of 10, attributed primarily to the change in the treatment of the residual stress threshold for Alloy 22, to the treatment of fracture-matrix diffusion and to this treatment of colloid properties.

And remind the Board that the effects of other conservatisms do not quantity, in particular, conservatisms related to the extent of damage following an igneous event.

If you look at the mathematics behind the calculation of that mean for the igneous event, it would reduce roughly proportionately to the extent of that magma flow, generally in proportion to the number of packages affected.

I have two final slides that show the results, or summarize the results of the PMA for the million year analysis. These are the horsetails of total expected dose. You can observe a general decrease in the expected dose for the first few hundred thousand years, as compared to the base model. That’s attributed to the change in the treatment of the Alloy 22 residual stress threshold. It makes it less likely for seismic damage to occur, therefore, it’s going to reduce these expected values.

No real long-term effect observed in the PMA because we did not consider changing the treatment of the general corrosion rates.

Next slide, please?

And, this just sub-divides the means by the contribution from the modeling cases. You can see the overall reduction in the contribution from seismic ground motion due to the factors I just described. This change in the behavior of the igneous intrusion is not so much a reduction as it is a delay. The igneous events are still occurring with the same modeled frequency. It’s just that when they occur, it takes longer for the radionuclides to get through the natural system.

Next slide, please?

My final slide is a--I’ve already covered this material. Let’s go to the next slide, please. Thank you.

This is the final slide of the presentation. The Performance Margin Analysis is documented in the TSPA report, is compared to the results from Version 5.00. As I stated, a slightly different and slightly improved model was used and reported in the addendum to demonstrate compliance. So, these figures for 10,000 and a million years compare the total mean dose for Version 5, which is the red curve, Version 5.05 of the TSPA model, which is the solid blue curve. And, then, to compare with the PMA results, that would be the black one. It’s our conclusion that this analysis would have reached the same conclusions had we used either version of the TSPA model as a comparison.

That concludes my presentation. I hope I’ve gotten you back on schedule.

MOSLEH: Thank you very much. You did, so we have time for a question or two.

CERLING: Cerling, Board.

If we could go to Slide 12? I first saw this a couple years ago, and I continue to be puzzled by the notion that you can have water leaving the repository that's oxidized, it enters a reducing zone that at one point had been called a reducing curtain, and then exits and somehow gets re-oxidized, because that's the implication that these flow paths, and so on, show. And, to me, that's very puzzling, because I don't know how you can re-oxidize ground water once you've reduced it.

HANSEN: Dave or Ming, do either of you want to respond?

SASSANI: David Sassani, Sandia National Laboratories.

I don't have a really robust answer for that. You know, these are the measurements, and it may be due to different influx of ground waters at one of the flow paths. But, this heterogeneity is certainly there and can be attributed to the local extent of pyrite in the tram member, and increased iron concentrations. But, by no means is it, as characterized it, as fully as some of our other understanding of the system. That's why it's part of the PMA and not part of the actual base case.

SWIFT: This is Peter Swift from Sandia.

I just wanted to say, in case Dave didn't say it, the last thing he said do not be alarmed when he stands up and says I don’t have a very good answer for that. This is the PMA case. This is not our compliance case. These are aspects of the model we felt worth investigating further, because we perceived these are places where the model might have been more conservative than it needed to be. But, it’s not our compliance case.

GARRICK: I think you must have been asked this question before, maybe I even asked it, I don’t know. But, is there a complement to the PMA study? That is to say is there a study that investigates where your margins are small, are possibly non-conservative?

HANSEN: I’ll let Peter respond to that.

SWIFT: We do not believe there are significant non-conservatisms in the model that we presented earlier. Had there been, we would have taken them out. The intent of the model that we’re using for our licensing case is that it shall be--it shall not under-estimate the total dose. So, it may have conservatisms in it. It should not have non-conservatisms.

KADAK: Could you go to Slide Number, let’s just say the last one, which is, I think, 20? I’m trying to understand why you don’t have a whole lot of margin over the million time period, and you have a lot more margin in the early time period?

HANSEN: Okay, the contribution--I’m sorry--the total dose mean out here at a million years is coming primarily from the igneous intrusion. We have not changed the occurrence of those events or the extent of their damage. We did change some of the transport models in the UZ and SZ, so when these events occur, it takes longer for radionuclides to reach the boundary. But, eventually, they’re going to get there. They may get there in somewhat reduced quantities. So, what you’re seeing here may be a little bit hard to interpret because it’s not the actual dose following an igneous intrusion. It’s the sum of doses over all prior possible events.

There is a reduction, maybe by a factor of two, as much as a factor of three, at a million years, which could be attributed to small quantities of radionuclide actually reaching the boundaries.

KADAK: Okay.

SWIFT: If we go to the slide that just showed the seismic response, I don’t know which one that is unfortunately, and that’s the million year. The point I’m trying to make here is that in the seismic modeling case, the change in the threshold for stress corrosion cracking primarily affected that cracking that occurred in the codisposed packages, say, prior to 200,000 years. After that, go back to my presentation and the seismic case turned out to be dominated by the nominal stress corrosion processes, and general corrosion processes after several hundred thousand years.

So, even on seismic alone, most of the benefit seen in the PMA, the reduction here, occurs at relatively earlier times.

KADAK: The other question is on Slide 17. And, there was one slide--well, maybe it's the slide that shows from zero to 10,000, and then the full million, the horsetails.

HANSEN: I don't know if I compared both times periods on the same chart.

KADAK: What I have observed, let me just make a statement, and then you can correct me if I'm wrong, it looks like by the spread of these lines in the first 10,000 year period, there appears to be more uncertainty in the dose than there is in the million year time horizon, and I find that somewhat hard to accept, that we know more about a million year time horizon than we know about the first 10,000 or a thousand years.

HANSEN: I think that might be stretching it a little bit far.

KADAK: The data runs kind of show that.

HANSEN: Yes, but this is a model. These are model results. So, I can explain them from that perspective. There are a large number of uncertain inputs. The uncertainty of those inputs don't have equal ranges, of course, and they don't have equal affects on the system model output.

In the first 10,000 years, the uncertainty that predominantly determines that spread, and here I'm referring to the TSPA model, is the uncertainty in that residual stress threshold, which you could essentially make equivalent to the probability of having a seismic event that causes damage.

In the later part of the million year period, the uncertainty in these expected dose results is determined by the uncertainty in the rate of general corrosion. So, a different range of uncertainty on the input. The inputs are having different effects on the system model from a mathematician's point of view. That's why there's different ranges of uncertainty in the model results.

GARRICK: Is it possible that the greatest uncertainties are associated with degradation and mobilization, and once the material gets into a mobile state, there's less uncertainty from a phenomenological standpoint as to what happens during the transport?

HANSEN: That is likely true. Although, I couldn't show--I don't have a slide in this presentation that would support that, but I think that's a fair way to think about the system. Uncertainty in the occurrence of events that compromise waste packages tends to drive the uncertainty in these expected dose results. Dr. Helton has something he'd like to add.

HELTON: There's one other observation on the uncertainty. At early times, some of that uncertainty is being driven by arrival times. Material is released, and then it takes a certain time for the front to get to the location of the RMEI. Whereas, at later times, you had many prior events that are being looked at, so the uncertainty in the arrival time kind of gets swamped out, and then brings down the overall uncertainty.

KADAK: You're not implying, though, precision at a million years, are you?

HANSEN: I hope not, no.

DIODATO: Diodato, Staff.

Just on Slide 14, as you look at this, I recall the way Bo Bodvarsson often expressed his view that maybe matrix diffusion was under-estimated in the models for performance assessment, and I recall him expressing that view often.

So, in terms of the magnitude of the impact with increasing this matrix diffusion coefficient, what's the change in the mean break-through time? Do you have an assessment on that in terms of the difference between the break-through time to the saturated zone from the TSPA case versus the PMA case?

HANSEN: No, I don't have break-through curves to show you. If you go back to Slide 13, you can convince yourself that it’s going to be on the order of--the travel time would be on the order of three times longer. And, you get there by comparing where you begin to observe a mean dose from aqueous species, you know, this quantity 10 to the minus 6, between the two analyses.

Ming, do you have anything you’d like to add to that?

ZHU: No, I don’t think you will have that intermediate results showing the break-through.

DIODATO: So, it’s kind of an estimate according to these--

HANSEN: Yes.

KIRSTEIN: You show us quite a few plots with radionuclide specific. Do you have any results available from GoldSim that illustrate the numerical precision in the form of material balances for a nuclide, such that you account for both ingrowth and decay, and nuclides of interest would be Plutonium, Technetium, Neptunium?

HANSEN: Is your question about the compliance model or about the PMA results?

KIRSTEIN: GoldSim in general, with regard to all these plots that show radionuclide specific information?

HANSEN: The answer to that is no, there are no plots that address that kind of mass balance specifically that are included in the TSPA MR. We have plots like that. They are in unpublished work that the lab has done, you know, part of our model validation and investigation, but they weren't prepared to bring to this meeting.

KIRSTEIN: Could we see those sometime?

HANSEN: If you can make that request to DOE? I can't answer that question.

SEVUGIAN: Dave Sevugian.

If you read Volume II that Peter showed in his presentation, the validation section of the AMR, we have a section devoted--it might be in the addendum--we have a section devoted to mass balance. So, although the plots may not be there, the calculation results are there.

HANSEN: Thank you, Dave. It slipped my mind, or I was unaware that it was there.

MOSLEH: Are there any other questions?

(No response.)

MOSLEH: Well, in that case, thank you very much. We're back on schedule. We have now, I think we're at the lunch break. We will return at 1:30.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

GARRICK: Some minor adjustments in our agenda for this afternoon. We’re not going to eliminate anything. We’re just going to rearrange a few things. And, because of some schedule conflicts, we’re going to allow the people that are interested in making a public comment to do so now. We have two people that have requested that time, Atef Elzeftawy and Victor Gilinsky. And, I know Atef is here, so we’ll turn it over to him. And, if Dr. Gilinsky appears, we’ll let him make his comments, and then we’ll proceed with the agenda.

ELZEFTAWY: Good afternoon. My name is Atef Elzeftawy, and I’m here for the Las Vegas Piute Tribe here in Las Vegas, Nevada. And, I’m enjoying the meeting very much, to the point that I get a text message on this phone, “We’ll see you at 2:30.” So, I guess the boss wanted me to go back.

But, I just wanted to say two things on behalf of the tribe. We wanted to say thank you for the Board and the Board members and staff and everybody who is here for coming to Las Vegas, Nevada and allowing us to save a couple dollars to listen to what you guys say and do, and all that. And, so, we just got you 85 degree weather, so enjoy the day, because it’s going to go to 100 degrees very soon.

The comments I wanted to say on behalf of the tribe is, according to what the chairman said, well, we want to trust you, but we want to verify, quote, unquote, Ronald Reagan some time ago. So, the work is being done, but when it comes to verification of the total assessment performance programs, it’s going to be difficult. So, somehow, somewhere, you need to get the feel and the understanding that you really are trusting your results of your data. We wanted to avoid the problems that NASA had gone through many times making programs, and then they are, unfortunately, they failed a couple times. But, they have the privilege of testing what they have. And, I wonder how we are going to test this big, huge, humongous computer programs that you guys are dealing with.

But, thank you again for coming, and thank you for everything. So, again, the chairman said trust and verify. Thanks for the Board, and all that. Appreciate it. Thanks.

GARRICK: Thanks, Atef. Is Dr. Gilinsky here? All right, well, we’ll hear from him later in the day.

The other change in the program is that I understand that the presentation on the Independent Performance Assessment Review and the EPRI Presentation are going to change positions. And, other than that, I think we’re okay.

So, with that, I’m going to turn it back over to Ali.

MOSLEH: All right. Good afternoon. The first technical presentation of this afternoon is the one by Dr. Jon Helton on Uncertainty and Sensitivity Analysis of the

TSPA. Jon?

HELTON: Thank you very much. What Cedric Sallaberry, my colleague, and I would like to do is to lead you through several aspects of the analysis. We're going to be looking at how the results for the individual modeling cases that Peter spoke of earlier came together to get the overall results. And, then, we will be showing you how we obtained sensitivity analyses results for these outcomes.

The way this presentation is organized is we have three large posters that basically lead you through the analysis. We'll put the posters up on the screen to show you what they look like, and then we'll start blowing up individual pieces of them. And, I believe the Review Board has copies of the big slides that are listed on the screen here, and I believe there are also handouts for the remaining audience. Is that correct? Did the additional handouts get here?

Would you put up the first poster, please? All right, there is one slide that lists some of the acronyms that get used very commonly in presentations involving the Yucca Mountain Performance Assessment. So, keep this handy if some acronym goes by that you're not familiar with.

All right, a bit of overall structure here before we go into detail. On the top half of the poster here, we have a summary of how we go through the analysis to get expected dose to the reasonably maximally exposed individual, or RMEI, for the 10,000 year period, and we’ll be going through these slides in more detail. And, then, on the bottom half, we have a similar tracking through the analysis to get results for the 1 million year time period.

All right, what we have in these three--pardon me--these six slides are the expected dose results for the six modeling cases that Peter talked about earlier. Let me give you just a little bit of background on how these are coming about, and what the pieces are before we go into them.

If you will back up and kind of look at the analysis that we’re doing in the large? There are essentially three basic pieces in this analysis. The first piece is a characterization of what can happen in the future. Often times, we’ve heard of it as a characterization of aleatory uncertainty. This has to do with the random events that may or may not happen in the future that are characterized probabilistically, the occurrence or non-occurrence of seismic events, and then the properties of the seismic events, if they do occur.

The occurrence or non-occurrence of igneous events, and their time that they do occur. The occurrence or non-occurrence of early waste package failures, and then the properties associated with those failures, if they do occur. The occurrence or non-occurrence of early drip shield failures and their properties, if they occur. And, then, finally, the occurrence and non-occurrence of seismic ground motion events, and their properties such as timing and size, if they occur, and the occurrence of seismic fault displacement events, the time of these events, and their size, basically, if they do occur. So, those are your aleatory properties.

Then, we have a model, given a particular set of occurrences that predicts consequences. That model is essentially what's referred to as the GoldSim model and its ancillary programs that support it. So, that's the second piece.

Then, the third piece is a characterization of uncertainty in quantities that we assume have fixed values in the context of our analysis, but we're not really sure exactly what those values ought to be. You're talking about things like spatially average distribution coefficient, spatially averaged chemical properties. Our disruptive events are assumed to follow Poisson processes with a rate of occurrence, but we're not really sure what that rate of occurrence is. Well, that type of uncertainty is referred to as epistemic uncertainty, and that's the third of the pieces.

What we have here are the results for expected dose to the reasonably maximally exposed individual for early waste package failure. And, those three quantities that I just mentioned go into it. Each one of these individual curves is the expected dose to the RMEI that is resulting from randomness and things that could happen. So, at any given time, the RMEI could be experiencing dose from many different things that happened at prior points in time, and each of those potential doses is weighted by the likelihood of it arising, and, so, that is what is giving you one of these curves. And, essentially, the generation of each one of those curves is an integration problem.

Then, because we have this--and it’s really integrating the GoldSim model in a fairly complex fashion, but it’s still calculus. Then, we have the epistemic uncertainty, which is this lack of knowledge. So, in the Yucca Mountain Performance Assessment, this lack of knowledge is characterized with probability distributions, and these distributions serve to numerically capture our state of knowledge with respect to where these uncertain parameters are located.

And, to incorporate this uncertainty into the analysis, we use a sampling technique called Latin Hypercube sampling, which is a very efficient sampling technique. So, we generate a sample the size 300 from these uncertain parameters. And, then, for each one of those 300 sample elements, we calculate just one of these expected dose curves.

So, the whole spread of these dose curves, the dark lines are showing our uncertainty in expected dose to RMEI as a function of time, and then we summarize this uncertainty with various summary measures, probably the most important one is the mean or the vertical average of all the individual curves, which is really just another integration over epistemic uncertainty. And, then, we can also extract the median dose curve, which figures in the post-10K regulations, and to provide a summary of the uncertainty, we also have a 95th percentile curve and a 5th percentile curve, kind of bracketing where the uncertainty is.

So, that is what we have done, and that's the type of results you're going to be seeing here.

ARNOLD: Just a quick question.

HELTON: Certainly.

ARNOLD: Any zero curves?

HELTON: There are no zero curves, because we're--there's some probability, something always happens, so we're always getting something, even with a low probability. And, that also brings up an important distinction. It's come up before. Dr. Garrick mentioned it. You know, what we're looking at here are expected results. They have, you know, units of dose, millirems if you refer to it as annual dose, or millirems per year if you just want to call it dose. But, the dose here is not the actual received dose by an individual. It is the expectation of the dose that this individual would receive, taking into account the likelihood of getting it.

So, in reality, in most of these curves when they’re calculated, you have a lot of futures, you know, 10,000 year sequences of occurrence in which the RMEI gets no dose at all, zero dose. You have a few in which he gets a dose, and then that dose is weighted by the probability of it being realized.

KADAK: What are those jumps at 2,000 and 10,000?

HELTON: They are associated with changes in climate where you have a change in the water flux. The infiltration changes several times. 2,000 years is one of those times. 10,000 years is another time. And, you will see a little tick up in the results when the infiltration changes, which really corresponds to a change in the flow field surrounding the repository.

KADAK: How hard would it be to take this curve and reconstruct it into a probability versus dose curve?

HELTON: This curve is the result of that calculation. Mathematically, I cannot go from this curve back to the CCDF. I can go from the CCDF to the expected results. Now, I have another slide which was really a backup slide that shows exactly that type of result. If there’s interest, I kind of indicated to John I would show it. That’s the sequences I think. I tend to think of distributions of dose and then going to expected dose. But, the regulations that we deal with speak--the regulation refers to it as mean dose, but most probability books refer to it as the expected value. So, I tend to refer to it as the expected dose.

Back up to all six?

The question was asked earlier what was the rationale for dividing our analysis into these six cases? And, it was certainly appropriate computationally. But, what we’re actually calculating for each one of these cases is the incremental dose that happens due to what’s being under consideration. So, here, for the early waste package failure, we’re seeing the incremental dose to the RMEI that happens that drives just from early waste package failure.

Here, we’re seeing the part of the expected dose that drives just from the early drip shield failures, and so on. So, by breaking it up that way, we were able to construct the analysis in a way that appropriately conserves probability, allows us to show the results for these different types of disruptions, and then add the results together to get a total dose for the RMEI, a total expected dose.

So, the focus of this presentation is on what happens when we bring all of these results together. Now, a very important aspect of how the analysis is designed is, as

I said, we used a Latin Hypercube sample of Size 300 in the analysis. For each one of these six modeling cases, we used exactly the same Latin Hypercube sample. So, that means we can add the results together for these six modeling cases on a sample element by sample element basis, which is what allows us to go over to the slides to the right, if you'd move us over there?

So, when we add, for each one of our 300 sample elements, we have six expected dose curves. We add them together, and that gives us one expected dose curve there. We do it 300 times. This is the results for expected dose to the RMEI from all processes over the 10,000 year time period. Again, you see the overall mean curve that figures in showing compliance with the regulations. Let's see, right there is 10 millirems per year, 15, so just slightly above that. So, in terms of the regulation, we're way below it.

Although, as you can see by the spread in the results, the individual expected dose curve is conditional on the individual realizations of epistemic uncertainty. There's still a lot of uncertainty, even though we're way below the standard, there's still significant uncertainty in the results we're calculating. So, a reasonable question is what's driving the uncertainty?

We, in our sampling, we sampled slightly less than 400 uncertain variables in that Latin Hypercube sample I was indicating to you, although not all those variables are relevant to all analyses.

To see what is driving the uncertainty, we did a sensitivity analysis. The particular sensitivity analysis technique that I'm showing you here is based on what are called Partial Rank Correlation Coefficients, so, correlation coefficient between a sampled variable and a calculated variable indicates the extent to which the variables move up or down together. The Partial Correlation Coefficient has a value somewhere between minus 1 and plus 1. Plus 1 means there's a strong positive relationship between the two variables, when one variable goes up, the other one goes up. Negative correlation means they move in opposite directions, in a very strongly pronounced way.

A correlation close to zero means there is--I could say it means there's no relationship, but that's not really quite correct. It means there is no linear relationship. If you're using raw data, or as we've done here, if you're using rank transformed data, it means there's no monotonic relationship. We do the analysis with a rank transformed data. In other words, we take the variables, the smallest variable value will rank 1, the next one largest will rank a 2, and so up to the largest will rank a 300. And, what that does is it makes non-linear relationships which are very common in what we do. Look linear after you've done the rank transformation.

GARRICK: Jon?

HELTON: Yes.

GARRICK: Can we back up to the integrated curve for just a second?

HELTON: Certainly.

GARRICK: Have you--did you look at cut curves at specific points in time?

HELTON: Yes.

GARRICK: To develop a better physical feel of the probability density functions that you would get as a result of that? In other words, the mean and the median are a little closer together than I thought they might be for something about which there is so much uncertainty, and for which the distributions are quite non-uniform. Do you have cut curves, cut probability density curves at specific points in time? Of course at a million years, that would be an interesting one to look at.

HELTON: Actually, the ones where I constructed the--you're talking about CCDFs; right?

GARRICK: Well, I was thinking in terms of actually a probability density function at specific points in time of the dose.

HELTON: Of actual dose?

GARRICK: Yes.

HELTON: We did not do the probability density functions. They were always presented as cumulative distributions at a large number of points in time. We typically, for the 10,000 year calculation, I constructed them at 10,000 years to see what they look like, and also as a verification of our calculation of the expected dose result there. I did not go through and calculate the CCDFs at multiple times along the way.

GARRICK: Okay.

HELTON: But, while I’ve got this slide up here, before we go back to the other one, on the sensitivity analysis results I’m going to be showing you, what we did is we went along here at a sequence of points, and basically drew vertical lines up through these curves, and asked the question what’s driving the uncertainty here, and what’s driving the uncertainty here.

GARRICK: That’s what I was getting at, yeah.

HELTON: And, so on.

GARRICK: Yes.

HELTON: So, that’s what it--we’ll go back over the sensitivity analysis results. What we have on the lower axis here is time. It’s the same time interval, 10,000 years, that we saw on the dose results. And, what we’re seeing now are plots of Partial Ranked Correlation Coefficients that indicate variable importance.

So, when you have a curve that goes down here close to negative one, as this one does, it says, wow, this variable has a big effect on the uncertainty in expected dose. And, it turns out this particular--this curve right here is the--corresponds to the variable that Cliff was talking about, which is the residual stress level at which stress corrosion cracking initiates and causes waste package failure for seismic events. This variable affects the analysis in two ways. First, it affects the likelihood that a seismic event of a given peak ground velocity will cause damage to the waste package, and, second, it affects the distribution of the damaged area.

So, as the stress corrosion initiating level goes up, the likelihood of damage goes down. So, you have a negative correlation. If you will look at the top, you will see we have another variable, which is the variable related which defines the rate of occurrence of igneous events. It’s basically the rate term in the Poisson process that defines the rate of occurrence of igneous events. It has a noticeable positive effect on the expected dose, which is what you would expect. As the rate of occurrence of igneous events goes up, the expected dose that derives from igneous events also goes up. So, you see that positive effect.

You see another curve here, which is the green curve. It corresponds to a variable which is called the saturated zone specific ground water discharge multiplier. This is a variable that incorporates uncertainty into the rate of flow in the saturated zone. As its value increases, the rate of fluid flow increases, so you move radionuclides more rapidly, so it has a positive effect.

Then, you will see several other variables that have smaller effects, but overall, you're seeing this dominant effect coming from the uncertainty, and the most important variable with respect to seismic effects, and the most important variable associated with igneous effects.

That's not too surprising, if you'll look, all six of these are on the same vertical scale. You can see that the results of the seismic ground motion and the igneous intrusive events are, you know, the biggest incremental expected dose, so it's not too surprising that the variables that are most important to the uncertainty in these two quantities also turn out to be the most important variables with respect to the uncertainty in total dose.

LATANISION: Latanision, Board.

I was with you until that last couple of sentences. I thought that the most dramatic correlation was with the residual stress, stress corrosion cracking and residual stress threshold.

HELTON: Yes.

LATANISION: Where does that appear on--in these six, how does that manifest?

HELTON: If we were to do an analysis of the seismic ground motion, if I was to present an analysis for these results here, the same form I was showing you for the total results, the residual stress variable would be hands down the dominant variable with respect to the uncertainty you're seeing here.

LATANISION: So, that has to do with rock--

HELTON: That has to do, when you have a seismic event and you bang the waste packages together.

LATANISION: Okay.

HELTON: It causes stress, and this residual stress level defines the stress level at which you will get waste package failure. So, as you move the stress failure initiation level up, you move the number of failures down. And, likewise, this is the result for the igneous event, and the uncertainty there, if we were to do an analysis, would be dominated by the rate term that defines the rate of occurrence.

LATANISION: Thank you.

HELTON: Let's drop down to the million year results.

Here, we have the same six results before--it's probably worth making a comment on something that's different--on the 10,000 year results, everything is sufficiently nicely behaved that we can do the analysis, we can contact those expected dose curves with a nice integration formula. Basically, a quaduture approach. It breaks down when we go to the seismic ground motion effects. The reason being is there are so many processes going on, you just can’t write a nice clean integration formula to integrate the GoldSim results, because you have not just one or two or three seismic events, but over a million years, you could have a hundred seismic events, because they’re occurring at a rate of something around, you know, 10 to the minus 4 per year. So, 10 to the minus 4 per year for a million years is, you know, something around a hundred events.

Plus, the repository is evolving through time. The waste packages are thinning due to corrosion. The drip shields are failing. There are just a lot of things going on. So, to get the individual expected dose curves for a million years due to seismic ground motion and nominal process, we have to include the nominal processes there because that’s affecting waste package thinning and drip shield failure, among other things. We use a Monte Carlo integration procedure. We just--we sample 30, 1 million year futures for every one of our 300 Latin Hypercube sample elements. So, each one of the individual curves there has been generated by sampling 30 individual futures, and then averaging them.

But, again, when you look at it, you can see that the dominant results are really coming from the combined seismic ground motion and the igneous intrusive conditions. So, once again, the individual curves from these six frames are added together to get the total 1 million year results, which is what you see here, the result of that addition, exactly the same structure you saw in the preceding slide. And, once more, we can do a sensitivity analysis and say what's driving it? The results aren't quite as smooth here as you saw before, and the reason for the noise comes from the use of the Monte Carlo integration to calculate expected dose with 30 sampled futures for each LHS element. If you use more and more futures, we would get, you know, smoother results.

ABKOWITZ: Abkowitz, Board.

Can you comment on your choice of these six variables? They seem bizarre to me in terms of the way they're measured, and how they relate back to the six different scenarios that they originally came from. I mean, you've got logarithmic values in here. You've got point values. You've got frequencies. How did you arrive at these six variables?

HELTON: Well, these--

ABKOWITZ: How did you associate them back to these scenarios?

HELTON: I think your question has two levels of answers. The easier one, as we saw, I'll give you that one first, is the six variables that we're showing are the six that have the largest and absolute value, Partial Rank Correlation Coefficients, over the time period we're considering.

We picked the number six because you start getting more curves than that, it gets to be too hard to read, so we picked the six with the largest Partial Rank Correlation Coefficients.

The harder part of your question is how do we come up with the original almost 400 uncertain variables that were the candidates in the analysis. And, there, the individual analysts working the various areas that go into the model were asked, you know, what do you feel are the important uncertain variables in your part of the analysis, and please give us a probability distribution that captures and mathematically characterizes your degree of belief with respect to where the appropriate value for this quantity is located for use in the analysis.

ABKOWITZ: Just one other followup question. Did you do any type of correlation analysis between those 400 variables to see how they might be masking the effects of one another?

HELTON: Some of the variables are correlated. Some of them are very strongly correlated. Correlations wreak havoc in analyses of this type. So, variables that had specified correlations were sampled with correlations. Then, we went through before we did this analysis and we essentially took out variables where you had a cluster of variables, say two or three variables, and they all had--they were correlated with the correlation of .95 or .9, and we said okay, we’re only going to include one of those correlated variables in the analysis, because any one of them is basically a surrogate for the others when you do an analysis of this type.

MOSLEH: A quick question on this one. Are there parameters that are a function of other parameters in this thing, in this list of 400?

HELTON: There are some that were--not very many, but there are some in here that are functions of other variables. But, again, we had to be careful when something like that occurred, not to basically put too highly correlated variables in because you just lose the effects of both of them when you do.

DIODATO: Can I just follow up on that for a second? Diodato, Staff.

So, WDGCA22 was the temperature dependence of the generalized corrosion rate of the Alloy 22 outer barrier. So, was temperature included in your analyses as well, or not?

HELTON: Temperature is a calculated quantity. So, temperature is one of the variables, one of the analysis outcomes that is calculated as part of the GoldSim model. So, it would not be an epistemically uncertain quantity that we sampled.

DIODATO: So, temperature is not viewed as an uncertain quantity in your analysis?

HELTON: No, temperature is not used as an uncertain quantity in our analysis.

DIODATO: Thank you.

HELTON: If you look at the--I’m glad you brought that up. If you look at the results here, you can see the changing importance of different variables with time. At fairly early times, you have failures really being dominated by seismic events. So, here is that variable that related to the stress level at which you fail waste packages due to seismic events, and you can see it has that strong negative effect that we--at early times that we also saw for the 10,000 year analysis. Then, it becomes less important.

Here, you see that the positive effect of the rate of occurrence of the igneous events remains important through time. Then, as you pointed out, as you go out later in time, you see this strong negative effect for this variable that relates to the temperature dependence of general corrosion rate. And, that only starts kicking in after about 200,000 years, which is when you start getting general corrosion, you know, failures. So, when you start getting general corrosion failures, the number of general corrosion failures is actually negatively correlated with this variable, because as its value goes up, the rate of general corrosion goes down. So, you get fewer general corrosion failures.

Also, you see a fairly constant positive effect of this ground water specific discharge multiplier through time, that positive effect results because its value increases as you increase the rate of ground water flow, you increase the release.

Any questions before I show you two stability results?

(No response.)

All right, this result has been shown before, but it's worth repeating at this point. We're using a Latin Hypercube sample of Size 300 from almost 400 variables. I mean, the question is always well, is that sample big enough. Boy, it just doesn't sound big enough to me. So, we replicated the analysis. We generated independently three Latin Hypercube samples, each with 300 variables in it, and 300 sample elements, and then we reran the analysis. Basically, we reran the analysis three times independently, and as you can see, each, there are the three, 95th percentile curves, there are the three mean curves, and median and 5th percentile. We got very stable results. Numerically, we have converged the results. We did not need a bigger sample size. So, that's for the 10,000 year case. We have similar results below for the million year case.

If you'd like, we can now look at some more specific results. Let's look at the Neptunium result first. All right, what we're just looking at were expected results that come from the consideration of all radionuclides across all of our modeling cases. We're now going to move and be much more specific. We're going to look at results associated with one radionuclide, Neptunium 237. It is results associated with exactly one of the modeling cases, namely igneous intrusion, with the igneous intrusive event taking place at ten years.

And, when you track across the poster what we are doing is we are looking at--we're going to be looking at release rate out of the EBS, release rate out of the unsaturated zone, release rate out of the SZ at the location of the RMEI, and dose to the RMEI, and then we'll be looking at sensitivity analysis results as we move along.

What you're seeing on this plot are the time dependent release rates for 50 out of our 300 LHS elements. We only put--all three curves, it tends to look like a black mass, so I only plotted 50 of the 300 time dependent release rates, simply because it makes it possible to see some resolution of the individual curves.

This is what the actual results are for release taking place out of the EBS following an igneous intrusive event at 10 years that destroyed all the waste package in the repository. You can see some jumping around here at the beginning, and then it kind of settles down and you’ve got a fairly constant release rate.

Now, we can ask the question, well, what’s driving the uncertainty. Well, that’s the sensitivity analysis. And, remember because this is conditional on the event happening, that rate term for igneous events doesn’t have any role here, because we’re just assuming the event has happened.

We have this variable, which has a strong positive effect, in other words, as its value goes up, the release rate goes up. That variable is a scale factor used to incorporate uncertainty into the solubility of Neptunium. Solubility goes up, release goes up.

Here, you see for this variable, a strong negative effect. That is a variable used to incorporate uncertainty into pH in the vicinity of CSNF waste packages. pH goes up, solubility goes down. Negative effect. You see this variable right here is kind of interesting. You notice it has a strong positive effect very early on, and then its effect drops down to basically zero, no effect. That variable is the thermal conductivity in the vicinity of the repository. Why does it have a strong positive effect early on? Higher thermal conductivity. You cool the repository more rapidly, you drop below boiling more rapidly, you start release of radionuclides earlier, Neptunium in this particular case, so it has a strong positive effect at early times from initiating early release due to drop in the repository beneath boiling, and then it basically doesn’t have any effect after that.

DIODATO: I’m sorry to interrupt again. This is back to this correlation question.

You brought up the EP1 is a variable that describes the solubility of Neptunium?

HELTON: Yes.

DIODATO: And, then, the pH, CSS is also a variable that describes the pH condition affecting the mobility of Neptunium. So, aren’t these variables kind of correlated in some sense in terms of the chemistry?

HELTON: They were not specified, but I will defer.

SASSANI: Dave Sassani, Sandia.

The variability one for the NPO2 is the uncertainty for the chemical data, constraining solubility.

DIODATO: KSP?

SASSANI: Yes. So, those are not correlated. I’m not quite sure, I think the pH is the--I don’t know if that’s the pH in the package, in the CSNF package, so that’s completely independent of these other constants for the--the solubility itself is related to both of those variables.

DIODATO: Okay, thank you.

HELTON: Then we have another positive effect for a variable here that is related to the partial pressure of CO2. This green here is the infiltration. It has a positive effect through time, increasing infiltration, increases water flux through the repository, increases radionuclide release. And, the orange line here is another variable related to solubility, except for this time, it’s related to the solubility of Americium. Why Americium? This is Neptunium 237. Americium 241 is the parent. So, you’re increasing this variable, increases the release of Americium, which increases the generation of Neptunium 237 due to decay.

These are kind of nice quantitative summaries. Sometimes, a visual summary helps more. So, Cedric, would you move out and show the two scatterplots here? Okay, drop down and show it. All right, so, we’ve got--what I want to show you is right here on this slide. Where this vertical line is shown, it’s really cutting 300 curves. So, I can generate scatterplots that show the 300 values for Neptunium release rate at 10,000 years versus various of the uncertain variables that were in the sample.

So, arguably, the two most important variables, as indicated by the size of their Partial Correlation Coefficients, was this variable related to the Neptunium solubility, and as you can see, yes, indeed, there is a positive trend in the data. It’s that positive trend that the Partial Correlation Coefficient is picking out. And, then, here is the scatter plot where we have the 300 values for this variable related to pH on the abscissa and, again, the 300 values for release rate on the argonaut, and, once again, you can see a negative trend here, which is what we saw in the Partial Correlation Coefficients.

Now, I have to tell you we’re only showing you a small sub-set of the analyses that were done. We did a huge number of analyses like this, both to understand the analysis and what was driving it, and also, and this is very important, analyses like this are a very powerful Q/A tool, model verification tool that allows you to look at the effects of large numbers of independent variables on large numbers of outputs, and say does this make sense, does this make sense. Well, wait a moment, maybe this doesn’t make sense. It’s very powerful.

KADAK: Before you leave that one. Tell me why you look at those two, number one and number two, and say you’ll feel good about that today.

HELTON: I see a positive trend. The analysts who developed the model say yes, this variable ought to have a positive--

KADAK: Tell me about the yellow ones and the blue ones. I mean, if you look at any one of those lines, you see large variability between say, minus whatever that number is on the “X” axis, and then there’s a whole bunch of numbers on the--I don’t know.

HELTON: Okay. Infiltration, that is the--basically, about the third most important variable in the analysis. It was a discrete variable. You only had the four infiltration levels, and actually, those four infiltration levels are really pointer variables to four different time dependent, three dimensional flow fields. But, because it’s a discrete variable, it only has four--it’s identified by a discrete variable. We were able to color code it, and give red to the flow fields that had the lowest flux associated with them, and then as we go up through the colors, we’re moving up to the flow fields that had the highest flux associated with them. And, although it’s not real obvious, these smaller values tend to be associated with the red dots. The red dots corresponding to the flow fields that have the lowest flux associated with them.

KADAK: Just take the zero/zero point on the “X” axis. The variability of the “Y” axis is quite large. Would you agree?

HELTON: Oh, I agree.

KADAK: So, how do you use this information?

HELTON: Well, it says, for one thing, although this variable--and, actually, it's the logarithm of a scale factor, when you get right down to it. These are log values, not actual values. It says, yes, increasing this variable increases the release of Neptunium 237 out of the EBS, but, hey, there are a bunch of other things that have affects on it, too. The release is positively affected by this variable, but it's not the only thing that affects it.

ABKOWITZ: Did you do any kind of regression analysis off of these plots with some--

HELTON: Yes, we did. If you will--in Appendix K of the AMR, we have large numbers of step-wise regression analyses, where you can see the incremental R-squared values. I did not bring any--put any step-wise regressions in this presentation, simply because tables and numbers are kind of a pain to look at.

ABKOWITZ: But, goodness of fit is important, is it not? Goodness of fit is important, is it not?

HELTON: Oh, absolutely. Absolutely. So, in the regression tables, what we will have is, all right, here's the variable picked first in the regression, here is its R-squared. Now, pick the next most important variable. Here is the variable picked, and here is the R-squared value using those two variables, and so on, working on down.

ABKOWITZ: You said that’s in Appendix K?

HELTON: Appendix K.

ABKOWITZ: Thank you.

MURPHY: Pardon me. I have another kind of “does this make sense” question. It refers to the figure that’s just below these two.

HELTON: Okay.

MURPHY: Here, we see Neptunium being released from a waste package, and--

HELTON: The EBS.

MURPHY: From the EBS, and at the upper end, we have about 1 kilogram per year, and durations here of 20,000 years. And, so, that’s 20,000 kilograms of Neptunium, and I’m wondering about the mass balance. I know Neptunium grows in, but eventually, I think at that rate, one would deplete the waste package of Neptunium and Americium.

HELTON: Yes.

MURPHY: But, none of the curves go down to zero rates.

HELTON: Obviously, over the 10,000 year period, the inventory has not been depleted. I presume, you know, if we ran this plot on out to a million years, you would see depletion.

MURPHY: I thought maybe I saw it when I--some of the highest curves do go down. They peak and start to go down, and I wondered if that was maybe a depletion. But, none of them go to zero.

HELTON: Remember, this is for an igneous intrusive event that damages, effectively destroys all waste packages in the repository. So, we’re talking about the entire repository inventory of Neptunium 237 being available for transport.

MURPHY: That might be a relatively easy one to check, just on a mass balance basis.

HELTON: Yes, quite reasonable.

ARNOLD: Well, you take the 10 to the 3, and 20,000 years, that’s already 10 metric tons. The whole repository, counting everything else, is 70,000. An infinite source of Neptunium.

HELTON: Well, there can’t be an infinite source of Neptunium.

SWIFT: The model does track the mass. None of us happen to know what that total mass of Neptunium is. But, as long as the waste form is still degrading and releasing mass, you will see a flat solubility release rate. So, I’m not sure what the total mass is, or how long it would be before that dropped off, but--

ARNOLD: Can’t be more than 70,000 metric tons.

SWIFT: I agree. We’re not there yet on that plot.

ARNOLD: No, you’ve used twenty of them.

SASSANI: If I could just take a quick ballpark at it, 70,000 metric tons in commercial spent fuel of Neptunium is about .001. So, you’re talking about 70 metric tons. So, if we’re at 20 metric tons, that’s not that entire inventory.

MURPHY: Well, this is about a third of that total of Neptunium inventory being released in 20,000 years.

HELTON: Peter, do you know the inventory numbers?

SWIFT: I don’t. But, I wanted to point out this is for an igneous event that occurred at a fixed time, ten years. In the full simulation, the igneous event could happen at any time throughout the life of the repository. So, yes, it is not unreasonable to say it’s several tens of thousands of years after an igneous event that damages all the packages in the repository. You might actually say inventory depletion. That’s not an unexpected observation.

HELTON: Okay. Well, we’ll double check that, but let’s move on through the transport process.

All right, this plot is showing you the release rate of your Neptunium out of the bottom of the UZ. You will notice the curves are a little jiggly there, not quite as smooth as you saw before. I’ll answer that question before I’m asked. The transport in the unsaturated zone is done with a particle tracking scheme, and the noise that you see, the jiggles and the curves are coming from the use of the particle tracking scheme, the arrival of--kind of the random arrival of particles used in the numerical solution of the transport equation. So, that’s where that is coming from.

In actuality, the unsaturated zone is not doing a great deal to hold up the movement of the Neptunium. Cedric, if you’ll drop down and show the comparison of the in/out plot? What we have here is essentially a barrier effectiveness plot where what we have on the abscissa is the integrated release of Neptunium 237 into the unsaturated zone, out to 10,000 years. And, what we have on the argonaut is the integrated release of Neptunium 237 out the bottom of the UZ out to 10,000 years.

So, if everything that went in came out over 10,000 years, all the points would fall right on this straight line here. This line here corresponds to one order of magnitude reduction. So, what you’re seeing is you’re getting some reduction in the Neptunium 237, some hold-up in the unsaturated zone, but not a great deal, factors of two, maybe, or less.

DIODATO: Jon, could you explain this? So, the next plot over is the--I thought this was the release from the EBS to the UZ, and then the next one would be the UZ to the SZ; is that not correct?

HELTON: That is correct. So, if we go back up to the release rate, these are the release rates out of the bottom of the UZ. And, the comparison, the scatterplot I just showed you, was a comparison between the amount of Neptunium

237 that entered the UZ over 10,000 years, and the amount that came out over 10,000 years. My point being is these curves here actually look a lot like the curves for release out of the EBS, because to a great extent, the release of Neptunium 237 into the UZ is coming out the bottom of the UZ. And, if we look at the sensitivity analysis results, what you will see if the sensitivity analysis results for the release rate out of the bottom of the UZ, look a lot like the sensitivity analysis results for release out of the EBS.

DIODATO: So, the variable, just to be clear, this variable that you have on the ESNP237C, that’s the release from the engineered system. And, then, on the ordinate, UZNP237 is the release from the UZ?

HELTON: UZNP237 is the release rate at the bottom of the unsaturated zone. When you see a “C” after it, it stands for cumulative, which means it’s an integrated release. So, the scatterplot I was showing you was comparing two cumulative releases, cumulative out of the EBS, cumulative out of the UZ over 10,000 years.

DIODATO: Thank you.

HELTON: So, there’s a barrier effect in this result. So, let’s just move on into the SZ. So, now, this is the next plot in the sequence going across the page, where we are now looking at the release rate coming out of the SZ at the location of the RMEI. That’s about 18 kilometers away from the repository, and as you can see, there’s quite a bit of change in the appearance of the curves due to the effects of transport in the saturated zone.

If we drop down, we can see what’s happening there. All right, here’s another comparison, except now we’re comparing the release coming out of the saturated zone at the location of the RMEI with the release going into the saturated zone at the bottom of the SZ. So, again, to the extent that we have points falling below this line, we have an indication of hold-up in the SZ. And, again, this is over a period of 10,000 years. So, there’s one, two, three, four orders of magnitude. For some realizations of our epistemic uncertainty, we’re getting a large amount of hold-up of Neptunium 237 in the saturated zone. On the other hand, there are also some sample elements for which there is not very much hold-up going on.

To see what’s driving the results, we can look at the sensitivity analysis. We see a new variable here. The orange curve, which has a positive effect on the release is, again, the saturated zone ground water specific discharge multiplier, which increases the release. But, we also continue to see some of the variables that affected the release coming out of the EBS as being important. Here is the pH variable still having a negative effect.

Here is the variable related to solubility still having a negative effect. Then, if we continue on over, we go--the release coming out of the saturated zone, also defines the concentration in the ground water that the RMEI is exposed to. So, from this release we get time dependent doses to the RMEI. So, what we have on this plot is the dose to the RMEI as a function of time.

Now, unlike the other results we were seeing on the preceding poster, we were looking at expected dose results, we’re not looking at expected dose results here, we’re looking at the dose that results to this hypothetical individual given this particular event as a function of time.

And, the uncertainty here, if we look at a comparison plot, is really being driven by the uncertainty in the concentration of Neptunium in the ground water, the variables that affect the actual conversion from concentration in water to dose have a very small effect, because what we have on this scatterplot here is the release rate at the location of the RMEI. And, here, we have the dose to the RMEI at 10,000 years. This is the release rate at 10,000 years.

So, essentially, the spread of values going from here to here is showing you the uncertainty in dose that derives from the uncertainty in the concentration in the water. And, the vertical spread is really showing you the uncertainty that comes from the variable related to the conversion of concentration in water to dose. So, you’re seeing a spread of two, three, four like that, and a spread of multiple orders of magnitude due to the concentration effects.

DIODATO: Can we also infer anything about the performance of the saturated zone from this plot, in terms of the hang-up in the saturated zone, or not?

HELTON: Well, I think the plot that tells you the most about the saturated zone is this one right here, because, you know, this is the integrated release out to 10,000 years from the unsaturated zone. This is the integrated release out to 10,000 years from the saturated zone. So, what you’re seeing is--you can get up to five orders of magnitude reduction in the release over 10,000 years due to the effect of the saturated zone. But, then, again maybe you’ll get considerably less.

DIODATO: This is a very surprising result to get this kind of performance out of the saturated zone, I would say, to me. It’s interesting.

HELTON: Now, if you were--you know, the nature of this plot here, the results you’re seeing on it is going to be very dependent upon the properties of the individual radionuclides. For example, if we were looking at removing dissolved Plutonium, you would see a great deal of hold-up. All of your points would be down here. There would be almost no points up here, because Plutonium is really held up in the saturated zone for most sample elements. On the other hand, if we were looking at Technetium 99, which is not held up very much at all, almost all your points would be right along in there. So, it’s very radionuclide dependent.

KADAK: So, what does this say about the effectiveness of the natural barriers?

HELTON: It says there’s considerable uncertainty in their effectiveness. Some parts of the recognized uncertainty range, you’re getting a great deal of hold-up, and at other regions, you’re not getting a great deal of hold-up.

KADAK: On average, based on all the analysis? What does it do for you?

HELTON: It may do a lot. It may not do much. What can I say? I mean--

KADAK: You’ve done the analysis of all of the isotopes that are of interest, and when you add it all up, what is the net effect of the natural barrier to retain some of these things? Are we completely relying on the engineered barrier?

HELTON: The engineered barrier plays an important role. The extent of it varies a great deal by radionuclide. The natural barriers, for Plutonium, are very effective. For Technetium, they’re not very effective. Iodine, they’re not very effective. For Neptunium, it’s kind of an intermediate thing.

How are we doing on time here?

MOSLEH: Basically, you’ve exceeded--

HELTON: Well, why don’t--I’m happy to step down. You’ve seen the general nature of the results that we have. There are many more results of this type.

MOSLEH: Well, it depends on--we have another 15 minutes. It depends on whether we want to go to basically a discussion session, or let you continue and then we won’t have any separate time for discussion.

GARRICK: Well, I do--I have offered Dr. Gilinsky a time on the podium not too distant from now, because he has a time issue. So, maybe between the presentation and the discussion, we can do that.

How much more presentation do you feel you have, Jon?

HELTON: I have another poster like this for Technetium 99 for the seismic ground motion damage. Basically, this is it. It’s the same type of poster. It is for a seismic event. I labeled it ten years, it actually occurs at 200 years. That damages a codisposed waste package, causes a certain damaged area. It’s the same type of analysis. I think the most basic result is you start looking at the release, here’s the release of Technetium 99 going out of the EBS from this event. Here is the release of Technetium 99 going out of the UZ. Here is the release of Technetium 99 continuing out of the SZ. And, as you look at the similarity of those three plots, you can see there is not very much hold-up going on. They look about the same. If you drop down, here are the same type of in/our plots I was showing you. This line corresponds to one order of magnitude reduction. So, you’re seeing over 10,000 years, you’re getting maybe a factor of twoish hold-up or less in the UZ, a factor of twoish hold-up again in the SZ, a few sample elements giving you up to an order of magnitude hold-up.

And, then, here is the scatterplot of release out of the SZ versus release out of--pardon me--versus dose to the RMEI. And, how you see that the spread this way is really not much bigger than the spread that way. So, now, you’re seeing that the uncertainty in the conversion from concentration of Technetium 99 in water to dose to the RMEI is having an effect on the dose. It’s almost comparable in size to the effects of the uncertainty in transport to the RMEI.

And, then, we have, you know, selected scatterplots here of the same type we were showing you, that illustrate the effects of individual variables. And, also we have partial correlation analyses that, again, show you that we’ve gone in and looked to see how the individual uncertain variables were affecting the transport of Technetium 99.

But, the real basic point of this, I guess, is two things. One, there’s not a lot of hold-up of Technetium 99 going on. And, two, we can carry out sensitivity analyses to see which of the uncertain variables are driving the results.

DIODATO: I have one question very quickly. Were the peak ground velocities treated as an uncertain variable? That was an input that was treated as a known quantity, or, say, the frequency of seismic events, was that a known quantity in this analysis?

HELTON: Was what a known quantity?

DIODATO: Peak ground velocities?

HELTON: Oh, peak ground velocities for seismic events?

DIODATO: Yes, was that treated as--

HELTON: No. the occurrence of seismic events is characterized by what’s called a hazard curve, which is essentially--basically, it’s a complementary--distribution function, except you have exceedence frequencies on the ordinate rather than exceedence probabilities. So, what you have in the hazard curve really defines the frequency at which peak ground velocities of different sizes are exceeded.

So, using the hazard curve, you really end up with two things. BB composed into two things. One is a rate of occurrence of seismic events. And, then, a distribution of peak ground velocity, given the occurrence of a seismic event. Then, almost immediately, this stress corrosion threshold variable comes in, because then you go for that variable and the hazard curve, and you can calculate the frequency of damaging seismic events, and the distribution of damaged area, given a damaging event. And, that’s really what gets, you know, put into the analysis.

The results that you’re seeing--

DIODATO: Okay. So, you included the hazard curves were included in--the parameters that describe the hazard curves were included as uncertain variables in this analysis?

HELTON: The--are you talking about the seismic analysis in general, or are we talking about what’s on the screen right now?

DIODATO: The drivers, yeah, in general for your uncertainty analysis.

HELTON: In general, in this analysis, the hazard curve itself was not treated as being uncertain.

DIODATO: Good, that’s what I wondered. That was my question.

HELTON: Yes, the hazard curve is not treated as being uncertain. These results here are conditional on a particular level of damage with no probability of occurrence incorporated into them.

MOSLEH: I think we need to move onto the--

HELTON: Okay.

MOSLEH: Thank you very much. We have a public comment, and actually at this time, this concludes the TSPA part of today’s hearing. So, with that, I also turn the--

GARRICK: All right. Well, I think before we have our break, we will give the podium to Dr. Gilinsky, and let him make his comments, because he’s got an airplane schedule to meet.

Why don’t you come up here?

GILINSKY: Okay. It’s very kind of you. I’m Victor Gilinsky. I’m a consultant for Nevada. I’m only going to take a minute or two of your time.

I’d like to go back to the subject of drip shields, which got a little bit of discussion here. The discussion proceeded, and all of DOE’s calculations, on the basis that the drip shield was going to be there. And, DOE does not present calculations for the case where you don’t have a drip shield.

Now, is it really reasonable to leave that case out? As you know, DOE doesn’t plan to put the drip shield in until about 100 years from the start of the repository. There may be all sorts of physical difficulties in installing a drip shield. I’m sure you’ve heard about this from briefers. But, just apart from that, if a government agency tells you they’re going to spend billions of dollars 100 years from now, do you take that to the bank? It seems to me you really have to consider this case.

Now, as I said, as you know, DOE has not presented an estimate for the no drip shield case. But, they have presented a early drip shield failure case, and you can use those numbers to estimate what happens if all the drip shields are gone. Now, if you multiply those numbers together, you end up with something like 100 millirem per year at about 2,000 years. Now, that’s way over 50 millirem per year, which is the operative standard at that point.

Now, in one of his backups, before Peter jumps up, in one of his backup slides, he said that there should be a factor of ten that you take out, that they did not. Now, I would say that DOE really believes that factor of ten. I don’t know why they didn’t put it into the early drip shield failure case. But, even conceding that factor of ten, you’re down to about 10 millirems per year, which is kind of in the ballpark of 15 millirems, which is the standard.

So, it’s not the slam dunk that you saw in the final curves that were presented. And, it seems to me my suggestion to the Committee would be that you ask DOE to present calculations for that case.

Thank you.

GARRICK: Thank you.

All right, I think that brings us to a break point, and according to our agenda, we have a ten minute break at this point. Okay?

(Whereupon, a brief recess was taken.)

GARRICK: As we indicated earlier, we are reversing the last two items on the agenda, that is, the last two technical presentations. And, we will now hear from the Electric Power Research Institute on their Yucca Mountain Performance Assessment model, and the presenters will be John Kessler and Andrew Sowder. John?

KESSLER: Thank you, John.

I’m going to kick this presentation off, and then hand the majority of it off to Andrew Sowder.

Before we begin, I cannot help myself but to address one of the interactions that occurred between Doctors Helton and I think Kadak on, you know, gee, this isn’t showing very much natural system performance when we’re looking at Technetium and Neptunium. That may very well be true, but we are looking at two long-lived radionuclides which comprise an extremely small component of all the radionuclides that are in the commercial spent fuel or the codisposal waste.

The vast majority of radionuclides could easily be held up to the point where they decay late and near nothing in the time periods of involvement. What you’re left with at the end are these handful of radionuclides that do have very long-lived half lives, and do cause more of an issue. But, let’s not forget the very powerful potential performance of the natural system for holding onto the vast majority of the radionuclides that are in commercial spent fuel and high-level waste.

Okay, I’d like to say that I’m very pleased to have Andrew Sowder join the EPRI time. In a short five months that Andrew has been with us, he has really picked up, come up to speed on Yucca Mountain work in general, and certainly the work that EPRI has done in specific.

So, I’d like to say a few words about Andrew, and, also, I wanted to mention we’re grateful to the Board for switching things around, as one of the people supporting this talk has an early flight also.

Andrew is a Bachelor’s and Master’s in optics from Rochester, and a Ph.D. in environmental engineering and safety and science from Clemson. He’s had various positions, including a AAAS Fellow, which he spent at EPA, and his last job was with the State Department looking at coordination and oversight of U.S. policy on radiological security. Andrew’s background is an excellent fit for both the industry needs, as well as the work on this kind of project.

So, Andrew?

SOWDER: Thanks, John, for the introduction. And, I also want to thank the Board, Chairman Garrick, as well as the Staff, for inviting us today to speak. And, I want to recognize John, my boss, as well as Dr. Mick Apted, who is also in the audience. And, since I am the new kid on the block, any questions that you ask, once I scratch beneath the surface, I may very well look to them for some of the detailed background on this topic.

So, today, I'm going to focus on what EPRI has been doing in terms of Total System Performance Assessment for Yucca Mountain independently of the Department of Energy, give you a sense of what our role is in terms of background, as well as our approach, and we will focus on our own Total System Performance Assessment code, the IMARC, and the results mainly. I will not focus so much on what the individual components are, you can read about that in our publicly available documents, I encourage everyone to go look on our website, or contact us if you can't get ahold of certain documents. They are publicly available.

I'm going to focus the talk on the 10 to the sixth million year period of performance, and we're going to go into the nominal scenario, the seismic ground motion scenario and the igneous intrusion scenario since especially those second two are the ones that are the chief risk drivers for the Department of Energy's TSPA work. And, finally, hopefully wrap up with a short comparison with our work and DOE's, and give a short summary.

I do want to make a note that this work kind of represents--we're in between versions right now, so some of the results you see are IMARC 8, some are from our latest version. This is kind of an artifact to the fact that we necessarily lag behind DOE, because as things change designs, the TADs, for example, we know about that when that becomes publicly available. So, that’s one thing to keep in the back of your head.

So, EPRI has been tracking and reviewing a lot in terms of Yucca Mountain, including preclosure, postclosure, transportation, as well as, of course, the license application. But, today, I’m going to focus on postclosure, TSPA, and again, the nominal case as well as disruptive events.

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As many of you know, EPRI has been doing this work since--for two decades now, since 1990. And, again, I would point out my boss, John, has been working on this for a large fraction of this, and Dr. Apted for even longer. So, again, within the room, I’m very comforted to have them as resources here.

Again, much of the published work out there is IMARC, Version 8. However, we are developing Version 9, and we also are completing a peer review by an international team of experts, and that will be reflected in that IMARC, Version 9.

But, the most important thing here is the EPRI work is intended to provide independent, technically defensible assessment of Yucca Mountain and its performance.

I think this slide is useful in showing kind of our relationship to the Department of Energy in that, in essence, we kind of consider our work to leapfrog from each release of Department of Energy results. We then respond, update our work as we see fit.

I did say we’re independent, but only to the extent possible. Again, a lot of the site specific characterization data is, of course, through the resources available to the Department of Energy, as well as things like design changes, and things that we have no control over.

Now, the early work that EPRI did, in fact, demonstrated the feasibility and utility of the TSPA approach for identifying and reviewing these FEPs, key features, events, processes, evaluating alternative conceptual models, as well as investigating or doing “what ifs” on certain scenarios and the FEPs, such as should colloids be included in the modeling. And, again, as I said, the TSPA evolves in response to new data. Our own work, we do sponsor research and other advances in scientific understanding, as well as, of course, repository design changes.

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So, the EPRI approach, we’re here to focus on risk important FEPs. We’re not going to go down every rabbit hole chasing to low risks and probabilities. And, that’s rooted in reasonable expectation and performance criteria set forth in 40 CFR, and, one of, of course, the primary outputs of our work as well as DOE’s, is the mean probability weighted dose to the RMEI. However, as with any model, the understanding of the underlying processes is as important in terms of understanding the performance.

Now, in contrast, the Department of Energy’s approach, we have taken a different one in terms of addressing uncertainty, which we consider to be equally valid, as well as appropriate for our purposes, and that’s using the Event-tree approach, whereby each branch, and I’ll show it on the next slide.

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So, this is one of the big differences between our work and maybe the larger DOE effort is basically addressing or capturing variability and uncertainty in the models as well as the parameters through an Event-tree, where each node typically has two or three different branches, and to each branch, you assign a probability, and for each model run, you essentially explore every single branch on the tree. So, in this way, we are able to, through a limited number of realizations, capture what we feel are central tendencies as well as extremes.

However, in certain independent sub-models, we do employ Monte Carlo sampling techniques, such as our engineered barrier system degradation model, and those outputs are fed into our main IMARC code as a look-up table, for example.

KADAK: Just a question on this one. What are the basis for those numbers?

SOWDER: The basis of those numbers, I would say, in general, a lot of this is based on professional judgment and opinion by and large. But, for a lot of them, what you’re trying to do is also capture perhaps a distribution where you’re taking a continuous distribution, and trying to perhaps represent it using low, medium and high range. So, in essence, trying to collapse a continuous distribution function into a triangular distribution, using three points. But, a lot of this is based on professional judgment.

MOSLEH: Do you do that for alternative models also?

SOWDER: Again, there’s a lot of branches on this tree that have been pruned off or no longer are used to explore different--it depends on the extent of work I think we see needed. But, it is applicable to any--

KADAK: Have you done sensitivity studies on varying those numbers?

SOWDER: Yes. And, again, as I will get to later, that’s one of the things we’re in the midst of now, is really understanding the key parameters, and what are the key drivers in terms of uncertainty.

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But, again, EPRI does not pretend to duplicate DOE’s work because, again, we I think have different purposes and roles. Obviously, we are not applying for a license. We’re again independent in the process. So, we’re not here to equal DOE’s calculations in their rigor and their depth. Instead, we’re here to look at those key risk-important FEPs. And, we’re also not here to necessarily yield identical results. The point here is that we’re supposed to provide independent analysis. However, in many cases, I think you will see the results are similar or explainable in terms of conservatisms, et cetera.

GARRICK: But, the importance of the FEPs is something you determine?

SOWDER: Yes, we have gone through and, you know, ourselves have screened the key FEPs as well. And, again, there are certain FEPs that obviously are still included in the DOE’s work that we either have screened out or later determined to be negligible.

Again, getting back to the touchstone of our approach is this idea of reasonable expectation. And, the last point is the one I’ll just highlight, is that it basically guides the assessment to focus on defensible and reasonable parameters rather than on extreme physical situations and values.

So, in summary, just the flavor of our approach is, we would characterize it as reasonable to somewhat cautious. Again, emphasizing what is likely to happen versus bounding cases. And again, this is getting back to where we select our parameters on our event-tree, we do use significant use of expert judgment, what processes are likely to happen, are the processes risk significant, and in terms of given the amount, limited amount of data, and also to determine best estimate ranges to capture this variability and uncertainty.

A quick snapshot of what goes into IMARC again. By and large, similar to how DOE has approached their TSPA components focused on infiltration and seepage, near field modeling of the in-drift and in-package chemistry. A lot of work continues to go into reviewing and updating our waste package degradation model, including drip shields, as well as cladding. We do take account of cladding as a barrier. And, also, near field release and transport modeling, which is then coupled back into the unsaturated zone, liked to the saturated zone, and then out to the receptor, the RMEI at 18 kilometers.

So, just quickly, on infiltration, we have essentially based a lot of our infiltration work on the last DOE modeling effort, TSP, I believe it’s SR, and we initially, for a long time, had three climate states for the first 10,000 years, and we have since now just gone with the post-10,000 year scenario in terms of infiltration because we do not see much of a difference for our modeling. And, that’s because not much happens in the first 10,000 years in our models. So, that’s just one example of constantly revisiting work and trying to streamline it based on this significance.

Again, our seepage estimates are derived from the previous DOE model, and in general, we are continuing to look at the climate model, although it’s prescribed in the regulation, or will be prescribed in the regulation for post-10,000 years, we are finding that a drier future climate is indicated, which suggests in general the net infiltration rates are conservative.

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This is a near field, looking at the near field different components. Basically, we do look at the Alloy 22 outer barrier of the waste package, the titanium drip shield and classing in our EBSCOM model. Again, nothing new here. This is just what one would expect. But, again, we do take credit for cladding in the fuel.

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Waste package degradation is modeled in our EBSCOM component, and it is run separately using full Monte Carlo sampling of uncertainty and variability. Basically, a mean cumulative failure rate curve is generated, and that’s input into our IMARC analysis. What we have found through our modeling is that the primary failure mode is by general corrosion. Again, this is a departure from some of the DOE results. We do not see stress corrosion cracking of the welds as a significant contributor. And, our mean waste package lifetimes typically exceed a million years.

LATANISION: Latanision, Board.

Can you explain why you’ve made that judgment?

SOWDER: In terms of the stress corrosion cracking?

LATANISION: Yes.

SOWDER: I believe a lot of that derives from our seismic modeling in terms of the amount of jostling of the waste packages, that our peak ground velocity, our maximum peak ground velocity I believe is .75 meters per second, and that’s substantially lower I believe than the upper range of the DOE modeling. And, again, that was based on our expert judgment, as well as--and, we continue to look at that in terms of looking at precariously balanced rocks in the region, to come up with that.

John, did you want to say--

KESSLER: Yes, John Kessler, EPRI.

There’s another component, which is the stress threshold, above which stress corrosion cracking would initiate. I believe that we’re at or above the range that you saw described this morning in the margin analysis. And, when you combine the fact that we feel that the seismic energy is lower than what DOE is estimating, we take that failure threshold into account. The way we’ve modeled it, we wind up with very few stress corrosion failures. It’s dominantly to general corrosion.

KADAK: Question. Kadak.

EPRI, as I remember, several years ago did a lot of work on seismic hazards. Did you apply that work in estimating the likelihood of certain frequency earthquakes in this area?

KESSLER: Yes and no, Andy, in the sense that what we looked at primarily was what we wanted--we focused actually on the upper end. We looked at some lower peak ground velocity cases. But, in terms of putting together and looking at, say, a shack type of study here, we didn’t formally do that. What we looked at was what we thought a reasonable range of peak ground velocities were, felt that kind of the upper end was .75. Once you get much below that, you get no stress corrosion cracking, and that’s essentially how we folded it into the model.

SOWDER: Next slide, please? Again, unsaturated zone is treated as 1D vertical columns. We do have the option of ability to model multiple columns, but, again, over the years, we’ve determined that actually a single vertical column works for our purposes. We have the capability of single porosity permeability or dual porosity permeability. And, then, we have subdivided the underlying geology underneath the repository into four--again, a simplification from what the Department of Energy has used--into four different zones, based on, again, professional judgment and looking at the site characterization data.

The saturated zone model. What I will mention here is, again, this is a fairly mature model, similar to that used in other applications. Have a lot of confidence in this. The model itself hasn't changed much, although the parameters may very well, upon review.

Finally, of course once you get to the biosphere, the receptor at 18 kilometers, again, we are following the regulatory specified criteria, basing our exposure characteristics on current habits in the Amargosa Valley.

The one thing I will point out here is our current BDCFs, biological dose conversion factors, are still conservative, and we are moving to a more probabilistic basis for these. So, that's one area where we recognize we were actually still overly conservative compared to DOE.

Next slide?

So, this is kind of an important slide for the talk. This is our results from the nominal scenario, and, again, this is for no seismic, rock fall, or igneous intrusion. And, our assumption is one undetected--I believe one undetected manufacturing flaw amongst the repository leads to an early failure. And, again, that’s where you will see these releases of Iodine 129 and Technetium 99.

And, actually, getting back to the issue of the geological barrier, this one single early release kind of acts as a demonstration of the function of the geological barrier. While you do have these mobile nuclides coming off fairly quickly, all the other ones are significantly retarded. So, this is, you could think of this as, in essence, a demonstration of the function of the geological system.

KADAK: Is that one package failure?

SOWDER: Yes, only one, yes. Now, we, again, do sensitivity analysis. I think I even have a slide later on on a thousand early failures as well. But, this is what we consider to be the most likely best estimate case.

KADAK: And, this is at time when?

SOWDER: I believe that’s immediate. Is it immediately after closure?

Now, in terms of the million year time frame, the thing to notice is that our peak dose here at a million years is on the order of 0.04 millirems per year, with Iodine 129 as a dominant nuclide. Neptunium and its daughters are also prominent dose contributors. But, again, we are three to four orders of magnitude below proposed or envisioned regulatory limits here. And, we're also probably about an order of magnitude below the Department of Energy's numbers.

ARNOLD: Past regulatory space.

SOWDER: Past, what, the period of geological stability.

APTED: I think it's about 5 million years. It comes into the--

ARNOLD: I noticed your previous curve on the failure of waste packages was 10 million years.

SOWDER: Yes. And, again, that was run separately in our EBSCOM model, and I think that was to come up with some kind of a--take a look at the mean package lifetime. But, being a sub-model is probably less intensive to run that.

Again, I'll make the comment that these results are still pre-TAD. We haven't even figured in the design changes due to the TAD. I will also make the disclaimer that we do not have the codisposal packages figured into our inventory yet. But, we have already done some preliminary estimates on those as well, and based on what we've seen so far, with the TAD improvements in terms of--it's a more robust package, and based on our assessments of the codisposal package, we don't envision our results changing substantially. If not, performance may actually improve.

So, next slide, please?

A lot of the rest of the talk I want to focus on just looking at those scenarios contributing to long-term peak dose and DOE’s assessment, those being igneous intrusion and seismic ground motion.

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And, here is a significant point of departure from the Department of Energy modeling. We started out by looking at appropriate analogs for volcanism at Yucca Mountain, and based on that, we pretty much, I believe, limited the scope to really looking at the Lathrop Wells as the appropriate analog.

As a result of this work, which we’re looking to publish, you end up with high viscosity magma flows, which implies limited intrusion into the drifts. And, it also calls for kind of distinguishing between waste packages, even within a drift, in terms of the effect. We have, I think, termed them red, red packages which are completely engulfed in magma. Then, you have a blue zone, which still receive impacts in terms of thermal, and then once you get out here into the green, even within a drift, you can envision minimally impacted containers.

We did look at multiple failure mechanisms, including the over-pressure, localized corrosion, and also increased failure rate once the repository cools down, or the drift cools down, and rewets. But, again, a rough estimate of probability-weighted dose remain relatively low, below a tenth of a millirem per year. And, I will go into more detail in terms of our best estimate case.

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I want to point out that this is again a conditional dose. This is assuming that the event actually occurs, and I believe we set our event, igneous intrusion event at 1,000 years.

And, again, unlike the Department of Energy scenario, the dike intersects the repository, but essentially only comes into contact with 14.4 percent of the drifts. And, let's see here--

KADAK: Are you sure about the .4?

SOWDER: I could probably be convinced to take that down to 14 percent, or 15 percent. I'll take that into consideration. And, contacts 5 percent of the waste packages in each drift, or .7 percent of all waste packages in the repository. And, it only impacts 20 percent of the non--okay, it contacts 5 percent of the waste packages in the drift, and then it impacts an additional 20 percent of the other waste packages in each drift. And, again, I think this number here is wrong. It should be 1.7. But, yeah, who's counting.

But, in terms of just doing a very--actually, what would be a conservative estimate of the dose, again, it's less than .1 millirem per year. Again, this is an earlier modeling, so it’s not--some of the nuclides are maybe a little different, but basically, what you’re seeing is the big difference here is this early dose here.

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KESSLER: Additional figures here, but I believe that that 28.8 percent of waste packages actually should be 2.9, 3 percent of waste packages, we got our decimal off one place.

SOWDER; Yeah, okay.

KESSLER: So, that number there should be essentially less than 3 percent of the waste packages.

SOWDER; I think the overall conclusion here is given the physical constraints of the magma flow, that really a more realistic igneous intrusion case impacts a much lower percentage of the repository.

And, here’s one of our most conservative cases where each drift, if each drift is filled completely, however, we’re still only assuming that, you know, 14 or 15 percent of the drifts are in fact contacted. A peak dose here is still less than 1 rem per year.

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The second disruptive scenario that we looked at is, of course, seismic and the associated rock fall scenarios. And, here, we looked at both the canister themselves moving around, jiggling around on the pedestals, and into each other, as well as dynamic and static loads caused by rock fall.

And, here is our peak ground velocity, in general, again, this is pre-TAD now, we still found robust behavior of the waste package, and really minimal effect on the package integrity.

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Again, remembering our peak ground velocity, our base case is essentially a seismic event with a return frequency of 100,000 years, and 100 percent of the drip shield fail with the first event.

What we have shown here is basically the nominal case of the waste package failure, and then the additional effect of these episodic events. And, I believe that’s fixed in time with a fixed return rate of 100,000 years.

Here, we find seismic failures dominate up to 500,000 years, and again, I believe that’s primarily just due to the package to package contact, and damage to the waste packages. And, the corrosion then dominates after that.

This is again earlier model results. Here, we show 15 percent of the waste packages failing at a million years. And, less than 1 percent due to actual seismic.

So, looking at our dose to RMEI at a million years, again, 0.04 millirem per year approximately. And, again, only 15 percent of the waste packages failed.

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As an example of the fact that we do care about, you know, sensitivity and performance of our model, we have varied in terms of the number of waste packages failing, this 44 percent failed at 10 to the 6 years, and then we have 100 percent failed after 500,000 years. Again, you can see the end result here is increasing the dose from .04 millirem per year, up to .2 millirem per year. So--

DUQUETTE: Duquette, Board.

If I look at those curves, you're making an assumption I think that the first failure--or the first event is at 1,000 years. If you move the event to 100 years, or ten years, as DOE has done, the shape of your curves implies that you're going to get about an order of magnitude higher with exposure if the seismic event occurs earlier. Isn't that correct? Am I missing something?

SOWDER: Well, actually, I think the first event is probably before 100,000 years, just prior to 100,000 years.

DUQUETTE: But, your curves are increasing. It looks to me like if I translate them to the left, that you get to some pretty high numbers, unless you calculate them out beyond that.

SOWDER: Well, pretty high numbers?

DUQUETTE: Well, an order of magnitude, moving it over one order of magnitude on the axis--

ARNOLD: They all have to fail.

KESSLER: This is John Kessler from EPRI.

First of all, remember you’re looking at a log time axis. Okay? So, shifting the whole thing to the left by a thousand years barely shifts the right part of that axis at all. You’re moving it over one one/thousandth of the way. So, peak dose at a million years is really not going to change.

DUQUETTE: Isn’t that a log curve on the “Y” axis as well?

KESSLER: Right. But imagine shifting the “X” axis over by a thousand years when you attended the six years, you’re not shifting it over any significant amount at all.

SOWDER: And, I’ll show a--we’ve done, even for a thousand--for example, a thousand waste package failures early on, the effect is really not that significant. So, again, yeah, here’s the example of the sensitivity analysis. I’ve just taken a look at what happens if you fail a thousand waste packages early on versus that one. You do get a substantial increase in the early dose rate here, but that remains less than .1 millirem per year. And, then, you do also of course get an increase in the peak dose right at a million years. But, again, it’s not as severe, and it remains on the order of .1 millirem per year.

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I won’t dwell on this. I just wanted to call out the number of reports, examples of some reports we have done, talking about elimination of colloid-facilitated transport as a risk-important FEP. A lot of this work is documented in our publicly available reports.

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And, here's the--always the required busy slide that's too small to see in the back of the room. I just want to kind of point out just--and you have this, so I won't dwell on it. But, just trying to call out some of the differences between our work and the Department of Energy's work in the TSPA-LA.

One example is what we consider to be a big difference is drift seepage. I believe for a nominal case, DOE is reporting on the order of 30 to 40 percent of the repository sees seepage. And, our work is much lower, on the order of a percent. So, again, this is subject to review, when you're looking at this again, but that speaks a lot to in terms of why our results are the way they are.

GARRICK: Do you have an order of magnitude difference on each of the biological dose--

SOWDER: Yes. Yeah, that's one reason why I have this on here, is I just wanted to point out that this is one area that we are in the process now of revising, moving towards a more probabilistic basis. So, this is one thing that, yeah, we are aware of, and we don't think this is good enough anymore, and we’re going to drop--those will probably drop to more along the lines of what the Department of Energy has.

I’ll also just point out because in the TSPA AMR, there was a very nice section, Appendix M, I believe, did a nice comparison between EPRI’s work and the Department of Energy’s work, and it was pointed out our list of radionuclides is abbreviated and different, and some key, at least according to the Department of Energy analysis, some key nuclides are missing, for example, Plutonium 242 and Radium 226. We have subsequently looked at those. They remain on our second tier, but again, our IMARC 9 will have those in there, and we are looking at those.

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So, kind of summarizing the comparison between the two, in terms of early time frame for waste package failure, I think the Department of Energy has shown seismic damage to those codisposal waste packages, this is from that stylized drawing, which I found actually to be a very useful representation. EPRI, we are essentially finding negligible waste package failure. We assume one, and that’s all we see going on in the first 10,000 years.

Along the intermediate time frame from 10,000 to 100,000 years, DOE reports stress corrosion cracking of the commercial spent nuclear fuel packages, and our work essentially just shows general corrosion is dominant throughout.

LATANISION: Andrew?

SOWDER: Yes.

LATANISION: Ron Latanision, Board.

Is the implication that you assume in your EPRI model a higher general corrosion rate than DOE?

SOWDER: No, I don’t--this is not to say that our corrosion rate is higher. It’s just saying that we’re not seeing stress corrosion cracking.

LATANISION: No, I understand that. But, if you are assuming dominant failure mechanism is general corrosion, are you--is the implication that you’re expecting thinning, wall thinning to be sufficient that you’ve got penetration, or what’s the implication?

SOWDER: Well, I would say it’s the one mechanism that’s actually operating. I’ll let John take that.

KESSLER: John Kessler, EPRI.

That’s right, Ron, in the sense that when you knock out stress corrosion cracking, the next one down on the list is general corrosion. We do believe that with the certain uncertainties and variabilities, there will be some waste packages that will thin all the way down to zero in less than a million years. And, that’s the roughly 15-ish percent of the waste packages that have failed by general corrosion after a million years.

DUQUETTE: Duquette, Board.

What are you using for a corrosion rate?

SOWDER: That's a good question for the--

KESSLER: We're using DOE data selectively in the sense that we're trying to pick up the right data. I'm afraid that I don't have the right guy here to answer that question to your level of satisfaction. We'll have to get back to you on that.

LATANISION: That was really my point, though. My impression was that the DOE argument and presumably yours, was that the corrosion rate corresponded to the passive current density, and I don't think there's any basis that I've seen that would suggest that the passive current density is changing over this period. So, I'm not quite sure I understand, John, your comment. You think some packages have a higher passive current density than others?

SOWDER: And, this may be an artifact of this table, the way it's presented. What I intended to display here is if there's any corrosion going on that's significant as a contributor to eventual failure of the package, it's going to be general corrosion, because we just don't see stress corrosion cracking.

LATANISION: Maybe it's the use of the word failure.

SOWDER: Yes.

LATANISION: That suggests--

SOWDER: I'm not implying necessarily the packages have to fail in this time frame.

LATANISION: Okay. All right. I'll buy that part.

SOWDER: Thank you. I'll remember that for next time.

KADAK: But, it sounds like your general corrosion rate is higher than what DOE has assumed; is that right?

SOWDER: No.

KADAK: Not correct?

SOWDER: No. I don't know what it is, but I can comfortably I think say that it's not more conservative than the Department of Energy's.

KADAK: I didn't hear them say that they had a gross general corrosion failure. We talked about their patches, how they were generated, we don't know, but--

SOWDER: But, from my understanding of their work is that late in the time frame of the compliance period, it is general corrosion that takes out all the waste packages.

ARNOLD: Well, in your curve, they're all gone at about 2 million years.

SOWDER: Yeah. And, when I say--it's the dominant mechanism for codisposal and commercial spent nuclear fuel packages.

KESSLER: John Kessler, EPRI.

Yeah, I think we recognize that not all the DOE waste packages fail in a million years. I think one of the differences we do need to point out is that we don’t do a patch model. We apply the same general corrosion rate over the entire waste package, such that if it’s corroded through 2 centimeters, now 2 ½ centimeters of Alloy 22, we assume that’s the failure. We don’t look at a patch model like DOE does.

DUQUETTE: Duquette, Board.

As long as we’re discussing this in this context, and we have some of the DOE people here, I think even the DOE people aren’t sure what corrosion rate to use. Some of their five and ten year data are questionable at this point, are being re-evaluated. I don’t know if for their models, they’re using the passive current density from polarization curves, or if they’re using the ten year data, or I don’t think any of it is quite clear yet what number is going to be used for general corrosion rates. I would suggest you monitor that carefully if you’re going to use their data.

SOWDER: Well, I can assure you that we are definitely looking at that and following the developments in the project as well. So, the point is well taken. And, again, this is one area that probably receives some of the most active evaluation and review from EPRI as well.

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So, just in terms of just summing up, in terms of one of the key processes in terms of rank, again, I think our modeling is similar in that, you know, we are seeing, of course, similar processes. It’s just a question of the magnitude. Igneous intrusion is probably the dominant in both, but again, I would point out that we are relying on the Department of Energy for the PVHA numbers, and EPRI is now undertaking its own PVHA work as well as a priority, again, for an independent look at that work.

Our nominal case, and really, our seismic case are about the same order of magnitude in terms of ultimate dose. And, we have pretty much--we considered the volcanic eruption scenario to be negligible on contributor to dose.

Next slide?

And, as I mentioned, this is about the kind of the iterative process that we take in terms of making sure that our model is still relevant, and applicable to the Yucca Mountain project, and also meets muster in terms of Q/A. We are wrapping up this IRT review of a three member team of international experts, and the key question is is our model really fit for purpose. And, that should be coming out as a separate publication later in the year. And, the suggestions and recommendations will be incorporated into our new model, new version.

Next slide?

ARNOLD: Could you name the experts?

SOWDER: I’ll turn that over to John, if at this point we can.

KESSLER: Nava Garisto is looking at EBS performance, backed up by David Bennet, who is looking at biosphere issues, as well as flow and transport issues, and Johan Andreson, who works in the SKB project, is looking at a lot of the natural system flow and transport issues.

SOWDER: And, just in terms of what we’re doing with our IMARC 9, it is a major update, especially of the documentation and the Q/A. We are, as I said, incorporating these independent review team recommendations, and we are also updating some of our conceptual models as well.

Next slide?

And, major efforts we’ll focus on, of course, incorporating the TAD design, looking at the repository inventory, adding the codisposal waste packages, reviewing our radionuclides, again, continuously looking at the materials issues on corrosion, degradation. Right now, we have material degradation study in progress looking at pit stifling, localized corrosion. And, then, we are also conducting our own probabilistic volcanic hazard analysis.

And, the last slide--

GARRICK: On that previous slide, what’s the level of effort?

SOWDER: In terms of?

GARRICK: People working on the project, or whatever metric you want to choose.

SOWDER: Well, I can say for EPRI, it’s now a two person team, and then in terms of our--the thing that John and predecessors have assembled, it’s probably a total of a dozen people working.

DUQUETTE: As a corollary to that, Duquette, Board, what fraction of the work is being done within EPRI and what fraction is being contracted out to other laboratories, other organizations?

SOWDER: Well, I would say the majority of the actual modeling work is done through our contractors, including Dr. Apted here kind of acts as our primary coordinator. And, our role is of course is to then consolidate, direct, consolidate and assimilate and analyze those results. But, the majority of the work is done through our contractors, which a lot of these have been with us for many years. Does that answer the question?

DUQUETTE: Yes, thank you.

SOWDER: So, I hope I have left you with at least a better idea of IMARC, our program in terms of how it came about. Probably the most important result is perhaps that our analyses indicate that the dose to the RMEI will be well below any draft limits if and when--or when they become final. And, also, we are in the process of revising our model, our system, to align it more with design changes, but we do expect overall repository performance to compare to previous work, if not exceed it.

So, that’s my talk. Thank you.

GARRICK: Thank you. Andy?

KADAK: Thank you. Your dose curves look very different than DOE’s dose curves. Could you try and explain the difference?

SOWDER: Well, I would say--let’s go back to Slide 18. In terms of--you can probably compare them in terms of--you typically have some early feature and a late feature, early failure versus late failure. A lot of the differences may arise from the radionuclides considered--included. And, I see--

APTED: Let me help you out. Mick Apted, Monitor Scientific.

Again, compared to the early morning when Peter was presenting, he had linear time, okay, along the bottom. That’s a log time. Anybody who does these kind of plots, you’ve got to realize that lower left quadrant, don’t put much weight, you’re talking about trivial doses over time frames that are sort of inconsequential compared to later. The biggest difference is we’re not looking at the codisposal package. Okay? So, as the DOE analysis, that early time, that first 10,000 years, is being dominated by releases of a package we haven’t yet considered. Okay? So, that’s maybe the missing chunk, if you will, on the left-hand side. We haven’t considered that. So, on the other hand, our commercial spent nuclear fuel packages compared to their, if you separate out their--for those same packages, look very similar in terms of shape, once you put them on the same log dose versus linear time. So, they don’t look that different, Andy, basically.

KADAK: I think in the future, if you’re going to make presentations like this, you ought to do it in a way that you can compare the plots.

APTED: Yeah, possibly. In that case, I would make the suggestion that we do linear dose and linear time, and that would be the most useful and illustrative of what really matters and doesn’t matter. So, I agree with you 100 percent. Let’s go linear dose and linear time, that’s in agreement. All those in favor?

KADAK: The other comment I’d like to make is it was really hard to understand what assumptions you made to be able to say these look reasonable versus not reasonable. And, you said you rely a lot on expert judgment to determine what kinds of models would fit into this code of yours. I also think that the next time you do such a presentation, you kind of explain that and compare it to what DOE has done. I think the only thing you said was you included the cladding as a barrier. And, there’s a lot of detail that is missing, so those of us who don’t understand the details of your model, can say is this a credible analysis or not, are hard for us to say so.

APTED: Give us a day, Andy.

KADAK: Pardon?

APTED: Give us a whole day.

KADAK: A whole day, okay.

SOWDER: Because trying to introduce the model, and then also go into a full comparison with DOE is quite a weighty task for even the time that I went over.

APTED: Mick Apted again, just speaking to that.

Since 1990, we’ve been nothing but interested in doing such a comparison with DOE. Of course, they have their own priorities and schedules of things they’re doing, design, and also, we want to be independent to the point that if we spend all our time meeting with DOE, there’s going to be a sort of homogenization possibly, of viewpoints. So, in some ways, until this license application comes in now, maybe in the near-term future, we can exactly maybe sit down and do that without this worrying of sort of loss of independence. But, that’s been a very guiding star for us, not to get to the point where we’re looking in the mirror to DOE, and they’re looking back at the mirror and seeing the same thing.

SOWDER: Well, in terms of I think the credibility of the work, I think we rely on peer review processes separately in publication and all of our reports that are publicly available as well. So, in that regard, we do pay attention to the credibility of our work, and peer review. And, that's one driving factor for the IRT review as well.

GARRICK: Ron?

LATANISION: Yes, if we could return to Slide 30 for just a moment? This is an observation, and it may be an incorrect observation, but I'd like to get your reaction. There's a lot of discussion of the issue of conservatism in TSPA, and I think Ron Ballinger will talk a bit more about that in his presentation later, and likewise in IMARC, and when I look at this comparison, I see stress corrosion cracking failure in the intermediate time frame. That, to me, is--looks to me like an extreme conservatism because as far as I understand, in terms of the data, there is some indication from Andreson's work that cracks may propagate, but it's very difficult to initiate them. So, there's a real question about whether or not stress corrosion cracking will in fact ever occur. And, yet it does appear as, I would say, an extreme conservatism in terms of the DOE TSPA approach.

And, the converse seems to be the choice of looking at general corrosion as a failure mechanism from the point of view of IMARC, because that's the very slowest process in terms of any of the corrosion modes that might be of interest. And, unless you can definitively exclude the others, like stress corrosion cracking or localized corrosion or pitting, some of which we can very clearly exclude, it seems to me that that’s the other extreme in terms of this issue of conservatism. Is that observation correct, or am I misrepresenting what I’m seeing on this table?

SOWDER: Well, before John speaks, I’ll say, one, I should have probably represented, instead of failure, I should have said degradation.

LATANISION: Okay.

KESSLER: In work that EPRI has published already, Ron, we do go through all the potential failure mechanisms, provide justifications why we can rule them all out except for stress corrosion and general corrosion, in terms of contributing in any significant way to waste package failure leading to release of radionuclides. So, we do have that basis.

LATANISION: Did you say you could not exclude stress corrosion, or you could?

KESSLER: We could not. We have a very low probability, or very small fraction of our waste packages that fail by stress corrosion cracking.

LATANISION: Okay, maybe I missed the point then, because that does not appear here.

KESSLER: And, the--showing are dominant failure mechanisms, not all the potential failure mechanisms.

LATANISION: Again, dominant is missing.

SOWDER: Point well taken.

LATANISION: So, maybe it’s an incorrect observation. Maybe that’s the conclusion.

SOWDER: Well, I think--

LATANISION: I’m quite serious, I mean, the issue of conservatism is one that can work both ways.

SOWDER: Well, and again, that’s in our--I think our credo here is--I mean, that is what drives us, is to constantly look for ways to take conservatisms out. Now, again as DOE mentioned, sometimes you can’t take--it’s probably not the best approach to take a less conservative approach if you don’t have a good alternative. But, again, we are not in the same position the Department of Energy is in, and we have a lot more, I think, or a better position to take perhaps the best estimate approach. And, that is, in essence what I think guides us in our work.

APTED: Mick Apted with Monitor Scientific.

Going back to an earlier point I think Andy Kadak raised, but it fits into this slide, the earlier the corrosion and calculations of the IMARC 8 are based on a much thinner, or somewhat thinner, I think 2 millimeter Alloy 22, the pre-TAD, so when you say how many packages are failing by general corrosion comparing our approach versus what you heard today in the morning from DOE, I probably think, Peter, they’re probably using the thicker TAD Alloy 22. So, that may be the reason. I do believe they probably have fewer general corrosion failures at a million years than our previous IMARC 9.

But, remember this leapfrogging that Andrew pointed to. We’re in the point of now catching up with the new design and what we’ll probably see is that we have now fewer packages failing by general corrosion, and again, I don’t know whether that, because we’re doing it independent of DOE, I don’t know where that will come in, but that’s a distinction to be made.

GARRICK: Okay, Ali?

MOSLEH: Mosleh, Board.

If you could go to Page 7, my question is I’ve tried to understand your general methodology. This looks to me, and I’m not splitting hairs, but it’s a little bit more a decision tree, where you’re looking at various combinations of parameters that you seem to have discretized to a few values each; right?

SOWDER: Except there’s no decisions.

MOSLEH: Yeah, I understand. But, just kind of basically to keep track of the possible combinations of different variables.

SOWDER: Right.

MOSLEH: And, therefore, I’m assuming that then you take these combinations, according to different pack, and then you do the rest of the analysis using Monte Carlo approach?

SOWDER: No, in essence, I mean, we have some Monte Carlo analysis feeding into this through separate sub-models, but, no, the entire is through a series of these branches that together, when you go through a realization, it’s, you know, a single combination of different branches all the way to the end. So, you end up with a fixed number of realizations.

MOSLEH: So, that’s combinations of physical parameters. Do you also have physical events, the typical event tree, what--your events to occur?

SOWDER: Yes, seismic is treated separately as a different scenario in terms of--but, I think it’s been pointed again in the Department of Energy’s Appendix M, is that I don’t think we address, I believe aleatory uncertainty in the same manner or if at all in terms of--

MOSLEH: That’s happened--

SOWDER: Yeah, I mean, my results I showed were conditional probabilities, and I think that’s something that we might look at in the future. But, we haven’t done that.

KESSLER: John Kessler, EPRI.

We do have a combination of not only parameter uncertainties here, but conceptual model uncertainties, for example, focused flow factor there is one example of where we have a different conceptual model of flow--or percolation in the near field, and we assign probabilities based on information that we think is appropriate there, and come up with a focused flow factor based on our use of DOE data, as well as other data to come up with that.

The other point that we keep trying to make with some people is that we could go and have continuous distributions of all of these parameters, like you've been seeing from DOE, and I think that what we're concerned about is the details getting lost in treating 300-some parameters with continuous distributions. In this case, we feel it's a much cleaner approach. We do not think we're losing much in the way of fidelity by pruning the branches down to these critical few. But, in terms of the rest of it being Monte Carlo, remember you have one Monte Carlo run, which is EBS failure, and we plot that failure distribution versus time, as Andrew showed, and pick off those numbers. So, essentially, it's an event tree, pick up the appropriate lookup tables, and then it's a straight run from there.

MOSLEH: Thank you.

GARRICK: Howard?

ARNOLD: Arnold, Board.

That remark, "full Monte Carlo," applies just to the EBS degradation model?

SOWDER: Yes.

ARNOLD: Would you go to Slide 14? That’s the one that presents the results of that model. I had a hard time finding--okay, the dry conditions, DS and WP intact, is that that right-hand dotted curve? It can’t be. Where do I find that on--

MR. SOWDER: That would also probably fall under the right-hand curve.

ARNOLD: Well, then, everything is intact.

KESSLER: Congratulations. That’s the answer. We have very few EBS failures in a million years.

ARNOLD: Well, that goes out to 3 million years.

KESSLER: Yes.

ARNOLD: Well, what is it that happens at the 3 millionth year where you finally get up to one? Nothing?

KESSLER: We assume 100 percent of the EBS’s failed.

ARNOLD: Yeah, but dry conditions--I’m missing where is this curve?

KESSLER: We have four curves, three of which are for wet conditions, where essentially we have active dripping. Then, we have the one, the blue X’s that’s a little hard to see.

ARNOLD: Okay.

KESSLER: So, the left curve, the red triangles, is where we assume no waste package performance, and then that’s essentially drip shield failure versus time. Then, the other ones are no drip shield waste package performance. Then, we put them together, and then we show you that if we go with dry conditions versus wet, it really makes very little difference to us in terms of failure distribution.

ARNOLD: Okay. So, these are unfailed? Well, let's look at a million years. You go up there to about 30 percent.

KESSLER: That's right.

ARNOLD: What do you say about that 30 percent in a million years? What does that point mean?

KESSLER: It means that both the drip shield and the Alloy 22 have failed.

ARNOLD: Have failed. Okay. And, up here at 3 million years--

KESSLER: That means when--all drip shields and waste packages have failed.

ARNOLD: Okay, got you. Have you compared that with the DOE patch model? Do you have any feel for how that--

KESSLER: We have less failures.

ARNOLD: And, there's no TAD in this either?

KESSLER: No, this is pre-TAD.

ARNOLD: So, it will get better?

KESSLER: Well, this also doesn't have codisposal waste packages, but, again--

GARRICK: So, it will get worse.

KESSLER: Well, it’s not a simple yes or--

ARNOLD: I just wanted to understand. Thank you.

GARRICK: Bruce?

KIRSTEIN: Has any information been published from IMARC on material balance that demonstrates conservation of mass, taking into account in-growth and decay, and, thus, illustrating the numerical precision of the model?

APTED: That’s exactly what’s been put in this IMARC 9, sort of overall analysis. So, you will see the verification, you will see the comparison, benchmarking, the code results against analytic solutions, for example, you will see the in-growth and decay chain type of information as well. All that will be in the sort of super report that’s coming out.

SOWDER: Well, one of the issues we’ve had is since this model was developed over a period of two decades, and we report periodically on the revisions, each revision is not necessarily a complete report on the entire model. And, so, now we’re trying to fold it all into one, one stop shopping for comprehensive documentation.

KIRSTEIN: But, you have done this before and seen it, that it does conserve mass?

APTED: Right.

GARRICK: Any other questions? Okay, thank you very much. Now, before we hear from Ron Ballinger, this morning, Board Member Murphy asked a question of Peter Swift, and

Peter has an answer and he’s now going to give it.

SWIFT: The question was how we treated the radon dose. And, I made a phone call and I have the answer for you/

MURPHY: Thank you.

SWIFT: Indeed, as I said, we exclude and screen out the radon dose, radon migrate directly up through the rock to the soil at the mountain surface. We do consider the radon dose from essentially from the ground water that is pumped at the RMEI location and used by the RMEI. This is discussed in the biosphere model report. Merrill Wasaolic (phonetic) is the main author of it. The example I offered of the radon inhalation dose from bathwater, from shallow water, actually, she discusses that and concludes it is not significant.

However, the pathway from inhalation from irrigation water from fields and around the house is considered. It is significant, and is a contributor. It’s captured in the Radium 226 BDCF.

GARRICK: Thank you very much.

KADAK: Since Peter is up, a quick question? Did you guys take into consideration the infiltration report in your TSPA, the final TSPA model?

SWIFT: Took it into consideration? Yes. Did we change our model as a result? No.

KADAK: Okay. So, you got three times the infiltration rate than you previously assumed?

SWIFT: If Ming is here, he should answer that question. But, the upper bound, a range of infiltration, yes, is considerably higher. However, it was weighted so that the meaning was not that--Ming, are you here, still? He may have left. I apologized.

GARRICK: Okay, we're now going to hear from Ron.

Ron, will you introduce yourself and the topic?

BALLINGER: I was a member of the so-called IPAR, Independent--and I stress the word independent--Performance Assessment Review Panel, which was put together by the lead lab, to take a look at the TSPA, and answer some very specific questions. I should be very careful to say that I'm the materials person that was a part of this panel. There were a number of others, dealing with risk and ground water transport, volcanology, and the like, and their names are here. And, we produced a report, which is available now I think on the web.

And, this presentation is an expansion of that report, and if anybody has questions about things other than materials related, I'm happy to either lie to you, or take the questions and get real answers back to you later on.

I have expanded it a little bit based on some additional things that have happened since the report was presented to the lead lab in January. And, then, I have a few personal opinions, which I was asked to give at the end.

So, I’m a materials person. I’m on the faculty at NMIT and I’ve been there since about 1982. Before that, I got my doctorate there as well. So, I’ve never really had a real job. Was in the Navy before that, so that wasn’t a real job either.

Okay, the Panel was formed in March of last year. We’ve had a number of meetings before. Most of them--well, all of them, actually, in Las Vegas. We’ve also been given access, or were given access to just about anybody we wanted to talk to. A lot of us met with other team members individually. We had no restrictions whatsoever on what we were doing. In fact, while I’m an expert, I consider myself an expert in corrosion, there was not a single document related to this project that I had read before this. Familiar with the literature, of course, so I started out with a very, very clean slate. So, I had a lot of reading to do.

We formulated our conclusions in January, and then we presented them to the lead lab management at the end of January 2008.

ARNOLD: Was there another panel like this before who turned thumbs down--

BALLINGER: There have been, I believe, interminable numbers of panels, like four or five, or so panels, which have had--which have viewed the various forms of this document. We’ll address that a little bit. But, you’re correct, there have been a number.

Our primary focus was, again, we didn’t have a whole lot of time, the better part of a year, nine months, or so, so we couldn’t go very exhaustive, so we focused on the conceptual models, and tried to look at the underlying science and some of the conservatisms as I’ll comment on a bit later, and we didn’t review any of the computer codes, or detailed numerical inputs. Nobody on the panel that I know tried to run GoldSim, for example.

Next?

Okay, here are the questions we were asked, and I’ll give you the answers. Were the processes and results of the performance assessment adequate, what I mean adequate now, not perfect, but adequate to support submittal of the license?

The answer we came up with was yes.

Were there significant unresolved technical or scientific issues that we felt that the lead laboratory should address? We found one, and that was related to oxide wedging, which I will comment on more a little bit in a few minutes.

Does the current performance assessment appropriately incorporate comments from other review panels?

And, the answer is yes.

Next, please?

Okay, a little bit more detail on Question Number 1. Again, are these processes adequate to support? Yes.

We felt that the TSPA is a pretty darned impressive analysis of the problem. Several of our panel members were on some of the other review panels, and, so, they had prior knowledge of some earlier copies of the documents. And, the current version, in their mind, was much, much, much improved. And, we, in fact, thought it was, in some cases, contributed significantly to our understanding of future performance. It’s comprehensive. There’s significant improvement over the previous TSPA versions.

Again, the evaluation of uncertainties and sensitivities, and especially including the PMA was very, very good, and provided really good insights into the future behavior of the repository.

Next, please?

Okay, with the exception of oxide wedging, we found the analysis to be adequate to support a license application. I keep stressing the word “adequate.” We’ll say, a little bit later on, the question always comes up is it enough? Should we do more? But, we felt that the analysis was adequate.

That doesn’t mean that every problems was perfectly solved. We don’t have time, if it’s a million year problem, we could always use additional data. All we’re saying is that we’re satisfied that additional data won’t show or turn up fences that would prevent things from happening.

It’s highly conservative in some aspects, and I’ll talk about that on the material side, and we think that in the long run, if those conservatisms were more carefully looked at, that it would give us a lot of insight and improve performance margin. And, two examples would be treatment of stress corrosion cracking, which has come up time and time and time again through the day, and a magmatic intrusion.

Next?

Okay, what about oxide wedging? Well, in the case of oxide wedging, we end up with what I would term tiery of the conservatisms. And, that is to say the FEP that was in force made an assumption that oxide wedging won’t occur because of stress corrosion cracking of the underlying stainless steel canister, and, therefore, there won’t be any load that would be transmitted from the underlying canister to the Alloy 22.

We thought that well, that was one possible scenario, but it’s more likely that at the time that the perforation would occur, that the temperature would be low enough, such that the likelihood of stress corrosion cracking of the stainless steel, it’s not a probability of one, and that another possibility is that you will get some wedging, but that the Alloy 22 is very ductile and tough, and that would resist failure by oxide wedging. So, we felt that there was another scenario that was possible.

Also, will say that oxide wedging itself would put the inner barrier, the stainless, in compression and you’re not going to get stress corrosion cracking with a compressive stress.

Next?

In terms of we reviewed several of the past documents, in particular, the IVRT review, and in the IVRT review, they listed a very specific set of their comments in Section--they had some very specific comments, and we addressed, the TSPA addressed those specifically, and, so, we felt that overall, that the comments were adequately reviewed.

Next?

Okay, hydrology. We felt that all of the important flow and transport processes were incorporated into the model, and we’ve heard about some of those today. State-of-the-art practice has been used. All of the AMRs provide a lot of insight into the role of ground water, and there’s no excessive--we felt that there were no excessive conservatisms or optimisms in the overall model.

In terms of geochemistry, no shortcomings in the geochemical input. There was a very detailed examination.

The seepage water chemistry were our best practice. There is this issue of the chloride to nitrate ratio. We felt that that’s a fairly good index for the potential for Alloy 22 corrosion, although, if you looked--I don’t have the equation to put up here, but that equation was a multi-variate equation, which had the nitrate to chloride ratio was one element, but temperature and other--there were other elements to that relationship as well. And, temperature is a pretty dominant one as well.

Now, there’s some recent data that Ron Latanision alluded to, I believe, and there were some discussions here about the nitrate to chloride ratio changing with time, and moving in a direction which would suggest that you might get more severe corrosion. The data right now from this morning indicates that that’s probably not--the nitrate to chloride ratio might be changing over time, but that’s not affecting the corrosion rate significantly. And, we have to remember, though, that the dose is already very, very low. So, the fact that the nitrate to chloride ratio might change such that you get increased probability of corrosion, it needs to be compared with the end point result. That is to say, what is the affect on the dose? So, if we’re off on some of the models, what’s the ultimate affect on the dose? That’s the important thing.

UO2 corrosion and migration. The model is simple.

The natural analogs are excellent for Yucca Mountain. And, there was a little bit of an issue related to colloids, which I’m not an expert on, and would have to defer, but it’s an improvement. But, we thought that it lacked a little bit of clarity.

Next?

Disruptive events. We felt that there were a lot of conservatisms in the seismic and igneous scenarios that could be relaxed. Remember, the seismic scenarios actually turn out to be the dominant events that control long-term dose, the seismic and igneous events. And, the seismic predictions include conservatisms for ease of computation, but the igneous intrusive scenario has excessive conservatisms, we believe, that are not addressed in the PMA.

For example, the model assumes that all the emplacement drifts will complete fill with magma. I mean, I think the EPRI model suggests that that’s not a realistic, a very conservative scenario, and I think that DOE would suggest that that’s probably true, as well.

And, also, I would comment that the assumption that the waste packages are compromised because of the presence of the magma is probably a conservative assumption as well, because the stainless steel won’t melt until 15 or--14 or 1500 degrees Centigrade. The magma temperature is 1100 degrees Centigrade. So, it’s unlikely that they will be melting of the stainless steel.

Also, with respect to rock falls, we felt that there was conservatism related to rock falls as they affect magma, and how much magma would contact the waste package.

Next?

The biosphere. State-of-the-art practice. Key assumptions, many of the key assumptions are prescribed by the regulator, and we found no problems with the analysis described.

Next?

The uncertainty and sensitivity analysis. It’s a rigorous, George Apostolakis was the member of the team that talked about uncertainty and sensitivity analysis, looked at that, very, very state-of-the-art practice, just a very good methodology that was used for the overall process.

Next?

Okay, now we come to, of course, the most important issues. Materials are always the most important issues. All the others sort of support materials. Okay, we felt, and I think it’s true that the corrosion phenomena are critical elements to the calculation of the dose. We found no relevant corrosion phenomena that would be left out of the analysis. The general corrosion model is complete, but conservative. And, now, there’s some notes here. These notes are my notes that are in addition to the report--the presentation we gave to the lead laboratory.

There have been some recent issues related to the five year data, and I think Professor Duquette alluded to some of this, the measurement issues. But, the problem is, we don't think it's a show stopper, we have chosen a material which has a very, very slow corrosion rate, and we're measuring weight gain, which is at the limit of sensitivity of state-of-the art instruments. So, whenever you are doing that, even for five year date, or ten year data, you're going to get--you're very close to the limit of your sensitivity, and, so, it's going to be difficult to make a judgment that there's any corrosion rate at all. And, so, the corrosion rate is very, very low.

Localized corrosion, and in particular, stress corrosion cracking, has been talked about here and alluded to pretty much all day. It's been treated in a very highly conservative way. To my knowledge--to my knowledge, there's been no stress corrosion cracking in Alloy 22. Now, we can go to the literature, and we can go to even the reports, and you will see that there's a stress corrosion crack growth rate which is called out. Well, in fact, that's not really a stress corrosion crack growth rate. That is a coaxed stress corrosion crack growth rate. It's never happened, but the assumption is made that it does happen.

So, it's entirely possible, although not with a certainty, that you will just not get stress corrosion cracking in Alloy 22. And, if that happens, that takes a lot of the scenarios, that modifies the intrusion, especially the seismic scenarios, that modifies those very significantly, and takes a lot of that dose off the table.

Also, the Grade 7 titanium is very resistant to stress corrosion cracking. In fact, that stress corrosion crack data that’s been published is also arbitrary in the sense that we just assume that it occurs, and that material, it’s hard to have a stress that’s high enough to even get initiation to occur. And, the other thing we have to remember is that the process is one of initiation, plus propagation, and initiation is not automatic. In fact, it’s often, and most often, the most difficult thing to happen, and yet we assume that it does happen. And, we assume that it happens at stress levels which are much lower than stress levels which have been applied in U-bends or other kinds of things, where you have not gotten initiation. So, it’s very, very, very conservative.

Pitting and crevice corrosion and MIC are included, but are very unlikely also to occur in C-22. Again, realistic repository conditions make pitting unlikely. We can contrive a set of environments, and we can give a set of environments where we can get pitting, but in realistic environments, we don’t think that that’s likely.

Again, back to the chloride to nitrate ratio, that data was not--we didn’t have that data at the time. But, again, we don’t think that this issue is going to affect the actual dose to the RMEI.

A lot of other forms of localized corrosion, hydrogen effects, and--I’ve got another slide on deliquescence. I’m not overlooking deliquescence. I’m not trying to slide that through. All right. There will be another slide.

Next?

Okay, the general corrosion models, again, they’re conservative at lower temperatures, but in fact they’re very conservative at higher temperatures. There’s assumptions made on humidity. There’s assumptions made on whether corrosion will--in some cases, corrosion occurs at temperatures where there’s no water, and, so, the assumption is that it’s just there. And, so, that’s conservative.

The SCC models are adequate, again, but they are overly conservative. They could be made more realistic, while still being conservative.

The inner stainless steel canisters, that’s a stainless steel canister, it’s over an inch--it’s about an inch thick in one case, and more than that in another case, and the assumption is often made that it just disappears. It’s not part of the system. When, in fact, at the temperatures--at the time when general corrosion would perforate, or possibly perforate the Alloy 22, the temperature is low enough so that degradation of the stainless steel, at least by stress corrosion cracking, is not going to happen.

And, so, chloride cracking and the like is a very unlikely event at the low temperatures after perforation would occur. Now, that’s not to say that pitting or some other form of corrosion wouldn’t occur, but it’s unlikely that that would be not a barrier at all.

Next?

All right, here’s the deliquescence slide. Okay, the issue of deliquescence has evolved within the project, and there is actually some new data in 2007, so the project has not been stagnant on that. It’s likely to occur for some conditions. In some cases, both the Alloy 22 and titanium would be susceptible to localized corrosion due to deliquescence, but these conditions are very unlikely. They are theoretically possible, but they are unlikely to occur in the repository.

So, while deliquescence is possible, it’s got a very low probability. That’s how the project has treated it, and for the waste package, it’s the same thing. It’s possible, but its consequences are believed to be insignificant, which is not to say that it doesn’t occur. It just says that the dose to the RMEI is not significantly affected if deliquescence related corrosion occurs. So, it’s screened out as low probability or insignificant, it’s not screened out as being impossible.

KADAK: Why do you say it’s unlikely, these conditions are unlikely?

BALLINGER: Because you could get deliquesced salts, but the corrosion due to those deliquesced salts is likely to be of little consequence. That’s what we’re saying.

KADAK: But the conditions are unlikely is different than what you just said.

BALLINGER: Well, there’s only certain times during the exposure period when it’s possible at all. Okay? And, so that may be a little bit--I probably should change the words. Deliquescence is possible. Okay? But, the consequences of deliquescence are very low.

GARRICK: Ron, you made the comment that DOE treated the mechanism as low probability. I don’t think that’s correct.

BALLINGER: Oh, I think that is--

GARRICK: Well, I think it’s correct that they just screened it out. They did not probabilistically treat it.

BALLINGER: Oh, okay, it’s screened in two cases. It’s screened out as either low probability, or low consequence. In the case of the drip shield, I believe it’s low probability. In the case of the waste package, I believe it’s screened as low consequence. Which in neither case does it mean it doesn’t happen.

GARRICK: Right. I just wanted to point out that fuzzy difference.

BALLINGER: Okay. George Apostolakis would be very happy.

So, in our opinion is the TSPA deliquescence ready? The answer is yes. Should we continue to expand the database in this area? Probably yes. But, that’s different than saying we shouldn’t submit the TSPA before we get the data.

Next?

Okay, what about Oxide wedging. Well, it’s the tree in the sidewalk problem. Again, the corrosion products have a larger volume than the material consumed. And, by the way, there is not a whole lot of corrosion products here. So, we conjure up this idea that we’ve got this voluminous pile of oxide that’s sort of forcing the canisters open. Both the stainless steel and the titanium or the C-22, the films are pretty thin. Pretty thin. But, we say that we ultimately--the forces results in failure of one or more of the barriers, either the inside or the outside. And, in our case, we think that we would say that the outer barrier is at risk.

Well, the FEP assumed that SCC will occur on the stainless steel, and that as a result, there’s no inner restraint. And, if there is no inner restraint, there's no possibility of having wedging.

So, what we're saying is that well, the corrosion rates are going to be very low. About a million years is a long time. The penetration of the system would be local, and general corrosion would remove one of the constraints. So, at 300,000 years, if you get perforation due to general corrosion of the C-22, then you can't get wedging on the inside, because there's no place to react the force to. Right?

DUQUETTE: No. We'll talk.

BALLINGER: Okay. I suspected we would.

The inner stainless steel barrier. It may not fail by stress corrosion cracking. And, if it did, there would have to be tensile stresses. And, the outer barrier is more resistant than the inner barrier. The inner layer may degrade by general corrosion before the outer barrier. No wedging stresses. Okay? And, there would be limited access of the corrosive agent, oxygen, and lower temperature. So, all of these mitigate or complicate the discussion of oxide wedging.

Next?

So, the more likely scenario, if it occurs at all, would be the Alloy 22 perforates by some means. If the temperature is high enough, you might get stress corrosion cracking of the inner liner. And, then, you’re likely to get, if you do get oxide wedging, you get compressive stresses on the inner liner. If the temperature is low, you are unlikely to get stress corrosion cracking of the inner liner.

Then, you have general corrosion of the inner liner. You get oxide buildup. You would get tensile forces applied to the outer barrier. But, we felt that the deformation would be accommodated by the ductility of the Alloy 22. One of the characteristics of Alloy 22 is it’s very, very tough. It’s a very, very tough material.

Next?

Okay, so that’s the state we had with the IPAR. The last two slides are my own opinions. We’ve chosen wisely, in the words of Indiana Jones, a set of material combinations that are very, very resistant to corrosion, to environmental degradation in general. So much so that we’re having trouble mentoring the race, and that’s giving us fits in some cases. We then process the EBS, the barriers, to minimize the effect of stresses. We have low plasticity burnishing on the welds. It’s a belt and suspenders approach.

We then set out to find a way to cause degradation, and when you tell somebody like me to go and find a way to make something degrade, well, I’ll go and find a way to make it degrade. And, that’s exactly what’s happened. But, we’ve had to do that with very extreme, in many cases, conditions, especially relative to, not more--actual environments, and we use extreme stresses. But, however, the fact that degradation can be caused doesn’t mean it will happen, and we have to remember that.

But, in the non-scientific world, happening usually gets translated into always. And, so, that’s something that needs to be remembered.

We then assume that it does happen, but we’ve had to contrive a set of variables to make it happen. But, this is conservative. And, with a million year problem, this approach just doesn’t work. It won’t work. Okay?

Next?

Can we guarantee that degradation will not occur? No. It’s a million year problem. Can we reduce the likelihood that degradation will occur? Yes. We keep the stresses low, and the project has done this. We minimize the effect of residual stresses. Material selection, we’ve got the best material, in my opinion, that’s available. We use thermodynamics. These materials, at low temperatures, most likely--and I keep saying most likely, I don’t say perfectly, they have stable protective films that are thermodynamically stable. So, if you don’t provide a means by which you can fracture them, they will remain stable. As long as they remain stable, then the only degradation problem that we have is basically the bare surface dissolution rate, or the passive current density. And, this system is at low temperature for most of the repository life. And, we minimize the overall aggressiveness of the environment. That is to say we use a drip shield, and we ensure that we have compressive stresses near the welds.

Again, has the project done enough to ensure the safety of the public? I believe the answer is yes. Will additional work be done going forward? I think almost certainly yes. But, is this work necessary prior to submittal? The answer, no, in my opinion.

Thank you.

GARRICK: Thank you. All right, let the fun begin.

BALLINGER: I’ve got my flak vest on.

DUQUETTE: Unless you’re wearing it over your head, you’re in trouble.

BALLINGER: I guess I don’t know how to respond to that.

GARRICK: Let me start with one question, and then I’ll come back to my other questions if there’s time.

You said in a previous slide that there wasn’t going to be much in the way of corrosion products.

BALLINGER: Corrosion occurs very slowly, so in a million years, there will be corrosion products, but we have to remember that these films are very, very stable, so we don’t want to think that we have a picture of--we don’t want to have a picture of a tree in a sidewalk effect.

GARRICK: But, I guess what I’m trying to understand here is what is the soup at the end of all of this with respect to the mobilization of the waste. When you say that the corrosion products inventory is small, although it increases with time, obviously, are we saying that we have containment?

BALLINGER: I can’t comment on what the soup looks like for the fuel. But, the soup, if you want to call it that, from the canisters is basically going to be oxide. I mean, the stainless steel or the titanium will convert, or the C-22 and it’s elements, will convert to an oxide, and those oxides, by the way, they’ve been around for as long as--they’ve been around for millions of years.

GARRICK: Well, I noticed that--I was very impressed with the report, and very impressed with two aspects of the report in particular, the ground water analysis and discussion, and the corrosion analysis and discussion.

But, the one thing that I was looking for that I did not see was my chronic question on this Board, and that is a clear indication of the mobilization of the waste, the mobilization process itself, and what we really end up with is a source term. That was not treated to the same level of review that the other issues were. Is there a reason for that?

BALLINGER: I can’t answer that. Peter might be able to--I mean, we had very specific questions posed to us, and we focused pretty narrowly on those questions. The only reason we expanded would be as it impacted those questions. So, it may have been a question of the problem posed.

GARRICK: Yeah. Well, just in my mind, there was a real absence in terms of the waste mobilization of what I would call radiochemistry and physical chemistry resolution of what the source term really looks like, or what it really is in terms of chemical and physical form. So, that aspect of the continuing question of the Board was not particularly addressed in any extensive way.

The only other comment I have is that you indicated that you did not consider the input data.

BALLINGER: Correct.

GARRICK: And, yet one of the other issues that the Board has always had is to try to get some resolution between the quality--some resolution of the quality of the evidence, the supporting evidence, which is the input data, between whether it was based on field work, that is to say, the site characterization program, laboratory work, literature, expert judgment, or whatever, and, so, that kind of continues to be a lingering issue as to when it comes to establishing confidence in the results, is the quality of the supporting evidence, and getting resolution between whether it’s field measurement based, or from some other source.

Did the committee look at that, or consider that at all?

BALLINGER: No, we didn’t.

GARRICK: Okay.

BALLINGER: I mean, I could talk for an hour about the difference between the field and the lab on the corrosion and metallurgical side, and that’s a very significant issue.

GARRICK: Yes. Okay. So, that is a continuing issue.

Okay, Ron?

LATANISION: Latanision, Board.

Let’s just turn to Slide 16. I’ve got a couple of comments and questions. Let’s start with deliquescence. I use the general comment I would make, and this is the second to the last bullet, is the TSPA deliquescence ready? And, the IPAR assessment is that it is ready. But, you do make the observation, or the point, that you did not consider the issue of the nitrate/chloride ratio. And, you know, I think that has evolved as something that has been of concern to the Board for at least since we’ve been alert to the issue, of the importance of the nitrate/chloride ratio in terms of the studies of deliquescence into the corrosion.

And, I think all the evidence that we’re seeing now suggests that it remains a considerable issue. When we first started considering this, you--the report, which I would agree with John, I think it’s a very good report on the part of your team. But, when I look at, on Page 32 of the report, you reference the, I guess it’s maybe not a fault tree, but the five topics, the five items, can deliquescence brines form at elevated temperatures? If they form, do they persist? Are they corrosive? If they’re corrosive, will they initiate localized corrosion. Once initiated, will it propagate to penetrate the waste package?

You know, when we first heard that list, the answer to all those questions was no. And, as time has evolved, and we had, at one stage, a workshop on localized corrosion in which all the organizations that have interest were represented, and I think we left--I think the current consensus is that the answer to the first four of those questions is either yes, or maybe, and the only one in which I believe there is still some uncertainty is the very last one, and that has to do with whether localized corrosion would propagate, or penetrate.

And, that is materially affected by the issue of the chloride/nitrate ratio. And, my comment there is that the Board identified two experimental tests that we thought the project could do that would put that issue to rest. And, you know, we’re fully prepared to take the position that if that answer turns out to be no, then the issue is done. And, I know that Neil will say they heard that and they’re responding to it. And, I know they are responding to it. But, I take that to mean that there’s still an open issue in terms of deliquescence, at least from the point of view of the chloride/nitrate ratio.

And, therefore, I’m not--maybe it can be submitted, but I think, and maybe in that context, it is ready, but I would guess there has to be some amendment somewhere along the line to really address that question.

BALLINGER: I’m not sure we’re very far apart at all actually. Remember, if the calculated dose was within a factor of two of the limit, the answer to this question might be different. But, we’re very, very--the doses are very, very low, and more likely to get lower because of the other conservatisms, not just in the materials area, but in the ground water transport, all of those models are conservative. So, the dose is likely to be--the actual dose is likely to be less.

Okay, we say deliquescence can occur, and by the way, the answer to those questions, I think the project has evolved to the point where the fact that it screened as low probability, or low consequence does not mean it doesn’t occur. Okay? And, so, I think the project has evolved to the point where the answers to those questions--we’re on the same page with you. The question that comes is even with the nitrate to chloride ratio issue, is that likely to result in a change in the outcome which would materially affect the dose in such a negative way that it becomes a fence? And, in my opinion, the answer is no. So, that's different than saying should we do more work to put deliquescence to bed, if you want to do it that way, that's fine.

LATANISION: My comment is simply that I don't know that we have the information to make that determination, because it's already been FEPed out, and, therefore, it is not being considered as a contribution to the dose. The seepage-induced localized corrosion is, but not that deliquescent corrosion. And, so, I don't disagree with your point, I think that what you guys have said, I am comfortable with, but I'm just not sure that--and I've said this before--I'm not sure there is compelling evidence that we can FEP out deliquescence induced localized corrosion.

That is the material issue. At the end of the day, what we're really concerned about is the release of radionuclides.

BALLINGER: The dose.

LATANISION: That's right. And, if, in fact, we can demonstrate that it's of no consequence, then I think the issue is solved. But, I just don't think we've gotten to that point yet, and at least from my perspective, I would have to say that I don't see a compelling or convincing argument, and I know the project is working on it, and maybe that argument will come forward. But, at this stage in history, I still do not feel convinced.

So, you know, we’re going to disagree on that one, I suppose, at least from the point of view of whether or not it’s ready. I don’t know. That’s an NRC question, I guess, or maybe a DOE question.

GARRICK: All right.

BALLINGER: He’s probably asked most of Dave’s questions.

GARRICK: David Duquette?

DUQUETTE: Duquette, Board.

I agree with John and I agree with Ron, and I agree with you that the dose is the important thing in this. But, I think you’ve--your report is very nice, but I think it over-simplifies a number of things.

First of all, let’s go back to deliquescence, since we’re on it. I don’t ever for a minute think that deliquescent corrosion is going to penetrate the container. But, I do think it will set up conditions where some localized corrosion will be initiated. And, that’s a long step for these alloys. And, so, now you go to seepage, and you don’t have the delay anymore. You’ve got localized corrosion initiated, and now you have to worry about it propagating, not re-initiating, which could take a long time under seepage conditions. So, I still believe this is something that has to be pursued, and not just brushed off as, you know, if you can prove the consequences are really low and you can't tie it to the seepage corrosion, then that's a different issue.

But, there are two different processes. In the decision tree that Ron quoted, I think almost everyone agrees that through Number 4, which is will it initiate, everyone says yes. The last question is will it propagate, and the real question is will it propagate to failure, and the answer is probably no.

BALLINGER: And, remember, that when seepage does occur, you're past the thermal pulse. You're at low temperature.

DUQUETTE: Not very low temperatures.

BALLINGER: Well, but it's low and decreasing.

DUQUETTE: You can get crevice corrosion. That's number one. Number 2, let's take the oxide wedging situation. And, I don't want to pursue it very much, because I don't think it's a very important phenomenon for failure. But, I heard this morning, and I heard you almost say that general corrosion is really general corrosion. You and I both know that's not correct. It's not likely that the container is simply going to recess down to zero. It's going to have general corrosion, and it's going to be more patchy than not.

BALLINGER: Well, just due to the temperature differences.

DUQUETTE: And, so, temperature differences, the way the water sits on it. There are a whole bunch of things. And, so, once you get perforation anywhere with a nice big shallow pit, if you want to call it a pit, then you’re going to expose the stainless steel. And, if you do that, and if the chloride ratio is in the wrong aspect, you’re going to get oxide wedging, because oxide wedging between stainless steel has been observed for 50 or 60 years.

You also might remember a little phenomenon that you’re quite familiar with called denting. And, if I have a local perforation, and I have a local wedging situation, the whole container does not go into compression. Part of it goes into compression, and part of it goes into tension. And, when it does, you can get stress corrosion cracking even at fairly low temperatures.

BALLINGER: Denting was between carbon steel and inconel.

DUQUETTE: Denting was a wedging problem.

BALLINGER: It was a wedging problem. It was a tree in the sidewalk problem, inverse tree in the sidewalk problem. But, it was between carbon steel and inconel. That’s a very different combination of materials. And, they operated the steam generators like toilets. They had chlorides in between there. Well, back in those days, that’s exactly what happened. So, you had a couple of thousand ppm chlorides between the carbon steel and the inconel, and in that case, the carbon steel corroded like gangbusters, and then you really did get wedging. But, now you have two materials which have very stable passive films, which you only can perforate that passive film into stainless steel because of pitting, or something like that, which I’m not saying doesn’t happen.

DUQUETTE: If there is chloride there, you will have wedging. It’s as simple as that. And, because it was carbon steel to inconel doesn’t really matter. The thing that caused the wedging was the oxide, not the fact that--

BALLINGER: But, the oxide has to be produced.

DUQUETTE: And, we do produce it. We can produce it in the laboratory all the time between two stainless steel materials. But, it’s probably not that important. I also would be very careful about saying that the films are thermodynamically stable. They are actually kinetically stable. Thermodynamics is not there at those potentials.

BALLINGER: Well, wait a minute. The oxide is thermodynamically stable. The metal is not thermodynamically stable.

DUQUETTE: No, it’s not. We can argue that later.

GARRICK: Mark, and then Andy.

ABKOWITZ: I have some good new for everyone. I am not a corrosion engineer.

BALLINGER: I have some good news. Neither is he.

ABKOWITZ: I did sleep at a Holiday Inn Express last night, though.

BALLINGER: We all slept at the Holiday Inn Express last night.

ABKOWITZ: I’m going to change the direction of the discussion here a little bit. I want to go back to Slide Number 3. You know, this is a tremendous undertaking that the six of you have performed, and I would like to get a little bit more into the process of how you did this. Did you take a leave of absence, or did anyone else take a leave of absence from their job to do this work?

BALLINGER: No.

ABKOWITZ: So, you were working with a limited amount of time?

BALLINGER: Yes.

ABKOWITZ: And, therefore, you were, I presume, highly dependent on the information provided to you by the lead laboratory?

BALLINGER: Yes.

ABKOWITZ: Okay. So, you pretty much, can we assume that your starting point was the information that was provided to you to review?

BALLINGER: That was the starting point, yes.

ABKOWITZ: And, then, at that point in time, you had free reign to go off and do whatever you wanted, but you had limited time to do that?

BALLINGER: We had enough time.

LATANISION: May I just interject? He’s not a corrosion engineer. He’s a lawyer.

BALLINGER: I can tell--

ABKOWITZ: I’m a little rusty at the law, though.

BALLINGER: I’m a lot of times leery of doing this kind of thing, just exactly for the reasons which you are here alluding to, at least. But, I can guarantee you that when somebody called Peter for something, we got it instantly. And, the rest of the people on the panel had prior experience, and, so, they pretty much knew where the bodies were buried, if you want to put it that way. So, we had a lot of information, and they knew--I don’t think we had a set which was not complete enough to make judgments, is what I’m saying.

ABKOWITZ: Okay, that’s fine. Let me just proceed a little further with this. So, your charge then was to basically look at the general approach; correct?

BALLINGER: Yes.

ABKOWITZ: And, so, the question was does the process make sense? Are they making reasonable assumptions about the way that certain things behave, or can occur, or what have you? And, you make the statement here that you did not review computer codes or detailed numerical inputs.

BALLINGER: Correct.

ABKOWITZ: So, your focus was really predominantly on is the approach reasonable, not whether it was carried out as intended?

BALLINGER: Yes.

ABKOWITZ: Okay. And, so, did you have a chance to look at any of the results and be able to go back and see whether they made sense with the approaches developed?

BALLINGER: We had multiple meetings, and looked at lots of--a lot of results, yes.

ABKOWITZ: Okay. So, you did have at least some way of looking at the conclusions and tracking back and saying intuitively, the conclusions seem to match the conceptual approach?

BALLINGER: Yes, thousands of horsetail curves.

ABKOWITZ: Okay. But, you were able to do that without ever looking at the codes or the inputs?

BALLINGER: That’s true.

ABKOWITZ: So, then, you’re implicitly assuming that the codes and the inputs are correct, even though you didn’t view them?

BALLINGER: Well, the codes, we didn’t look at. If you’re meaning inputs, we looked at the--we knew what the inputs were to the models, at least I did, on the material side.

ABKOWITZ: Well, there are models, and then there are 3,000 variables that you need to specify values for and distributions for. I take it that's not part of what you did. You looked at the model structure and the variable definitions and said okay, this looks like a sensible way to go?

BALLINGER: On the material side, I looked at pretty much everything. But, you're right, there's no way that we had time to be able to do the kind of analysis that the IVRT, which was the previous one, which spent a lot longer.

ABKOWITZ: So, if you had more time and resources, how would you do it differently than the way you did it?

BALLINGER: How would we do it differently?

ABKOWITZ: Well, are you satisfied that you had enough time and resources, and there is no need for more rigor?

BALLINGER: I can only--now, I can only comment on the material side, because I don't--I'm not an expert in the other areas. I think that there are--we could probably spend more time looking and exercising the models, looking at the details. That would provide a lot more--more insight. Would it change the results? I would say no. I think we all got to the point where we had seen enough, if you will, to make a judgment. I don't think anybody on the team would have made a judgment had they not seen enough.

So, I think it’s always good to see more, but I think we got past the threshold where we had seen enough. I mean, we can joke about the--it’s not a joke--the deliquescence issue, and the like, and we can disagree a little bit on sort of tactics, and whether or not it’s a serious issue or not, but we had gotten to the point, or at least I had gotten to the point, and with respect to deliquescence, a good example, I asked the project to put together every single document, every single--and, they gave me a pack, just on deliquescence alone, because I knew that that was a serious issue, and if I was going to make a judgment, I needed to have every piece of information, and I got it. So, I think it was adequate.

ABKOWITZ: Did that include the Board’s previous reports on corrosion?

BALLINGER: I have every one of them.

ABKOWITZ: Okay, thank you.

GARRICK: Andy?

KADAK: Yes, I’d like to get back to the deliquescence question.

GARRICK: You’re not entertained enough yet.

KADAK: No, I want to be more entertained, actually.

I think if I can understand what the Board’s frustration with this question, it’s not about whether deliquescence occurs or not. It's the fact that somebody decided to FEP it out. And, if we got over that thing, that barrier, we might have a reasonable conversation.

Now, in terms of the report of this IPAR, what they conclude on the outer barrier, the waste package outer barrier, was that they don't believe that if deliquescence brines are potentially corrosive, they will initiate local corrosion, and they don't believe that once initiated, will localized corrosion penetrate the waste package outer barrier. But, they do believe that it can occur. Okay?

So, the conclusion that they draw from all this is that in the unlikely event that deliquescence causes corrosion of the waste package, such corrosion would be limited and would progress slowly. Now, do you agree with that?

LATANISION: Do I agree with that?

KADAK: Yes.

LATANISION: I don't think there's any evidence at what rate it would proceed. I mean, would it propagate slowly? I don't know. I mean, localized corrosion is sometimes described as being autocatalytic in the sense that the environment trails local geometry, becomes more aggressive with time. Now, in this case, the response from the point of view of the project has been that localized corrosion would stifle. And, that may be true. I mean, if it dries out, there’s no electrolyte, it’s done, at least in the rise transient in terms of the thermal pulse.

But, I think Dave Duquette made the appropriate observation, and that is during the decline transient, when seepage begins to occur, that initial local corrosion geometry is not going to disappear. It’s not going to heal. It will begin to propagate. And, I don’t think you can disconnect those two. My comment has always been that there’s an asymmetry in the project’s attitude about FEPing out localized corrosion during deliquescence, but keeping it in during seepage. I think that doesn’t add up from my perspective.

KADAK: Okay, let me ask Dr. Ballinger. Since Dr. Latanision--

BALLINGER: He was my thesis advisor. You know that.

KADAK: Does not have any compelling evidence to support the statement that says such corrosion would be limited and would progress slowly. Notice stifling was not used. Why do you believe that it would be limited and progress slowly?

BALLINGER: Okay, there are other complicating factors here also. We keep thinking that--what do we say--will deliquescence occur? Would you have deliquesced salts? Answer, yes. All right. Would that result in corrosion? Maybe. Maybe not. There are other variables involved here, too. There’s oxygen in the atmosphere. There’s CO2.

There’s carbonates. There’s other kinds of things in that seepage water that have to be sort of factored in on what’s going on.

If you were to get--now, we’re starting to talk about minutia--if you were to get a little--when people think about localized corrosion, when I think about it, it’s very localized. So, you think about a pit with an aspect ratio that’s pretty high, that is to say, it’s very deep and not very wide, and, so, you can get isolation, if you will, and this autocatalytic business, you can get that. I’m not sure that that’s what would happen here.

Remember, you’ve got seepage going on. You’ve got water that’s covering the hole, now we’re saying the humidity is above, whatever it is, 80 percent, you have a film on the surface. And, so, there’s some washing that goes on as well during the seepage part. So, can you get the kind of pit that would become autocatalytic, like would happen potentially with stainless steel in chlorides, and the like, where you get a pit? I think that that’s not the case. I think that that’s not what would happen.

So, I don’t think you get the kind of extremely localized effects. And, so, again, can I say it would never happen? No, I can’t say it will never happen. Will it likely propagate at rates which are fast enough and maintainable? We just--I don’t think that’s happened.

LATANISION: Mr. Chairman, if I can interject? Latanision, Board.

I made the comment earlier today that unlike igneous event, which I think you do have to treat probabilistically, I mean, I don’t know how else you can do that, what we’re talking about here can be treated experimentally. And, you know, I know the budget issues, I understand the staffing issues. I know that. But, the fact is there has to be, in my mind, some priority attached to an issue that isn’t resolved. And, it can be resolved. It can be resolved very definitively, I think, with a relatively simple set of experiments that we have suggested over probably two years ago.

And, all I’m asking, from my perspective, and just my perspective, it can be ignored if people wish, that’s not the issue, the point is that from the perspective of at least the two corrosion engineers on this Board, there are ways of addressing the issue of deliquescence induced localized corrosion that could put it to rest definitively, and they haven’t been pursued. If they’re being pursued, they’re being pursued very slowly, perhaps for budget reasons, and staffing reasons. But, it’s unlike other issues that needed to be treated probabilistically, this is something that can be treated directly, and I, for one, would like to see that done.

GARRICK: All right, I think what we have here is a very healthy disagreement between two outstanding experts, and that’s not a bad thing. The issue is, one of the issues at least, is whether or not it really matters in terms of the dose. There seems to be a substantial margin here that would suggest that maybe it doesn’t matter that much, but still, there is the issue. So, I think that, I don’t know, there’s not much more that can be said about that.

BROWN: Pardon me.

GARRICK: Yes.

BROWN: This is Neil Brown with Los Alamos.

I would like to get it on the record that the experiments you’re talking about, we’re in the process of doing. So, you know, respectfully, yes, we’ve been slow. Respectfully, we’ve had lots of problems, budget, plus technical. But, the work is going on as we speak.

LATANISION: And, Mr. Chairman, I do acknowledge that. I didn’t suggest that it wasn’t being done. I know, we visited in Sandia, we know that there’s a capability in that autoclave system, or chamber that they’ve developed, to do what we would like to see done. And, I know that that’s on the agenda, but, you know, I’m addressing the questions that have come up in this conversation, and, you know, I think with a little bit of effort on that experiment, these questions can be addressed definitively. That’s all I’m saying. And, I’d like to encourage that, that the project put the effort into it to make it happen.

GARRICK: And, they are on the record now for saying that it is happening. Is that correct?

BROWN: Not deliquescence localized corrosion, but testing, looking for it.

GARRICK: Well, that’s a little different. That’s a little different.

BALLINGER: Now he needs the flak jacket.

GARRICK: Any other questions from--okay, David Diodato?

DIODATO: Yes, thank you very much for coming, and we appreciate that. I read your report and found it interesting. I’m a hydrogeologist, and I was mostly interest in the hydrology section. I just want to pick up on one thing that was stated there and gained a reaction to it.

There’s a statement, I’ll just read part of it, this is in the general overview, “--adopted to simplify the analysis of water movement in the unsaturated and saturated zones--ellipsis--are considered reasonable when evaluated in the context of the intended purpose of the TSPA-LA, and the anticipated performance of the engineered barrier systems.”

So, I read through the rest of this section looking for which parts of the engineered barrier systems were critical to justify the assumptions in the natural systems, the hydrology, and couldn’t find it. So, I’m wondering if you could tell me which elements in the engineered system were necessary for these assumptions to be justified? Could it be drip shield, waste package?

BALLINGER: Now I have to apologize, because I'm not a hydrologist. But, I think if we can codify that question--

GARRICK: You don't have to apologize for that.

BALLINGER: Maybe I should apologize for being a materials person. But, if you can codify that question, I can get it to the right person, and I think we can get that answer. I mean, I can't give you a satisfactory answer.\

DIODATO: Well, the second part to it then would be in the analysis of the engineered system, were there natural system features or elements that were used to justify assumptions in the engineered system that you're aware of?

BALLINGER: Natural analogs for--

DIODATO: No, the mountain, were there features of the mountain, for example, that were used to justify the assumptions in the engineered system that you're aware of? In your evaluation. You know what I'm getting at here?

BALLINGER: Now, I think the answer is yes, but again, if we can--I need to be careful to make sure I can get you the answer that you want, from the right person.

DIODATO: I appreciate that. Thanks.

GARRICK: Any other questions? An excellent discussion. Thanks very much, Ron. And, we appreciate your coming out here on such short notice.

BALLINGER: I had to do something to keep you awake.

GARRICK: I hope it did that.

Okay, we’re now to the part of our program that is for public comments. Because of schedules, we already heard from two members of the public. We have a third one, Irene Navis from Clark County, that has asked to be heard. And, we’re delighted about that, and would like to hear from you.

NAVIS: Thank you very much, Mr. Chairman. Good afternoon.

I want to welcome you, as always, to Clark County, Nevada. And, we appreciate the valuable role and significant insights that you’ve provided all these years to us, to not only the affected units, but also the DOE and the other stakeholders. I think you’re doing very, very important work, and we’re glad to hear that you plan to make that an ongoing effort.

We are fully supportive of any ongoing work that the Board undertakes, because we think it is so important to gaining a better understanding of the whole system related to the potential Yucca Mountain repository, especially as we’re entering into this new phase with the License Application being submitted as early as next week. We are really looking forward to what else that the Board can undertake as some either peripheral or directly related issues, such as the long-term TAD performance, issues related to thermal load throughput, safety issues related to that.

The second repository report is coming out in the next few months. We think that there are some issues that this Board can undertake in terms of considering what the potential impacts are related to Yucca Mountain, either if there is a second repository, or if some other decision is made.

We think that the discussions related to interim storage, reprocessing GNEP, all of those not related directly, but also could have an impact on Yucca Mountain, I think could be within the purview of this Board to discuss at some level.

We also think that the most important thing that you could undertake is taking a look at the long-term performance issues related to the repository, especially in terms of long-term management and operations.

So, that concludes my comments, and I just want to say thank you for contributing, as always, to our local economy, and have a good time.

Thank you.

GARRICK: Thank you. Thank you very much, Irene.

Are there any other questions or comments from either the Board, the Staff, or from anybody in the audience?

If not, I want to thank the presenters. I thought it was a very outstanding day of presentations. And, these are very difficult and busy times for everybody, and the Board greatly appreciates the quality of the presentations, as well as the presenters. And, we want to especially recognize that, and we look forward to the upcoming weeks and months. We’re at a pretty pivotal point with respect to this project, but we also think it’s a time for a great need of continued inquiry, and we’re hopeful we can do that, and continue to serve our mandate that we’ve committed ourselves to.

So, unless there are further discussions, comments or questions, this meeting is adjourned.

(Whereupon, at 5:25 p.m., the meeting was adjourned.)

CERTIFICATE

I certify that the foregoing is a correct transcript of the Nuclear Waste Technical Review Board public meeting held on May 29, 2008 in Las Vegas, Nevada taken from the electronic recording of proceedings in the above-entitled matter.

June 6, 2008

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