United States
Nuclear Waste Technical Review Board (NWTRB)

Transcript

Winter 2022 Board Meeting

Tuesday

March 1, 2022

VIRTUAL PUBLIC MEETING - DAY ONE

NWTRB BOARD MEMBERS PRESENT

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BAHR: Okay, well, hello, and welcome to the U.S. Nuclear Waste Technical Review Board’s Winter Meeting. I’m Jean Bahr, the Chair of the Board, and this meeting will focus on the U.S. Department of Energy spent fuel and waste disposition research and development activities and DOE’s consent-based siting process for identifying federal interim storage facilities. Due to the Covid-19 pandemic, we’re holding this meeting in a virtual format, and Mr. John Hattin of Precon Events will serve as the host for the meeting.

I’ll introduce the other Board members and then briefly describe the Board and outline what we do. I’ll then tell you why we’re holding this meeting and summarize the meeting’s agenda.

So, we’re going to switch now to the panel view so I can introduce the Board members, and I’ll ask that as I introduce them the Board members activate their cameras and come online and say hello so that the audience can see who they are. I’ll begin. I’m Jean Bahr, the Board Chair. All the Board members serve part time, and we all hold other positions. In my case, I’m Professor Emerita of Hydrogeology in the Department of Geoscience at the

University of Wisconsin, Madison. Our first two Board members are only able to join us today by audio. First is Steve Becker, and Steve is Professor and Chair of Community Environmental Health in the College of Health Sciences at Old Dominion University.

BECKER: Good afternoon.

BAHR: Thank you, Steve. Then we have Mr. Allen Croff. Allen is a Nuclear Engineer and an Adjunct Professor in the Department of Civil and Environmental Engineering at Vanderbilt University.

CROFF: Good afternoon.

BAHR: Thank you, Allen. Then there’s Dr. Tissa Illangasekare. Tissa is the AMAX Endowed Distinguished Chair of Civil and Environmental Engineering and the Director of the Center for Experimental Study of Subsurface Environmental Processes at Colorado School of Mines.

ILLANGASEKARE: Good afternoon.

BAHR: Next, we have Dr. Lee Peddicord. Lee is Professor of Nuclear Engineering at Texas A&M University.

PEDDICORD: Good afternoon.

BAHR: Next is Dr. Paul Turinsky. Paul is the Board’s Deputy Chair and Professor Emeritus of Nuclear Engineering at North Carolina State University.

TURINSKY: Guten tag. I’ll be different.

BAHR: Okay. I’ve just introduced five Board members plus myself, not the full complement of eleven. Other Board positions are currently vacant.

As I usually do at Board meetings, I want to make clear that the views expressed by the Board members during the meeting are their own, not necessarily Board positions. Our official positions can be found in our reports and letters which are available on the Board’s website. So, if we can switch back to the slides now. Thank you.

So, on to a description of the Board and what we do. As many of you know, the Board is an independent federal agency in the Executive Branch. It’s not part of the Department of Energy or any other federal department or agency. The Board was created in the 1987 amendments to the Nuclear Waste Policy Act to perform objective ongoing evaluations of the technical and scientific validity of DOE activities related to the management and disposal of spent nuclear fuel and high-level radioactive waste.

Board members are appointed by the President from a list of nominees submitted by the National Academy of Sciences. We are mandated by statue to report Board findings, conclusions, and recommendations to Congress and to the Secretary of Energy.

The Board provides objective technical and scientific information on a wide range of issues related to the management and disposal of spent nuclear fuel and high-level radioactive waste that will be useful to policymakers in Congress and the administration.

All of this information can be found on the Board’s website, www.nwtrb.gov, along with Board correspondence, reports, testimony, and meeting materials, including archived webcasts of recent meetings. If you’d like to know more about the Board, a 2-page document summarizing the Board’s mission and presenting a list of Board members can be found on the Board’s website.

We’ll have a public comment period at the end of each day’s meeting. Because of the virtual format of this meeting, we can only accommodate written comments. As you joined the meeting, on the right of your screen is a comment for the record section where you can submit your comments.

If you’re viewing the presentation in full screen mode, you can access the comment for the record section by pressing the escape key. A reminder on how to submit comments will be displayed during the break.

Comments that we receive during the meeting will be read by staff member, Bret Leslie, in the order that they’re received. Time for each comment may be limited depending on the number of comments we receive, but the entirety of the submitted comments will be included as part of the meeting record.

The Board welcomes comments but particularly in light of our virtual format we will not be responding to them. Comments and any other written materials may also be submitted later by mail or email to the points of contact noted in the press release for this meeting, which is posted on our website. These also would become part of the meeting record and will be posted on the Board’s website along with the transcript of the meeting and the presentations you will see today.

The meeting is being recorded, and the archived recording will be available after a few days on our website.

The meeting agenda and presentations have been posted on the Board’s website and those can be downloaded.

Okay, so, why are we holding this particular meeting? This meeting is part of the Board’s continuing review of DOE activities related to the management and disposal of spent nuclear fuel and high-level radioactive waste. Over the past several years, DOE has been conducting research and development efforts on non-site-specific disposal, storage and transportation, and integrated waste management. According to DOE, this work is part of its efforts to develop a sound technical basis for multiple geologic disposal options in the United States and provides data and analyses to support decisions regarding storage, transportation, and disposal of spent nuclear fuel and high-level radioactive waste.

In addition, DOE recently published a request for information on how to site federal facilities for the temporary consolidated storage of spent nuclear fuel using a consent-based approach. The Board views both the research and development and siting efforts as activities that enhance the DOE’s capability to address several of the recommendations we made in a report that we issued last spring, mainly to anticipate the required high performance computing and data management infrastructure required for a multidecade waste management program, to facilitate application of iterative and adaptive approaches to development of a geologic repository, and to embrace openness, transparency, and engagement. Our meeting will examine DOE’s spent fuel and waste disposition research and development activities and DOE’s consent-based siting process to identify federal interim storage facilities for spent nuclear fuel.

So, moving on to the next slide, today’s meeting will start with a presentation by Tim Gunter from the DOE, Office of Nuclear Energy, and Geoff Freeze from Sandia National Laboratories, and they’ll provide an update on DOE’s dual-purpose canister disposal research and development activities.

Then we’ll hear from the national laboratory researchers who are conducting some of the work for DOE. Laura Price will give a presentation on repository-scale performance assessment that includes nuclear criticality after repository closure. After a 15-minute break starting at 1:55 pm eastern time, we’ll hear a presentation on efforts to develop spent nuclear fuel cladding degradation models.

In the final two talks today and then the first talk tomorrow we hear about DOE’s storage and transportation research and development activities. Ned Larson from the DOE, Office of Nuclear Energy, will summarize those activities and provide a status update. Then we’ll hear from national laboratories researchers about investigations in the cannister corrosion and surface environments for spent nuclear fuel that is in interim storage.

As I mentioned earlier, we’ll have a public comment period at the end of the day during which staff member Bret Leslie will read public comments that we have received, and we’ll adjourn day 1 of the meeting at 5:00 pm eastern time.

Looking ahead to tomorrow, our meeting tomorrow will start at 12:00 pm eastern time with a presentation from a national laboratory researcher on investigations of aerosol transmission through simulated cracks in dry storage cannisters.

Then, in the following three talks we’ll hear about some of DOE’s integrated waste management research and development activities. First, we'll hear about a canister project that is supporting efforts at the Hanford Washington site and DOE's integrated waste management program. After a 15-minute break starting at 1:45 pm eastern, we'll hear about two software tools developed as part of the integrated waste management research and development efforts. A presentation from a national laboratory researcher will describe updated requirements for, and enhancements to, the Next Generation Systems Analysis Model. Then, Erica Bickford from the DOE, Office of Nuclear Energy, will update us on the DOE's Stakeholder Tool for Assessing Radioactive Transportation software.

The last presentation of the meeting by Alisa Trunzo from the DOE, Office of Nuclear Energy, will update the Board on DOE's consent-based siting efforts.

Okay, so, now we're going to switch back to a view of me, and I just want to note that much effort went into planning this meeting and arranging the presentations. I want to thank our speakers for making presentations at the meeting today.

Thanks particularly to Board members Paul Turinsky, Lee Peddicord, and Steve Becker, the co-leads for the Board for this meeting, and to the Board staff, particularly Bret Leslie and Andy Jung, for putting the meeting together.

So, now it’s my pleasure to turn the meeting over to Tim Gunter, who will get the meeting started.

GUNTER: Good morning or afternoon, as the case may be. Thank you, everyone, for joining our meeting with the Nuclear Waste Technical Review Board today. As Dr. Bahr said, my name is Tim Gunter. I am a federal program manager with the U.S. Department of Energy. My main area of responsibility is research and development on the disposal of spent nuclear fuel and high-level waste.

I’m going to share my screen, hopefully. Alright, so continuing on, this is going to be a dual presentation with Geoff Freeze, who is the manager at Sandia National Laboratories for the work on the dual-purpose canister and direct disposal activities. So, Geoff, you can go ahead to the next slide.

This first slide is a disclaimer. I’m going to spend just a very brief minute on it. You’ll see this on most of the presentations throughout the next couple of days. And basically, what this says is there are standard contracts between the U.S. Department of Energy and the utilities on what is acceptable to dispose in terms of spent fuel, and some of the work we do is not necessarily in alignment with those contracts. So, this disclaimer just represents the fact, and reminds folks that we do research and development. We look at, you know, a lot of different issues, a lot of different scenarios on things that could be done potentially, feasibility studies. And that doesn't necessarily mean that this is the department's plan or that it would be acceptable in terms of the contract without, you know, modifications by the parties. So, it doesn't supersede anything that is specified in the standard contract. Okay. Go ahead, Geoff.

So, I'm going to start out with a little bit of just the way things are laid out for the next couple of days, at least, focus more on the organization. I know that Dr. Bahr went through the meeting agenda and gave you an idea of what presentations you're going to hear.

But in terms of the way the department is organized, all this work falls under the DOE's Office of Spent Fuel and Waste Disposition. That office is broken down into different areas of responsibility. The first is

Spent Fuel and Waste Science and Technology. That's the office that I work in. And there we do all of the R&D for the storage, transportation, and disposal of spent fuel and high-level waste. As I said, I'm the manager for the disposal. Those will be the first three presentations you hear today on the DPC and post-closure criticality, and also on cladding degradation.

That group is split into not only disposal but also storage and transportation, and Ned Larson who is the federal program manager for that area will be speaking to you later this afternoon along with a couple of presentations from the national laboratories.

The other main group is in the SFWD office is the integrated waste management. You'll hear presentations from them starting tomorrow. And then finally tomorrow afternoon, our consent-based siting group will update you on the activities that the department is taking on consent-based siting, with initial focus on siting a federal interim storage facility. Okay, next, Geoff.

Just a reminder of what our mission is. I know a lot of people have seen our mission before, but I just want to remind folks and then also, you know, there may be people new listening that haven’t seen this before. So basically, we’re an R&D program. We identify alternatives and conduct scientific research and technology development to enable storage, transportation, and disposal of used nuclear fuel and wastes generated by existing and future fuel cycles.

Sometimes you’ll see the term used nuclear fuel that we used early on in the program we generally use spent nuclear fuel more now. So, if you see either term, we’re really talking about the same thing. And this goes back to a photo of the cover page of our implementation plan, which was developed back in October of 2014, which lays out all of the details of the program that we have and is basically still in effect today as it is. Next, Geoff.

Our campaign structure, this shows again the SFWST group with the split between storage and transportation research shown on the left side of the chart in gray – you jumped ahead – there you go -- and then the disposal research in blue on the right side of the chart.

We have it broken down into multiple focus areas, and then the host-rock investigations. There are three main host-rocks which we’re looking at primarily, argillite, or clays and shales, crystalline, such as granites, and then salt geologic medium. The box below that are more cross-cutting areas that we’re looking at. But there’s really overlap a lot in pretty much all of these.

And we’re focused today on the direct disposal of dual-purpose canisters like you see an arrow pointing to that, and also there’s other areas that feed into that or are crosscut into the DPC, such as the one above it, the Geologic Disposal Safety Assessment, or GDSA, which is basically a new performance assessment model. Also, engineered barriers and then the inventory and waste form characteristics and performance down below. Okay, next.

FREEZE: Tim, sorry to interrupt, but Bret and others, I’m getting notes that the webcast isn’t working again. I’m not sure if others are getting that.

LESLIE: Yes, Tim, and we’re going to go ahead because we are recording this, and the recording will get online. So, we’re not going to stop, so just continue, please.

GUNTER: Okay.

LESLIE: Thank you.

GUNTER: Alright, so this slide gives a little more on our concepts and goals that we’re working towards. Like I said, you know, we want to provide a technical basis to show that we have multiple viable options for disposal in the U.S. We primarily focus on three what we refer to as reference cases. You'll probably hear that term if not today certainly throughout future meetings. What we mean by reference case is that in developing our performance assessment models, the GDSA, of course in developing a model you have to have something, a foundation, design assumptions, you know, parameter inputs in order to develop that model, so we have these reference cases in argillite, salt, and crystalline that we use for that purpose.

Really, one of the points I wanted to make and remind folks here is that we're not trying to develop an off-the-shelf design, you know, that could be pulled out and used in the future. We're using these reference cases to really develop capabilities, maintain and develop capabilities, and to develop our, you know, the GDSA, the new model, which is an advanced model based on high performance computing. So, you know, a little bit faster than the old models, and also, we look to increase, you know, the robustness of the analyses. Also, we don't necessarily try to compare the three different models to, you know, to each other. It’s really just about, you know, developing our analysis tools to use in the future.

And then the final bullet there is, we also rely on international experience. We collaborate with numerous other countries who have nuclear waste programs and that are, you know, pursuing geologic disposal. Okay, go ahead, Geoff.

Then, this is my final slide, and this is just kind of a generic conceptual timeline, you know, that could be implemented as we go forward. The real point here is that DOE in our repository development program we are now very early on in the program. So, in that red block up on the top left, we’re in a concept evaluation phase where we are evaluating disposal concepts. We look at FEPs, which is features, events, and processes that could impact the performance of any repository, develop and demonstrate technologies, and then generic research and development.

Then, I guess the last thing is to get us to where we are going with the rest of this presentation. In addition to just generic repository development and performance assessment, we look at specific issues that have interest of repositories going forward, such as technical issues that we may want information on, or in the case of the dual-purpose canisters, feasibility studies of whether these existing loaded canisters that are in dual-purpose, which means they’re designed and licensed for storage and transportation, but not disposal -- whether there’s a way that they could be directly disposed of without repackaging, which has numerous advantages, you know, from time savings to cost savings to personnel radiation savings. We just have to be able to demonstrate, you know, from a safety standpoint and a performance standpoint that it would be an acceptable path to directly dispose of those in a deep geologic repository.

Okay, next. Okay, and Geoff, I think this is where you were going to pick up, so I will mute, and you can take over.

FREEZE: Okay, thanks, Tim. So, yeah, the rest of these slides are a quick updated overview of the activities that are going on in the DPC direct disposal R&D, work package. Tim showed this part of the slide a few slides ago. Here’s the work on the direct disposal of DPCs, and this is integrated with many other work packages as you’ll hear about, argillite disposal, the GDSA work, the performance assessment modeling, the engineered barrier system R&D, and the waste form R&D are what specifically you’ll hear today about the cladding work.

This is updated from, or there’s going to be an update from when this was last presented to the Board, I believe around July of 2020, when it was presented by Ernie Harden. Ernie has since retired from Sandia, so I’m presenting this as the control account manager. Laura Price has taken over from Ernie as the technical lead, and you’ll hear from her a little bit later today.

So, the DPC disposal is of course investigating the feasibility of the disposal of commercial spent nuclear fuel in dual-purpose canisters, and those would be disposed of not just the canisters themselves, but they would be in some sort of designed disposal overpack. This work is led by Sandia, but it’s a group effort by a number of different national labs that are listed here.

Just to be clear, we use the term DPC somewhat broadly, and it sort of collectively refers to the large number of multi-assembly canisters that are currently used by the utilities to store CSNF in dry storage. They’re called DPCs because they’re licensed for storage and transportation, but they are not designed for disposal. So, all of this work is looking at potential alternative to what would be needed to repackage all of this already-loaded spent fuel into some sort of disposal-ready canisters.

So, why might this be a problem? The graph on the left here shows a projection of U.S. inventory of commercial spent nuclear fuel. On this particular projection, shown with the red line, this is the total inventory that would be produced from existing reactors under existing licenses, with no new reactors built. Even under that scenario, you can see by the end of the century we'd have on the order of 140,000 metric tons of spent fuel in dry storage, you know. And on the order of 10,000 DPCs, which would need to be disposed of.

To put that in perspective, that's, you know, two Yucca Mountains worth, 140,000 metric tons. Back in 2008, when Yucca Mountain license was submitted, very little spent fuel was actually in dry storage, so it made sense the Yucca Mountain license proposed that spent fuel be loaded into transportation, aging, and disposal or TAD canisters, which were specifically designed for the Yucca Mountain conditions. But now, or at least at the end of 2020, we now have about 42,000 metric tons in dry storage as shown here and about 3,300 DPCs. So, even if a repository were to come online in the next decades, we’d have to do something with this fuel that’s already loaded in the DPCs, and that’s the focus of this research.

So, what are the options? Well, we mentioned repackaging, so if we were just to repackage all of the spent fuel from DPCs into some sort of standardized or specialized canisters, it would be costly. There would be a dollar cost, but then there’s also radiological, operational, and management risks associated with opening the canisters, and the cost of repackaging itself could be on the order of $20 billion. This comes from the cost of new canisters, the actual standardized canisters, the repackaging operations themselves, and the disposal of the DPC shells and baskets. Of course, to design a specialized or standardized canister, it’s beneficial to know what the geology and the design of the repository is, which, at this point, that would be somewhat uncertain in the U.S.

The second option is to just continue storing the spent fuel at surface facilities, repackaging as needed. This, of course, is an interim solution. The NRC recently published NUREG-2157, the continued storage report, which suggests that storage could be safe, dry storage, could be safe for on the order of 160 years after the end of a reactor’s licensed lifetime. So, again, that’s an interim solution, so currently the spent fuel is safe, but we still do need an ultimate disposal solution.

The third option of course is to construct a repository that can accommodate DPC-based waste packages without repackaging, and that’s the subject of the research that’s going on.

Four activities that we’re focused on are listed here; operational and post-closure safety, engineering feasibility, thermal management, and post-closure criticality. The first three that are shown in blue were the subject of prior R&D, and that’s been documented in these reports, Hardin et al 2015, SNL 2021. And these reports suggest that these things are achievable in multiple geologic media as documented in those reports.

The focus of the ongoing R&D is looking at the potential for post-closure criticality, which arises because the DPC fuel baskets are designed to control criticality for short periods of time, essentially during storage and transportation timeframes, but not disposal timeframes. During disposal, the DPCs, you know, are expected or are likely to breach and flood with groundwater, which of course is a moderator. The aluminum-based neutron absorbing materials currently used in the DPCs tend to corrode fairly quickly, from long-term exposure to groundwater. So that leaves us with a fuel and package degradation and the potential for a critical configuration.

So, there's two flavors of, sort of, post-closure criticality R&D. The first is things that could contribute to the DPC disposal without modification. So, these would be already loaded DPCs and any future loaded DPCs.

These two activities, the first we refer to as the reactivity margin. This just involves detailed as loaded analyses of existing DPCs to see if some of them may actually have a low enough $k_{effective}$ that they wouldn't go critical under disposal conditions. Other things in this category include the possibility of high-performance overpacks that might keep out groundwater, and also noting that geologies with high-salinity groundwater -- high-salinity groundwater tends to limit the reactivity and so more DPCs could be shown to not go critical in those types of conditions.

The second activity is the criticality consequence studies. Laura Price is going to talk about this in the next presentation, so I’m not going to spend any time on it here. You’ll get to hear from her in a few minutes.

The second group are disposal of the DPCs with some modifications, either to the already loaded DPCs or potential design changes for future loaded DPCs. So, the first of these is the injectable fillers. We’re currently looking at two categories of these, the cementitious fillers, which are focused on calcium phosphates, and molten metals -- low melting point metals. The idea here is that these could be injected without opening up the DPCs. They’d be injected through a vent port, or they might require a small hole to be drilled to inject them, and this would fill up the void spaces so that the groundwater moderator would not be able to enter the DPC under disposal conditions.

The second category are the modifications, so these would be various aspects of either fuel assembly modifications or basket design that would, in the simple sense, replace the existing aluminum-based neutron absorbers with more corrosion resistant materials, you know, for example, nickel, chromium, molybdenum, gadolinium alloys. So, there’s some laboratory work going on to look at that.

Finally, one other thing that’s been going on is, we recently initiated an independent technical review of the DPC disposal work. The objective, as is shown here, was to try to get feedback from independent reviewers on the technical approaches that we’re using. These were industry people, so we also wanted to learn if, you know, some of the things we’re proposing made sense to industry and if they had any additional ideas on things that maybe we should be doing.

So, the specific members are shown here. Six people with expertise in a range of storage, transportation, and disposal topics, licensing, nuclear engineering, nuclear physics, post-closure performance assessment, fuel canister and basket performance, and geohydrology. This work is ongoing. DOE is currently evaluating the independent technical review observations. And so, this should be ready in the next month or two, at which time we can share that.

So, in summary, this just graphically shows the five different post-closure criticality things that we’re working on, and whether they might be applicable to currently loaded DPCs, or future loaded DPCs.

So, there’s the references, and that’s the end of our presentation. Thank you.

BAHR: Okay. Thanks, Jeff. I think I can bring myself up. First, I just want to apologize to those of you out in watching the live stream we were encountering some projection and audio difficulties. I believe those have been resolved. The presentation that proceeded the resolution of that will all be available in the recording that is posted on the web. If you missed some of that, I encourage you to go see the recording in a few days.

I see Dr. Lee Peddicord's hand up, so if we can bring him into the spotlight, we can get his question.

PEDDICORD: Hello. I am assuming you can hear me. This was a question to Tim Gunter on his presentation. It is a quite small point, but when you were showing the various collaborations with the media, under salt you had an asterisk under that one and it referred to, I think a 2019 commercial waste disposal. Yeah, this one is here. Tim, the little footnote there in the corner. Can you amplify a bit on that? What is this referring to?

GUNTER: I think that's, well, Jeff may be able to help on this too. But I think it was a commercial waste disposal case that Sandia had developed back in 2019.

PEDDICORD: Uh-huh, okay. It doesn't have anything to do with the interactions with Germany.

GUNTER: No. No. Because in fact, I mean, it says considered by Germany and the Netherlands, and in the case of Germany, they are now opening things up to consider. Things other than salt.

PEDDICORD: Thank you, appreciate that.

FREEZE: All of the generic cases are sort of collections of information and material properties from various different places.

BAHR: Okay. I see Tissa's hand up.

ILLANGASEKARE: Thank you. This answered my question may come up later. But when you select the three generic cases, the geology can be anywhere. So, you could have same geology in any geographic location in the country. But when you look at the other drivers like climate and stressors, external stressors, I am assuming in your performance modeling you will factor in the variability of climate conditions that can exist at different parts. Let's say you select a certain type of formation in eastern United States or the south it will be different. Are you factoring that in the model later or the simulations?

GUNTER: Yeah, that certainly will be considered. We are doing a little bit of that now, but obviously, you know, climate and environmental conditions are something that are very -- are going to be very site specific. So, we are limited in the generic sense of how much detail we can get into and how that would be implemented in the models.

Certainly, it would be a big part of the performance assessment. And, you know, the reason we are doing the three main geologies is first, if you look at international communities that are pursuing nuclear waste disposal in a deep geologic repository, these are the three that primarily they are looking at. In some cases, it is one. In some cases, it is more than one per country. In the case of the U.S., we actually have all of these geologies and more really that could be potential suitable sites. And as we are moving towards a consent-based siting approach not just interim storage but also permanent disposal, we have no way of knowing how that will turn out in terms of, you know, which communities will be willing to host a disposal site. What geologies, they have that are available and where in the geographically in the U.S. that might be located.

ILLANGASEKARE: Thank you.

BAHR: I see Paul Turinsky’s hand up as well as a hand up from Dan Ogg, but we will go to Paul first.

TURINSKY: Okay. I have two questions. I think one is for Tim and the other is for Jeff. You showed this conceptual timeline on that. Where in that timeline would the decision be made that direct disposal of this DPC is the preferred path that is going to be taken?

GUNTER: I don't think there is a specific spot on that timeline where you can say this is where a decision would be made. I mean, obviously we have to complete feasibility studies to the point we are comfortable to say, yeah this is an option that can be done. And we will have to coordinate with industry because they will have to, in some cases, agree, depending on the specific option we will look at. We will have to have collaboration with industry to implement some of those. I would say that obviously the sooner the better; right? We are continuing to package fuel and inventory is growing. So, that would be the only thing I would be able to say at this point. I don’t have a specific time, but the sooner the better.

TURINSKY: Let me clarify a little bit. Would it be made prior to site selection characterization? In other words, it is going to influence the site selection and characterization.

GUNTER: Direct disposal of, you know, DPCs would be more suitable to certain types of geology and certain sites than others.   But that's not to say it could not work in any of them. But again, it would be preferable to have a decision earlier. I mean, not to say, you couldn't make modifications to your site design at some point during the -- or after the characterization. I think, certainly before you build and open it, I think at that point it is obviously too late to do much in terms of accommodating if it is not already built into it.

FREEZE: I would, Tim, I would just add here too, yes it could factor into the site selection, but it turns out DPCs cannot be shown to be disposal at the specific location, you can always repackage, then it is just a financial, it is a cost. An added cost to that particular location, which could or would be factored into the decision.

TURINSKY: What I was thinking is that if you made the decision before site selection, it would really narrow your focus when looking at sites. Sorry. That's my phone that I forgot to disconnect. Okay.

And the second question is, is there any plan to do a very detailed quantitative risk assessment of the post-closure performance with repackaging and with bare fuel repackaging versus direct disposal so that you could compare it on a risk assessment basis?

FREEZE: So, the ongoing work under GDSA is looking at -- I don’t know what's the right term -- we are looking at DPCs, just in general, sort of the radionuclides and the thermal loads without worrying about criticality, possible criticality constraints. In that sense, we are looking at one aspect. Laura is going to talk about potential, I will call it a bounding case. If there were a criticality what the performance assessment would look like.

As far as repackaging, we can also -- we have the capability to do that type of performance assessment. We are not currently doing it. If we got to the point of having to compare specific locations, we would be able to do that.

TURINSKY: It would seem a comparison like that would sort of provide some guidance on the preferred approach.

GUNTER: Yeah, but I would say keep in mind, either way, the bottom line is you will have to meet the regulatory performance criteria. And, if both of them, you know, through our analysis is shown to be below the regulatory limits, comparing the two, it might be interesting information, but I don't know how real necessary. I mean that's premised on the assumption that we would have multiple sites offered up to choose from. And we don't really know what the condition or the situation will be in the future. It may be one site. Again, on a consent-based siting process it might just be one, might be more or might be zero.

TURINSKY: Yeah, I was thinking of a single site and then looking at the two alternatives.

GUNTER: Okay. All right. But again, as long as they both and you have done the analysis and either one of them meet the regulatory performance criteria, there may be other factors that is play in the decision.

TURINSKY: There are, I mean there are certain risks associated with repackaging clearly. And there are certain risks of moving very heavy packages, so it goes throughout the whole disposal packages, from basically starting repackaging to long-term performance.

GUNTER: Right.

TURINSKY: Okay. Thank you.

FREEZE: You know, just …

BAHR: Sorry. We are about out of time, but I see Dan had a question. I think we had time for one more question before we move on to the next speaker.

OGG: Thank you, Jean. And thank you Tim and Jeff on the presentation. On slide 7. The timeline. Tim, you pointed out you are in the conceptual evaluation step that you have the arrow pointing “we are here.” And I just want to clarify this graphic. Does that indicate that you are also, in parallel, doing some early site selection work? Or is that really a sequential step and that will happen after you finish the conceptual evaluation step.

GUNTER: Well, under the R&D side, we are not really doing any siting work at the moment. I mean, like I said this is a kind of a high-level conceptual timeline. It is not necessarily a detailed and accurate representation. The real point was to show that we are early in the sequence. But as it is laid out, at least the way it is represented, there is a bit of overlap between siting guidelines and criteria and conceptual evaluations.

OGG: Right, but for now your group on the technical side is not doing any of that early site selection work.

GUNTER: That's right.

OGG: Okay. Thank you. That's all I have.

BAHR: Thanks to Tim and Jeff, and now we need to move on to our next speaker who is Laura Price from Sandia National Laboratories. And she is going to follow up talking about these performance assessments that incorporate post-closure criticality.

PRICE: Yes, following on to what Tim and Jeff talked about, I will be talking about the efforts we have been making towards doing repository-scale performance assessment modelling that incorporates post-closure criticality. Next slide, please.

This is the disclaimer that Tim talked about and so we will skip past that one.

As Tim and Jeff both said there is a lot of people that are working on this, and this is the list of people who have done the work that I will be discussing today. I certainly didn’t do it all and as you can see there are lots of different people from lots of different organizations, and I appreciate all the good work they have done to get as far as we have on this. Next slide, please.

So, there are four topics I will be covering today. The first topic is the objectives and scope of the DPC criticality consequence analyses to define what we cover and don't cover. Second, I will be giving an overview of the two different repository concepts and the two different post-closure consequence scenarios that were being considered in these analyses. And Jeff kind of had a short slide on those. I will of course, be expounding on those more. Third, I will give some of the recent major accomplishments and indicate how these results are informing the technical activities that we are doing now and in the future. And finally, I will talk about the probability of occurrence and uncertainty and how they are being addressed in our analyses. Next slide, please.

So, one of our objectives was to evaluate the features, events and processes, often called FEPs, that could affect post-closure criticality or could be affected by post-closure criticality or both. The reason for this was so that we would know which processes and events to include in our models. For example, if we neglected evaporation of water as a result of the heat generated by a steady state post-closure criticality event in an unsaturated environment, we would have overlooked a very important thermohydrologic process.

Another objective was to develop the tools we need to model the consequences of post-closure criticality. That is, we are trying to build the modelling capability to include the feature events and processes that we identified in that first objective in our models of post-closure criticality. As an example, we need to modify PFLOTRAN, which is a massively parallel computer code used to model subsurface reactive transport. We had to modify it to be able to change the inventory and heat output at the waste at some arbitrary time, midway through the simulation, to represent the inventory and thermal effects of a post-closure criticality event. Usually, the assumption for a repository that does not experience a post-closure criticality event is that inventory is set at the time of disposal and can be estimated in the future by simple decay and in-growth calculations and the thermal output is monotonically declining as the heat generating nuclides decay. This all changes when there is a criticality event and one of the first things, we had to do was to build this capability into PFLOTRAN. We are also refining our coupling between neutronics calculations and thermohydraulic calculations and we are building a sub module in PFLOTRAN that specifically addresses the features, events and processes that are associated with the post-closure critical event.

The third objective is to identify the processes that could lead to permanent termination of a criticality event. It is important to be able to know the conditions under which a criticality event might cease permanently as a part of modelling the post-closure criticality. Finally, we wanted to identify areas where further work is needed.

With respect to scope, we are considering post-closure criticality in dual-purpose canisters as Tim and Jeff talked about DPCs. We are not examining post-closure criticality external to the waste package, only internal to the waste package, nor are we considering waste other than commercial spent nuclear fuel. Also, in the work I am describing here we are describing only consequences. We are assuming that a criticality event occurs; we are not considering probability of occurrence at this time. Next slide, please.

So, moving on to repository concepts post-closure scenarios and assumptions, we’ll begin with repository concepts. We considered two repository concepts. One is saturated geologic environment and one in an unsaturated geologic environment.

We selected the hypothetical shale repository which Tim showed a picture of for a saturated geological environment based on the reference case developed a few years ago as part of the GDSA program. This hypothetical repository is a layer of shale at a depth of 500 meters where the depth hydrostatic pressure is 50 bar or 5 MPa -- megapascals. The saturation temperature at that depth is 264 degrees Celsius. This is an important number because we are assuming that the temperature in the waste package cannot exceed 264 degrees Celsius in a steady state criticality, which I will be describing in a few slides here, because of water will boil at that temperature and the system will become subcritical. That is our conceptual model. At least, it becomes subcritical temporarily until the water cools off and then criticality could start again.

The waste packages are emplaced horizontally in long drifts that is backfilled, surrounded by bentonite, which is also a clay material. And the waste packages consist of a DPC inside an overpack made of stainless steel 316 that’s 5 meters long. The center-to-center waste package spacing along the drift is 20 meters and the centerline-to-centerline spacing between drifts is 30 meters.

The model we have is repository-scale that contains 4,200 waste packages and all the waste packages contain spent nuclear fuel from pressurized water reactors. The repository has an upper sandstone aquifer intersected by a well 5 kilometers downstream. And this well is important because it is used to calculate the dose to a member of the public. The assumption there is that the member of the public drinks two liters of water per day. And the next slide has a picture of this repository, so next slide please.

So, there is several different pictures here. The upper picture is the entire modelling domain. The repository is on the left side where those kind of yeah, Jeff is pointing to it right now. There is a well in the upper right-hand corner shown in turquoise. It is really hard to see. Jeff has found it right there. This is the well the member of the public gets water from.

On the lower left picture, zooms in on the repository and you can see the drifts, those parallel lines in which the waste is emplaced.

And the lower right you can see the well, zooming in on the well, and its location in the top of the modelling domain. This well reaches into the sandstone layer which you can see in that picture, and this becomes relevant when I show you the results because you will want to know where the sandstone layer is. Next slide, please.

So, this is a bird's eye view of the repository. Those long lines represent the drifts. The upper picture, the picture in the upper left, shows the waste package spacing, a little hard to see, there’re red dots. Those red dots are the waste packages. The orange is the buffer and backfill. We use those words interchangeably -- the bentonite. And those kind of yellow-orange lines along the side are the disturbed rock zone. Once again, these locations are important because when I talk about the results a few slides later. Next slide, please.

So, moving on to the hypothetical alluvial repository, this is the environment that we chose to examine post-closure criticality in an unsaturated geologic environment. The repository concept is based on the alluvial reference case that was developed a few years ago as part of the GDSA program.

It is a depth of 250 meters in an alluvial basin. The infiltration rate varies from 2 mm per year to 10 mm per year. As a point of reference, an infiltration rate of 10 mm would represent a very wet pluvial environment. Because it is above the water table the pressure is ambient and the saturation temperature is about 100 degrees Celsius.

Each waste package consists of a DPC which is placed in an overpack, also made of stainless steel 316, and it is five meters long which is emplaced horizontally in drifts surrounded by alluvium. The waste package center to center spacing is 40 meters and center line to center line spacing between drifts is also 40 meters.

In this case, we have a model of a single waste package not a repository filled with thousands of waste packages. This single waste package is assumed to have its top removed 9,000 years after repository closure upon which time, the criticality event can begin. We will talk more about that later. Our goal is to have repository-scale model of the unsaturated model like we do for the saturated repository, and we are still working on that. I will show you a picture of the single waste package in the next slide. Next slide, please.

So, this is one quarter scale model of the hypothetical unsaturated alluvium model. The light blue in the middle is the DPC and the contents of a DPC. The dark blue line is the over pack. Thank you, Jeff. The green is the alluvium backfill and the brown is the alluvial host rock. Next slide, please.

So, moving on to post-closure scenarios, we consider two different scenarios for each hypothetical repository. A steady state criticality and a transient criticality. Consistent with the DOE’s criticality topical report, the primary concerns with respect to steady state criticality are the thermal effects and the change in the radionuclide inventory as a result of fission. The steady state criticality event is characterized by a low power, anywhere from 50 watts to 4 kilowatts, with a higher power being associated with a saturated repository and the lower power being associated with an unsaturated repository. It is also characterized by having a long duration when I say a long duration, I mean hundreds of thousands of years. In the steady state criticality scenario, failed waste packages fill with water 9,000 years after closure.

It is important to note here that criticality cannot occur unless a waste package has failed and filled with water. Otherwise, there is no moderator to allow criticality. Criticality doesn't cause a waste package to fail. The waste package has already failed before criticality can occur. For the saturated repository, we assume the waste package fills with water when the waste package fails, which is 9,000 years. While for the unsaturated repository, filling of the waste package with water is a function of infiltration rate.

It is also assumed that all waste packages become critical, which for the saturated repository means all 4,200, and for the unsaturated repository means the single waste package. This is not a reflection of what we expect to occur, it is simply a calculation of convenience, and we are working on modifying the model to allow different waste packages to experience criticality at different times in different locations. The power level of the criticality event is handled differently in the different environments. In the saturated repository it is determined by the saturation temperature which is as I mentioned before 264 Celsius. And the unsaturated repository it is varied to determine the evaporation time and refilling time of the waste package. Next slide, please.

So, continuing with the steady state saturated shale repository, it is assumed the post-closure event lasts for 10,000 years, which is what was assumed in the past. For the unsaturated repository, the steady state criticality event lasts until the water evaporates. For the hypothetical saturated case, we also considered changes in the permeability of the bentonite buffer as a function of temperature because the bentonite buffer stays hot for 10,000 years.

Finally for the saturated case, the consequence is dose to a member of the public, it should be noted that the absolute value of the dose is not important here. What is important is a comparison of the dose with steady state criticality and the dose without criticality as we talked about in the discussion. So, we run two cases, one with criticality and one without criticality and we compare the dose. And that's what is important is the comparison of those two values.

For the unsaturated case, the consequence was the power that could be supported before the water evaporates, and the time required for evaporation refilling of the waste package. And consistent with the DOE's topical report, the primary concern is a mechanical effect on barriers and their properties in the transient criticality case. These events are higher power, hundreds to 100,000 megawatts in short duration of 0.01 to 10 seconds. So that's the one main difference between the transient criticality and steady state criticality. Next slide, thank you.

Moving on to post-closure scenarios, we considered two different scenarios for each hypothetical repository, steady state criticality and transient criticality. Consistent with the DOE's criticality topical report, it went to the wrong slide, sorry.

In our model, reactivity insertion rates in the transient criticality, reactivity insertion rates are consistent with the sudden loss of the neutron absorber and how long it might take that neutron absorber plate to fail.

We used an established neutronics code to develop a neutronics model for a single waste package and we varied the insertion rates and the insertion period. We used two different codes one was Razorback for the unsaturated model, and we used commercial code SIMULATE3-K for the saturated model.

For a range of reactivity insertion rates and insertion periods, we calculated the peak power and the power peaking factor, total integrated energy, maximum and average fuel temperature, maximum and average coolant temperatures, time of peak power, and maximum reactivity. That’s what we calculated. This is all for the transient criticality scenario. Next slide, please.

So, here is our assumptions that we made. We assume the waste packages failed 9,000 years after closure and we assume criticality occurs. We don’t identify the mechanism that causes criticality to occur or the waste package to fail. We simply assume that waste package is failing, and criticality occurs.

We also assume that the fuel assembly remains intact, but the cladding has pinholes in it that permits radionuclide transport. This assumption ties into the next presentation on cladding failure, cladding degradation, because assuming the cladding degrades when the waste package fails is not conservative for the purposes of post-closure criticality modelling. If you have immediate cladding failure that would lead to reconfiguration of the fuel pellets into a subcritical configuration. So, for the purposes of this analysis, the cladding is assumed to remain intact.

We further assume that the post-closure performance requirements are similar to those in 10 CFR 63 and 40 CFR 197 such that the dose is a relevant measure at least for this saturated repository. We will get there for the unsaturated.

We further assume that the basket neutron absorbers have degraded, which Tim and Jeff alluded to in the previous presentation, such that the system is no longer subcritical. Oak Ridge has done extensive criticality calculations using stylized scenarios for potential post-closure scenarios. And these calculations have shown that some as-loaded DPCs could become critical under these stylized scenarios under post-closure conditions.

And finally, we assume that the steady state criticality does not oscillate between supercritical and subcritical, or critical and subcritical, as it would be in an uncontrolled environment. We assume for the purposes of the calculations steady state means steady state. That the heat remains steady at a constant temperature and is constant power output and it doesn’t shut off for 10,000 years, at least for the saturated case. Next slide, please.

Moving on to major accomplishments, first of all, we went through the list of features, events and processes. We went through several lists that have been published over the decade and we identified those that are relevant to criticality. Both those that could affect criticality and those that could be affected by criticality and those that fall in both camps.

We then started modifying PFLOTRAN to include these relevant features, events, and processes in the model of steady state criticality. Some of the examples of this are modifying the code to allow a change in radionuclide inventory and thermal output of the waste midway through the simulation, which I already mentioned. We also developed a loose coupling between neutronic and in-canister thermohydraulic processes and rates of heat transfer out of the canister. And that is a complicated modeling because neutronics tend to be in time scales of seconds and length scales of inches and centimeters whereas repository scale modelling tends to be on the order of thousands of years and meters. Getting those two codes to work with each other, we are not there yet but we can some loose coupling between thermohydraulics and neutronics.

We also identified the nuclides that might need to be included such as those with a 30-year half-life like strontium-90 and cesium-137 that get produced during the fissions that occur during a criticality event. We modified PFLOTRAN to be able to include the temperature-dependent and anisotropy of thermal conductivity and we modified PFLOTRAN to include the change in buffer permeability as a result of thermal illitization of the backfill. Next slide, please.

Next, after looking to the question of what makes criticality permanently terminate, we identified grid spacer degradation as a primary means for that to happen. And we started the process of implementing that in PFLOTRAN. The model of grid spacer failure is not in the results that I will be showing, but we are working on that.

Finally, we found there was no difference between hypothetical saturated repository in which a post-closure criticality event does not occur and one in which a steady state criticality event occurs. There is no difference in the dose. That is what we mean by performance here. That's the dose.

And I will show the results of some of these simulations on the next few slides but two important points to be made on this slide. First, the only radionuclide to reach the well which a member of the public drinks 2 liters of water per day and receives a dose is iodine-129. Second, the 30-year half-life radionuclides that are produced by the criticality event, and that don't usually need to be considered in a performance assessment, they decay before they reach the upper aquifer, so they were not a concern. Next slide, please.

So, these are the results of running the simulation with criticality and without criticality. They are identical within a few percentage points of each other. The point here is that the dose is the same. The point is not what the absolute value of the dose is. This is a hypothetical situation and we have not accounted for all events and processes. But at this point, we can say there is no difference in dose between a critical event and absence of a critical event. The inventory of iodine-129 increases by about 3 percent over the 10,000-year simulation assuming 4 kilowatts power output. That is ameliorated somewhat by the change in the buffer because the permeability goes down. So that's why there is very little difference. Next slide.

So, this is cesium-137. And what you see in this slide, as you start in the upper left, it says that wp0 at the upper left. So, this is the cesium concentration at the waste package.

As you move to the right where it says bf0 that's in the buffer. As you move and you move down to the lower left, the drz is the disturbed rock zone and the next lower left is sand. As you move to left to right across the top, and left to right across the bottom, you are moving further away from the waste package and the point of this graphic is the concentration of cesium drops by the time you get to sand layer, the concentration is zero. It is shown as 10 to the minus twenty in PFLOTRAN, that’s the value to represent zero. Okay. Next slide.

This is a similar thing for strontium-90. As you move from left to right and top to bottom, you can see the concentrations of strontium drops and by the time it gets to the sand aquifer above the repository the concentration is zero. So, the point is that by the time these nuclides reach well, they don't reach the aquifer, but they decay quickly enough they are not an issue. Next please.

Moving on to a hypothetical unsaturated repository, one of things we found that there are limits to the power that can be generated. And the power is a function of infiltration rate. In general, higher powers are associated with a higher infiltration rate. So, for 2 millimeters per year infiltration rate, the limit is 50 watts to 100 watts. And it is 300 to 400 watts for a 10 milliliters infiltration rate per year.

We also found that water evaporates from the waste package at temperatures well below 100 degrees, which causes criticality to cease, such that the water in the waste package doesn't boil as well. This also means the temperature rise is not that significant and these temperatures will likely not affect barrier performance over the long term or any term because the temperature rise is just not that high. Because intermittent nature and lower power associated with the steady state critical event the increase is in the radionuclide inventory is less than about 1 percent for radionuclides such as iodine-129. Next slide.

Here is a summary of the results which demonstrates dependence of the power of criticality on infiltration. Starting with the reference infiltration rate, 2 milliliters per year, the power output is between 50 watts and 100 watts with the initiation time of 17,100 years after closure. Which means even though the waste package fails 9,000 years after closure that it takes another 8,100 years for the waste package to fill with enough water, which for the purposes of this analysis was assumed to be 1 liter, for criticality to begin. And at 50 watts, I think the waste package stays full of water. At 100 watts, the water evaporates from the waste package within about a thousand years.

With a lower infiltration rate, shown in the next row, 1 millimeter, the upper bound on the power output is 50 watts and the criticality event doesn't begin until 25,300 years after closure. So, it takes a long time for that 1 millimeter of water infiltration rate to fill the waste package.

With the 10 millimeters per year infiltration rate, that leads to a power output of 300 watts to 400 watts. Once again, with the start time of 10,600 years after closure. It takes the waste package about 1,600 years to fill with enough water for a criticality event to begin. These results all assume that the top has been ripped off the waste package.

A more reasonable assumption of a partial breach for water to fill the waste package with an infiltration rate of 2 millimeters per year leads to a power output of between 100 to 200 watts with a criticality initiation time of 22,600 years. With the partial breach, it takes longer for the water to fill the waste package, but a higher output can be sustained. In any case, it takes a few hundred years for the water to evaporate from the waste package so that criticality ceases. Water would eventually refill the waste package and criticality can start again and we are working on being able to model that behavior moving forward. Thank you.

Another major accomplishment with respect to the transient modelling case, we have used existing neutronics codes, Razorback and SIMULATE3-K, to characterize the pulse from a transient criticality event. This helps us to understand the power pulse and energy that results from the event. And we have also been able to calculate some temperatures of various components in the waste packages. Next slide, please.

With respect to future work, we have categorized this work into several categories, neutronics based activities, steady state criticality events, transient criticality events, and repository wide sensitivities and variabilities. With regard to neutronics based activities, we are working on improving the coupling between neutronics and performance assessment calculations. As I mentioned, those couplings are difficult because of the time scales and length scales involved. With respect to, we are also working on developing a model that includes spent fuel from BWRs. Right now, we have modelled only fuel from pressurized water reactors we are working on including that type of fuel as well. We are looking at reactivity multiple times greater than 9,000 years up to half a million years. And we are also looking at more realistic water compositions. Right now, we assume the water composition is pure water, so we are trying to incorporate more realistic water compositions that reflect actual repository conditions. And we have started working on some of these but not all of them.

With respect to the steady state criticality events, we want to expand our model of how the higher temperature for extended periods of time can affect the barrier materials and their properties. We want to include the effects of higher temperatures on radionuclide solubility because solubility is certainly a function of temperature. We want to continue working on implementing the grid spacer degradation model that we have included next time we run PFLOTRAN, and we want to look at the effects of gas generation on barrier performance. That could be an important effect from criticality. Next slide, please.

Continuing with steady state criticality events, we want to examine the thermal fatigue of waste package materials and how that fatigue could affect their ability to act as barriers, examine the effects of criticality of one waste package on an adjacent waste package, and examine thermally-induced-stress changes in backfill.

And I want to mention that second sub-bullet there. Examining the effect of criticality in one waste package on an adjacent waste package is of particular interest because we want to make sure the failure of a waste package in a subsequent steady state criticality event would not lead to failure of an adjacent waste package.

Moving on to transient criticality events, the plan is to calculate mechanical damage to the fuel, the engineered barrier, and natural barriers from the power pulse produced by the transient criticality event. And we have looked at several different codes that will help us do that. We want to refine our transient neutronics calculations and get a better handle of characterizing what that looks like. We want to examine the role of subcritical heating and want to examine thermal and mechanical fatigue of materials resulting from intermittent criticality as could happen in a transient event. And with a steady state criticality event, we want to examine the effects of one package on an adjacent waste package. Next slide please.

With respect repository-wide sensitivities and variabilities, we are working on being able to vary how many waste packages experience criticality, when they experience criticality, and the location in repository. That means we won't have to assume as we did previously that all waste packages experience criticality at the same time. We would also like to examine the effects of varying hydrostatic pressure for saturated repository. And that would change the saturation temperature. And we would like to increase the distance from the repository to the model domain lower boundary. The purpose for that would be ensuring that we are not seeing artificial effects induced by the boundary conditions by the boundary being too close. We also want to incorporate variability uncertainty in parameter values into performance assessment calculations. Next slide, please.

With respect to probability and uncertainty is a very short answer we are not looking at the probability of criticality occurring, not at this point. For such a calculation to have meaning, we would need a specific site, specific waste package design, and specific repository design. As mentioned, we are working on incorporating uncertainty and variability in parameter values into the model. That's all I have. Those are the references. The next slide has acronyms and any questions? Thank you.

BAHR: Okay. Thank you, Laura. I have a question to get started. You said that the 10 millimeters per year infiltration per year represents a wet pluvial environment. For someone that spent their career in the humid Midwest where recharge rates can be on the order of 15 inches a year, 10 millimeters per year about a centimeter doesn't sound very wet to me.

PRICE: It is at depth. I think the point is this is at depths in a dry environment in an unsaturated environment.

BAHR: A wet Yucca Mountain.

PRICE: Correct, kind of, similar, yes, a wet Yucca.

BAHR: Do you have plans to look at wetter conditions. Even a wet Yucca Mountain or some place that initially has a deep water table and a thick unsaturated zone there could be situations in which you could get higher infiltration temporarily.

PRICE: That is certainly something we could look at, yes.

BAHR: Given the sensitivity of the power output to the infiltration rate, it would seem like it would be worth looking at that.

PRICE: Sure, that is certainly something we could do, yes.

BAHR: I see Paul's hand up.

TURINSKY: Laura, if you explained it, I didn't grasp it. What's the canister content and how did you select it? How, size of the canister, the assemblies that went into the canister, their composition?

PRICE: Yes, I probably didn't describe it. I should have. So, what we do is, Oak Ridge has the UNF-ST&DARDS database. They selected one of the canisters, so we use the as-loaded contents of one of the canisters in their database. I can't tell you off the top of my head what it was. But they could identify it for you.

TURINSKY: I assume it is one that tends to be more easily going critical.

PRICE: Yeah, this one, when they ran their stylized scenarios on it, I forget what they came up with the $k_{effective}$ was. But it was one that would go critical under repository conditions according to their calculations. We used the actual as-loaded inventory.

TURINSKY: And has anybody done any validation for S3K for this geometry? That is a little diffusion theory code. A diffusion theory breaks down in small geometries due to anisotropic behaviors.

PRICE: Not that I know of.

TURINSKY: Okay, I would be cautious.

PRICE: We realize we are stretching the capabilities and using them not quite what they were visioned for.

TURINSKY: I saw you were doing some work with Monte Carlo. Honestly, I have to go back and read the report in more detail. And I am glad you are doing BWR fuel. The fuel, because the water to fuel ratio, is so much greater for BWR. They are designed to operate with 40 percent void fraction. I think you will get quite different behaviors when you look at the BWR.

PRICE: Could be.

TURINSKY: Yeah, okay. Thank you.

BAHR: Tissa is next.

ILLANGASAKARE: Thank you. Actually, I'm not going to simplify the issue, it is a very complex modelling problems as I see. But my first question is in the conceptual model. The post-criticality conceptual model you are basically assuming that the backfill and the engineered barriers are affected, and the other parts of the geology remain intact. I mean that the assumption seems like, most of the criticality changes the system around the backfill material and then in that system, is that correct?

PRICE: For right now, but that's one of the things we are working on. I guess I didn’t make that clear is we are trying to include -- the heat effects go far beyond the backfill which is what I think you are getting at. We are working on changing our PFLOTRAN to be able to incorporate the temperature dependance of the host rock. Anywhere, where the temperature would change significantly.

ILLANGASAKARE: Yeah, yeah. That sort of brings to the next question. Looking at your simulation, it seems like your problem is still diffusion control. What happens from the source to your well is a completely a diffusion process?

PRICE: That's correct.

ILLANGASAKARE: The material is intact, that's why -- yeah so that explains. Because the landscapes are very large, and the diffusion process takes thousands of years it could be very slow.

PRICE: Right. It is a clay environment so that's the main transport process diffusion. We have not looked at a crystalline environment where you might get convection.

ILLANGASAKARE: The third one is the constitutive model. In the constitutive modeling, eventually when you simulate the coupled thermal behavior, constitutive model should have a range of the temperatures you are dealing with, the extreme temperatures you are dealing with. So, these constitutive models, you haven't really developed or validated constitutive models for those temperature ranges. Have you done that at a laboratory scale or a small scale?

PRICE: Not really. We are trying to -- right now, if I understand your question, we are working on developing lookup tables for between to make the connection between the neutronics codes and performance assessments codes. Is that answering your question?

ILLANGASAKARE: Yeah, yeah, my question more has to do with the constitutive model which has the temperature, large temperature. I mean the constitutive model should have two things connected each other. One of them should be a temperature. Have you looked at the constitutive model validity for these large temperatures?

PRICE: Not yet. Not yet.

ILLANGASAKARE: Yeah, again, this is -- are detailed questions I should be able to find in the reports. Thank you very much.

BAHR: Just a follow up on your answer to Tissa that you haven't looked at conceptual models where you might have advective transport. Do you plan to do that and do similar work with the crystalline repository scenario? Or do you feel the shale and unsaturated case might present some sort of bounds on what might be expected?

PRICE: We don't have any plans to do crystalline right now, but that could be planned in the future. And I would also point out, the increase in iodine which is one of the radionuclides of concern in performance assessment because it is mobile and long lived. The increase in inventory over 10,000 years at 4 kilowatts was only 3 percent, which is not very significant. Unless something happened that would cause the entire repository to perform much more poorly because of the criticality event, increasing the inventory of iodine by 3 percent is not very significant in terms of overall repository performance given the other uncertainties, and inventory uncertainties that already exists. But that's just my ideas on the subject right now. We don't have any plans right now to do a crystalline repository, but we certainly could, I think.

BAHR: Thank you. I see Bret Leslie’s hands up.

LESLIE: Thank you Jean and thank you Laura for a nice presentation. Jean kind of was going where I was going to go. You didn’t say insignificant, but it is certainly very low in terms of the inventory. So, if you go back to the Yucca Mountain topical report and also the language that was used for the independent technical review. It was almost like it had the consequences had to be relative to dose had to be relatively insignificant. I think that is not quite what is in the regulation, and I don't know if you went back to that topical report from 2003 if that language is in there.

What I heard as you were walking through is it, even though it might not have significance to the dose, a criticality may have significance to the degradation of the barriers around them. So that's kind of the prospective and that's the reason why you want to continue to do the criticality analysis, is that correct?

PRICE: That's correct. We have not yet incorporated all the different possible effects on the engineered barrier that might be affected by the heat of a criticality event. So, we don't know how that would affect performance.

LESLIE: Okay. Second question which is really if you go back to the disposal criticality methodology report, you are not really following it. You are using parts of it. The way the logic outline of that is you figure out the probability first, and then you determine whether you need to do the consequences. The question about probability is if it doesn't make a difference to dose, even if you had a hundred percent probability a 3 percent change in inventory, then how important is it to know what that probability is? It doesn't real, I mean, you would have to have every package going critical more or less to have any sort of significance, is that a fair characterization or?

PRICE: Well, first of all. A couple of things trying to keep all of this straight. It is true we are not following exactly the methodology report because that methodology report was written specifically for Yucca Mountain which is an unsaturated repository. So, it doesn't always apply to a saturated repository, and it was also written with an eye toward being able to modify the waste package design which is not something -- that is not on the table for us right now. So, that's one reason why we are looking at consequence.

I guess I would say even if all of the waste package in the repository went critical, they would all have to fail first. So once again, you would be comparing the consequences with criticality to the consequence without criticality and if the inventory only goes up by 3 percent. I don't know if that's significant in evaluating features, events, and processes for inclusion in the performance assessment. Or the criticality could be included in a performance assessment either way. But that would be decisions that have to be made later. Does that answer your question at all?

LESLIE: It clarifies things a bit. That's fine.

PRICE: It gets complicated if you don't have a real site. Yeah.

LESLIE: Actually, let me go back to ask Jean's question in another way. Although you are not doing crystalline right now, are there processes associated with a crystalline repository that impact or would be impacted by criticality that are not considered currently in your argillite or unsaturated cases?

PRICE: I don't know if I can answer that right now. We haven't studied a crystalline repository in that perspective.

LESLIE: Okay, thank you.

BAHR: So, I see a comment in the chat from David Sassani who apparently has his hand up, but he is not showing up on my screen. If we are able to bring up, he may have something to add to the discussion. There we go. Thank you.

SASSANI: Hi. Thank you. I appreciate that. All I was going to add. I am unmuted on there, but it looks like my icon is muted on the image. If you can hear me, that’s great. I would say Laura hit it. We are not following that process explicitly because it’s for Yucca Mountain but even if it wasn’t for Yucca Mountain, it is relatively site specific to determine the probability of the system and how that might play through. It is beyond the capabilities we have of looking at specific aspects of some of the pieces of the system.

And in terms of crystalline, as Laura said, we haven't looked at that explicitly yet. But of course, the crystalline system does rely more heavily on the engineered barriers with the fractured fast flow pathways as opposed to an argillite repository system which relies more heavily on the natural barriers. So, you might expect you might look there if the ancillary effects of the thermal aspects of the criticality do have substantial impacts on the engineered barriers. I hope that helps.

BAHR: Yes, thanks, Dave. Do we have other questions from Board members or staff at this point? I am not seeing any other hands up. If somebody has their hand up, I'm not seeing you could contact me in the chat. I'm going to give it a minute or so. Just make sure -- oh Bret has his hand up again.

LESLIE: Yeah, and this might be a question to Geoff. Which was, you had mentioned that the independent review team is going through the observations, and you said that whatever would be ready in a month or two for review. Could you explain a little bit more about that? As you know, the Board has the capability to review draft information from DOE. So, were you saying something would be made publicly available in one to two months?

FREEZE: Yeah, Bret, so there is a combination of the observations made by the independent technical review team. That will be published and in parallel with that will be a documentation from DOE that kind of addresses or makes comments on the observations as to how those could be incorporated into our R&D planning going forward. So together, those will be ready in about a month or two. They are not quite finished yet. We would like to release them all together and they should be finished, I think, Laura jump in here if I am misstating something. I want to say the end of this month, the end of March.

PRICE: Yeah, I think that's right. That's the goal.

LESLIE: Thank you Geoff and Laura.

BAHR: Thank you. I see Lee Peddicord’s hand up.

PEDDICORD: Yes, thank you. Just to make sure I understood. I may not have. Is the -- is the assumption necessary to achieve criticality that in all cases the lid on the canister comes off? Or is that only in particular cases?

PRICE: No. So, the canisters are emplaced horizontally, so the assumption is there is a crack or a hole in the side of the canisters. And it fails in some manner and water fills the canister.

PEDDICORD: But did I hear you say something about the lid coming off or being completely exposed in some cases?

PRICE: In the unsaturated repository, in the hypothetical alluvium repository, the way the waste package was modelled they removed the lid -- it was a box. And they removed the top 9,000 years after repository closure and looked at how long it took water to infiltrate that waste package given heat that was still there from decay heat. It is not the top of the DPC as you would think. It is more like it’s a model convenience how do we make this thing fail and have water enter it.

PEDDICORD: Okay. Thank you.

BAHR: Bret Leslie, again.

LESLIE: Let me go back in terms of the geometry. You are filling this as a bathtub, correct? You are not making any assumptions, if it corrodes on the top, it is not going to corrode on the bottom?

PRICE: Correct.

LESLIE: And is that realistic?

PRICE: [Laughter] We are making the assumption because we want to look at the consequences of criticality, we have to make assumption that allow criticality to occur. That's our objective in this. Once again, we don't have a real site. We are hypothetical over packs and hypothetical sites we are making assumptions that allow criticality occur.

LESLIE: Okay, thank you.

BAHR: Looks like Geoff wanted to add something to that.

FREEZE: I was just going to add, Bret, in general, the conditions that lead to a critical configuration, there is so much uncertainty in the degradation, et cetera. Laura even mentioned that we need to have the cladding stay intact and our assumption is that is required to keep a critical configuration. So, we are kind of doing our best to create, you know, simulated conditions that would lead to a critical configuration. What the reality of this is, 10,000 years, a hundred thousand years in the future is very complicated. I will just leave it at that.

PRICE: And Bret, to kind of go back. We don't have the analysis to support modeling where there might be a hole in the bottom, so the bathtub configuration is lost.

BAHR: Okay. Any other questions from Board members or staff?

ILLANGASAKARE: I have a brief question.

BAHR: Is that Tissa?

ILLANGASAKARE: Yeah. I cannot get my video back. So, when you say bathtub assumptions. From a look at the larger system, does it matter because you are looking at the source, which is a small area and then you basically looking at the performance or the risk of a well very far away. If you assume different geometry in the source, it didn't matter.

PRICE: For the saturated case you don't have to assume a bathtub. You can have hole in the bottom of the waste package. The waste package is in a saturated environment surrounded by water. You don't have to make assumptions about a bathtub. With the unsaturated environment the waste package has the to remain in what was called at Yucca Mountain a bathtub because if the bottom of the waste package corrodes, then water will flow through, and the waste package can't contain water as a moderator and there will be no criticality.

ILLANGASAKARE: Got it. Thank you very much.

BAHR: Lee Peddicord had another question.

PEDDICORD: I was going to ask what are your assumptions for U-235 concentration in the fuel assemblies?

PRICE: So, we modeled as-loaded assemblies so the data we got from the UNF-ST&DARDS database. These are not assumptions we are using as loaded in the DPC. As a 37 -- it is a 37 PWR DPC.

PEDDICORD: So, is it nominally now 0.8 percent 235 now?

PRICE: I think it is less than that. It is on the order -- each assembly is different obviously. And I don't remember off the top of my head what they were. I don’t think they are close to 8 percent, there are all under 5.

PEDDICORD: No. 0.8 percent. They are burned assemblies.

TURINSKY: More like 1 percent. Depends on the discharge burnup. 0.9 to 1.1 percent.

PEDDICORD: But you are using actual data.

PRICE: Yes, actual as-loaded data for one DPC.

PEDDICORD: Good. Thank you.

BAHR: Anything else for Laura? At this point we are scheduled to take a break. And that break will last until 2:10 Eastern time. That will be 1:10 central, 12:10 Mountain, and 11:10 Pacific. So, I will go silent now and we will see you back in about 15 minutes. Thank you. [Break until 2:10 EST.]

BAHR: Okay, according to my clock it is time to get started again and I see Laura is going to lead off the next presentation which is joined by Brady Hanson on degradation modeling and development. So, I see Laura. So, if we can bring Laura into the spotlight. There we go.

PRICE: Okay. Brady is running the slides. Good afternoon, everybody. Once again, I am presenting this with Brady Hanson and we are talking about the cladding degradation modelling that we have been doing recently, he and Pat Brady have been working on this. Pat Brady has since retired. But we are discussing this and how it interact with the previous -- we how this interacts with the previous work we have been doing. Next slide.

This is the disclaimer, so we can move on. You have already seen this one.

The first topic I will be covering today is a brief overview of the processes that are considered in the geologic disposal safety assessment framework. We talked about this earlier in the first presentation. Tim Gunter talked about this and so did Geoff.

This is called the GSDSA framework. This is a modeling capability that has been developed over the last few years to model generic disposal facilities in different types of geologic media such as clay, crystalline, and salt. And the model has some standard source term processes and those will be summarized.

Next, I will discuss why the renewed effort on developing a cladding degradation model. One reason which I alluded to in my previous presentation is that it was driven by the DPC criticality consequence analysis. The previous and commonly held assumption, as Brady will talk about, is that cladding unzips when the waste package fails. So, there is nothing to keep the fuel pellets in place. This may be conservative for performance assessment that does not include post-closure criticality but for a performance assessment that does include post-closure criticality such we discussed in the last presentation it is not conservative. For a post-closure criticality event to occur the fuel pellets need to stay as configured in the fuel rods. A loss of configuration such as would occur if the cladding unzipped would lead to sub critical conditions and not allow post-closure criticality to occur. Another reason is to simply improve our modelling capability as discussed in our research roadmap from 2019.

I will also discuss the cladding degradation processes included in the model, I think there were 12 or 13 of them. Some recent accomplishments and considerations for additional work. Next slide, please.

So, the GDSA model uses PFLOTRAN which as I mentioned earlier is a massively parallel reactive flow and transport model for describing subsurface processes to model repository performance. The GDSA framework includes several processes as shown on this slide here. The model includes inventory of significant radionuclides and allows them to decay using the Bateman equations to estimate the inventory over time as it changes from radionuclide decay and ingrowth. It allows the user to specify which radionuclides are released when the fuel fails. This is instant release fraction which you see on the screen. These are usually radionuclides that are released from the fuel pellet as it is irradiated in the reactor such as iodine, technetium, cesium, and chlorine.

It allows the user to specify different waste form degradation rates and there are four of them shown up there. Instantaneous degradation rate in which all radionuclides are immediately available for dissolution when the waste package fails. This is usually applied to spent fuel not the glass waste form.

There is a more complicated fuel matrix degradation model, which was developed at Argonne. And this considers radiolysis, major chemical boundary conditions, and the surface area of the fuel in calculating the fuel degradation rate. But also, some FMDM, fuel matrix degradation model, surrogate mechanisms that run a little bit faster and also custom user-input degradation rates such as fractional dissolution rate or rate based on the specific surface area. These are usually used for glass wastes.

With respect to waste package degradation rates there is both a canister vitality model that calculates the timing of which a canister breaches and also a canister performance model that models the performance of the canister after it breaches. The second one the canister performance model is not yet implemented in PFLOTRAN. The canister vitality model there is implemented in PFLOTRAN. It is a function of both temperature and the sample canister degradation rate. So, there is a stochastic element to this as well that provides variability in waste package failure times. You can also set a specific waste package failure time; you can say they all fail at 5000 years. Note that cladding degradation is not one of the processes considered because cladding degradation is considered to be failed upon waste package failure. Next slide, please.

So, this slide describes the nexus between DPC criticality consequence, which was my previous presentation, and the cladding degradation. This is because the occurrence of criticality dependence on the configuration of the spent nuclear fuel. As I mentioned earlier, assuming the cladding fails when the waste package fails is not conservative for analysis that involve post-closure criticality. Therefore, we need assess a reasonable time for loss of configuration that would lead to termination of the criticality event. So, we evaluated a variety of cladding degradation mechanisms, which I will present on the next slide, and look at the longevity of the basket hardware that keeps the fuel rods in their positions, namely the grid spacers. It’s important to note here that grid spacers are often made of the material as the cladding, namely Zircaloy.

The result of this evaluation was that the degradation of the grid spacers was identified as the primary mechanism for configuration loss and for permanent termination of post-closure criticality. This is important to know this. We developed a model for failure of Zircaloy grid spacers as a result of general corrosion -- developed a model for failure of the zircaloy grid spacers as a result of general corrosion based on the cladding degradation work. And we are working to implement that model in PFLOTRAN this year because it is complicated to do that. So that's the nexus between cladding degradation mostly because the cladding -- the grid spacers are made of Zircaloy, which is the same as the cladding. As the grid spacers fail, the criticality terminates. That’s the main nexus there. Next slide, please.

So, this is the results of the study by Pat Brady and Brady Hanson. Brady will be giving the rest of this talk after a few more slides. The cladding degradation processes -- on the left is a list of the 13 degradation mechanisms considered. And as these results demonstrate most mechanisms are unlikely or have little to no effect on cladding or on Zircaloy grid spacers in either hypothetical alluvium repository, which is shown in the left two columns or the hypothetical shale repository. For the hypothetical shale repository, the only mechanism of cladding degradation and hence grid spacer degradation that was found to be important was general corrosion.

As you can see by the dot in the upper right-hand corner. Therefore, general corrosion of the grid spacers is the only grid spacer mechanism considered for the hypothetical shale repository. General corrosion is described by two Arrhenius type laws. One for the first phase of the general corrosion, which lasts for a few years, and the other for the second phase of general corrosion which lasts after that time period.

These rates are based on data collected by Hillner et al. over 29 years. The data were not collected in a radiation environment. Other studies have shown that radiation can increase the general corrosion rates by anywhere from a factor of 2 to 20. So, for our model, the general corrosion rates from Hillner are multiplied by a factor of two to account for the effects of radiation.

For the hypothetical unsaturated alluvial repository, which is two columns on the left, localized fluoride enhanced corrosion was identified as an important mechanism and we are still investigating that process. I have nothing to present on that at this time. Next slide please.

This table gives the failure time in years for three different components: cladding, grid spacer walls, and guide tubes assuming a constant temperature of 250 degrees Celsius. The component is assumed to fail when the material has a thickness of zero. So, failure from mechanical loads as material thins are not accounted for. And the reason why 250 degrees Celsius is relevant temperature is that it is pretty close to 264 Celsius, which if you remember from the previous presentation, is the saturation temperature of the hypothetical shale repository at the depth of 500 meters where the hydrostatic head is 5 MPa.

So as discussed in the previous presentation in our conceptual model of a steady state criticality in the hypothetical saturated shale repository the temperature does not exceed 264 C, otherwise criticality would cease because water would boil creating subcritical conditions. So, each of those components has a different thickness as you can see in the column there. The cladding experiences corrosion on only one surface because it is enclosed, while the grid spacer walls and guide tubes experience corrosion on both surfaces. As you can see from this table, at 250 C grid spacers will fail first after 366 years followed by guide tubes and then cladding. Our theory is that criticality will terminate permanently once the grid spacers fail. The corrosion rates are very dependent on temperature as you will see on next slide. Next slide, please.

So, this is a visualization of the general corrosion rate of a 10-mil-fit Zircaloy grid spacer. This has been plotted in terms of time to complete corrosion of grid spacers. The blue line is first stage rate from Hillner, multiplied by two to account for radiation. The orange is second stage rate from Hillner, also multiplied by two to account for radiation. The first stage lasts a few years, so it seems appropriate to use the rate law for the second stage in the grid spacer degradation model.

Note the temperatures associated with hypothetical unsaturated repositories, such as 100 degrees or less, the grid spacers will last more than 1 million years, 10^7 to 10^8 years, which is why the general corrosion of grid spacers is not an important grid spacer degradation mechanism for an unsaturated repository. And you can see at 250 degrees, it is consistent with a number on a previous slide of about 366 years. Next slide, please.

So, for criticality analysis at higher temperatures, degradation rates for the different hardware components are necessary. You can see some of these components in the picture here. On the left-hand side you can see the grid spacers. The green arrows point to those grid spacers. Those are the components we think, if they fail, the fuel will lose its configuration because the fuel will no longer be kept in place. And that middle picture there, you can see a close-up of the grid spacers with the guide tubes. On the right-hand side, you can see the green arrow showing the guide tubes these are typically made of Zircaloy or Inconel and our working theory, as I mentioned, is that relocating the rods, thereby reducing the pitch, can terminate criticality. And with that, I will hand it over to Brady to continue the discussion.

HANSON: All right. So hopefully, you can hear me and see me. So, thank you Laura. As Laura just showed, corrosion of the either cladding itself followed by fuel pellet relocation or of the grid spacers allowing rod relocation could result in termination of the criticality event. And she showed that those corrosion rates are very sensitive to temperature.

Right now, I want to go through a little bit of the history to help us understand better how to develop a test plan for assembly hardware and cladding degradation. And first, is the overall discussion of what is meant by cladding credit either for the safety case or for clad credit or for criticality. So, there are two options.

The first, when you take cladding credit, is to recognize that only a very small fraction of cladding is actually failed, meaning a through wall breach when placed in a repository. These failures occur almost exclusively in-reactor. Additional failures can occur in the repository as a result of external mechanical forces such as rock fall or a severe seismic event or from internal forces largely related to the hoop stress created by the internal pressure of the rod or lastly from corrosion. It is necessary to understand, and then to be realistically bound, both the initial failed fraction as well as the rate of new failures over time due to these other mechanisms.

The second option is to simply assume that all cladding is failed upon on placement, or what we call the no cladding credit case. This case is conservative for dose calculations as the entire fuel surface area is assumed exposed to water as soon it enters a failed waste package. However, as Laura said it could be nonconservative for criticality assessment in that it would presume much faster waste form degradation and relocation than would really occur.

So, to provide some international perspective on cladding credit we look at how other countries are treating the cladding and associated materials. Finland assumes that one thousand years after water breaches the copper waste package or canister, both the canister insert, made of cast iron, seen here on the right, and the cladding itself is breached. This is again conservative for radionuclide release and those calculations. But, if you look the much smaller packages, the thicker walls, and separation between assemblies helps to limit the potential for criticality by limiting the quantity of water that can be in a failed package.

In the photo we see their 12-BWR assembly insert, and you can compare that with the dry storage canisters in the U.S. that we are currently analyzing for direct disposal, and they contain somewhere between 52 and 87 BWR assemblies. Similarly, the Finnish PWR insert contains four assemblies compared to the 21 to 37 assemblies in a U.S. PWR DPC. But as was said earlier by Geoff and others, the most important factor is the DPCs were not designed for disposal.

In Canada, there are no plans currently for taking cladding credit. Of course, they have the CANDU fuel, which is only naturally enriched, so the criticality analyses are much different than for the DPCs.

Finally, Sweden also takes no cladding credit. Their package design is the same as the Finnish one. It is interesting to note a few of the points they made in their analyses. First, they, like other countries, have not done any testing to determine the corrosion rate of zircaloy under repository conditions.

Second, they postulate that cladding lifetimes are on the order of hundreds of thousands of years. And lastly, most cladding failures would not automatically expose all of the fuel to water but would rather serve to limit water ingress and radionuclide egress. So as Laura said that is an important thing to consider moving forward. Even if you do fail, how big is that failure and does it allow pellet relocation?

So now, we will take a brief look at the history of cladding credit in the U.S. program. Prior to the viability assessment, the U.S. program was similar to the international programs in that all cladding was assumed failed or the no cladding credit case. Then as soon as the waste package failed that allowed water to contact the fuel rods and the fuel was able to dissolve or to relocate.

For the viability assessment cladding credit was taken by first replacing that assumption of all cladding has failed, with only one and a quarter percent has failed based upon reactor record and that percentage is skewed high because of the relatively large number of failures on the order of three to five percent that occurred early on in the 1970s. The only cladding failure mechanisms considered under the VA were mechanical from rock fall and then general corrosion. And even then, there was no research on the corrosion rate of the Zircaloy. Rather it was assumed to be 1-3 orders of magnitude smaller than that of alloy 22. The TSPA-VA was subject to a peer review, and they found this approach to cladding credit may be optimistic because it did not consider other forms of degradation. But their key recommendation and finding was additional experimental data with a high pedigree under appropriate environmental conditions needed to be obtained to reduce uncertainties and provide the necessary confidence in the models.

So, next came the site recommendation. Here a more detailed examination of the initial fraction of failed rods was performed and estimated failures following in reactor operations including handling dry storage and transportation were added. Operationally, it was assumed waste packages would be loaded with a mixture of old and young spent fuel so we would have relatively consistent heat loadings and temperature distributions amongst packages. This loading results in the range of failed fuel shown here in the first sub-bullet, and based on the data at the time, it was estimated that there were on average 2.2 failed rods in every assembly that contained failed fuel.

A much more rigorous examination of cladding failure mechanisms was performed. However, the increase in temperature associated with criticality were not considered. Under the FEPs, features, events, and processes analysis, many mechanisms including the ones in the peer review of the viability assessment were analyzed. Mechanical failures remained important and failures from seismic events were also included. Creep failures and those from stress corrosion cracking were also considered. And the mechanism of the potential of localized corrosion of Zircaloy occurring as a result of dissolved fluoride in the groundwater was included as Laura indicated in that previous table.

Still, it was found cladding would remain a significant barrier beyond a hundred thousand years, which is what the Swedish program agreed with. A peer review of the TSPA site recommendation was performed by a team from the IAEA and NEA. And their two main conclusions with respect to cladding credit were that the mechanical and chemical effects of degradation of the basket components had not been accounted for. And there is still this feeling that the model and the approach to taking cladding credit was optimistic and additional efforts, both experimental and modeling, would be needed to reduce uncertainty and strengthen confidence.

Because of those concerns about optimism for cladding performance, and the limited amount of quality data under applicable conditions, and because the overwhelming majority of spent fuel was to be loaded into the TADs, the transportation, aging, and disposal canisters, the license application took a different approach for cladding credit. A log uniform distribution between .01 and 1 percent was assumed failed upon emplacement. And note that the percentage of failed rods has been decreasing as we move from viability assessment, to site recommendation, to license application.

Once a waste package failed and allowed water or air to contact the failed fuel rod, it was modeled that the cladding would split or unzip from end to end instantaneously, given the oxidizing environment and the conservative waste form degradation rates. That is a reasonable assumption for the geologic time scales and the time steps within the TSPA model.

But now, instead of worrying about other failure mechanisms, it was simply assumed as soon as an area or patch of the drip shield and waste package failed, accounting for 20 percent of the area, the cladding under that patch was assumed to fail from static loading of rock fall. This then increased with the uniform distribution up to 50 percent of patch failure, and then after 50 percent, as the patch area increased, the cladding beneath would also fail, and the percent of rod failures increased linearly until you reached 100 percent.

However, as we have pointed out, again, what is conservative for radionuclide release and dose calculations for safety assessment is not necessarily conservative for a criticality assessment. In this case if the cladding and fuel are modelled to degrade or relocate more rapidly than what would really happen in reality, that incorrectly reduces the likelihood of a criticality event.

So, in order to better understand cladding and assembly hardware performance with either the safety case or criticality assessments we need to understand what has changed in the past two decades since most of that work was done. First, the development and deployment of more corrosion resistant alloys, starting the early 2000’s. These new alloys such as M5, ZIRLO, and optimized ZIRLO, were designed to reduce the general corrosion or oxidation of the cladding during reactor operations. This is done through a combination of adjusting the alloy chemistry to include about 1 percent niobium as well as changes in the cladding texture or grain orientation. For reference, corrosion of Zircalloy 4 would basically be along the upper edges of this ZIRLO data and would be significantly higher than the M5 data points. One of the current limitations to burnup is that oxide thickness, which corresponds to thinning of the metal cladding, is limited to 100 microns.

In the push to provide more margin and go to even higher burnups, industry is now developing and deploying accident tolerant cladding, designed to further reduce oxidation and production of hydrogen as was the issue at Fukushima. Some of these design changes include a thin layer of chromium on the outer diameter of the cladding to resist oxidation and corrosion. In combination with these alloy developments has been a concerted effort by the nuclear industry to further reduce fuel failures.

So, we see here on the figure on the left, the number of fuel failures has been trending downward since 1980. In the early 2000's the new cladding alloys were introduced and began to replace Zircaloy 4 in PWRs. In 2006, the Institute of Nuclear Power Operations set a very ambitious goal of zero failures by 2010. Together with utilities and vendors, a number of operational changes were implemented, such as increased surveillance and inspections, preventing reloading identified failed fuel back into a core, and better understanding the cause of failures and how to prevent them. We see that those efforts, while they have not yet achieved that goal of zero failures, combined with the new alloys has led to decreasing failures in the U.S. in both PWRs and BWRs. This upper graph shows the failure rate per million rods in PWRs with the U.S. in green, and the lower graphs shows the failure rates in BWRs with the U.S. in red.

It is important to note that between 2010 and 2015, the PWR failure rate in the U.S. dropped by more than an order of magnitude. It is worth pointing out that the utilities also have excellent procedures to determine which assemblies contain failed fuel rods. And while it is not regulation, it is not guidance, it is common practice to load the dry storage canisters with intact fuel first and save the failed fuel towards the end often after the reactor has shut down. Thus, the overwhelming majority of canisters have no failures in them. Assemblies that do have failures, if the defect is known or suspected of being larger than 1 millimeter in any dimension, are placed in what is called a damaged fuel can. The number and location of damage fuel cans within a canister are limited as defined by the certificate of compliance for that system. So you won't have a canister full of nothing but failed fuel. And even for assemblies that do have failed fuel, that average number of failed rods per failed assembly has decreased from the

2.2 that we had in the TSPA site recommendation to between 1 and 1.5, as recently reported by IAEA.

Now we want to shift gears a little and take a look at some of the lessons we have learned from the storage and transportation work over the last decade within the spent fuel waste and science and technology program.

We have previously reported on the high burnup spent fuel data project and the demonstration cask that was loaded in November 2017 at the North Anna nuclear generating station. Thermocouples inside that cask measured temperature during drying and they continue to monitor temperature as that cask sits on the pad. Those measurements, together with modelling performed by the program and international round robin sponsored by the EPRI Extended Storage Collaboration Program, showed that cladding temperatures were much lower than those predicted using the conservative methodologies employed by the vendors and utilities. The peak cladding temperature in the demo cask was only 237 degrees C well below the 348 degrees C modelled using standard conservative methodology. More importantly, the realistic rather than conservative models showed only a fraction of percent of the cladding was at this peak temperature, barely visible in this figure on the left.

When you account for the radial and axial temperature variations in a cask or a canister you get the broad temperature distribution shown here. A similar modeling effort was undertaken for a high heat load vertical canister system. Again, the peak temperature was well below that predicted using the conservative methodology used by vendors and utilities. And again, the radial and axial variations show that most of the cladding is significantly below the peak temperature. The applications and implications to a repository environment is that we can assume a conservative temperature for a safety case but if we assume a high bounding temperature or assume that all cladding and assembly hardware in a canister is at this bounding temperature, we will predict degradation or relocation faster than will occur in reality. And this would give a false conclusion that criticality would be terminated permanently based on this relocation sooner than it really would occur.

The next thing we learn from the storage and transportation program, and specifically from the sibling pin testing, is that modern fuels have much lower end of life rod internal pressure than had been assumed for many years. These red squares and circles are data from rod puncture of the 25 sibling pins performed at Oak Ridge and Pacific Northwest National Laboratories.

We see that the pressure is less than or equal to 4 megapascal for all rods except a few of much older Zircaloy 4 rods, which had higher initial helium fill pressure. Even when we consider this maximum of around 5 megapascals, and then we conservatively assume a uniform 400 degrees C temperature of the rod, the hoop stress is well below 90 megapascals. And 90 megapascal is almost universally acknowledged a threshold below which creep, stress corrosion cracking, and especially hydride reorientation is limited enough not to effect material properties and cladding performance.

At the typical temperatures expected in a repository this hoop stress would be substantially lower. And as Laura said, for a postulated post-closure criticality in a shale environment with a peak temperature of 264 degrees C, even under a criticality, this hoop stress will be very low and so these mechanisms associated with internal pressure and hoop stress aren't considered important.

And finally, the storage and transportation program has examined the effects of external loads on cladding. In previous Board meetings we have highlighted the multimodal transportation test that was performed in 2017. The test included handling, truck, ship, and rail testing. The maximum shock event from coupling two rail cars at 8 miles per hour had the kinetic energy equivalent of one rain drop hitting a rod. Similarly, the total accumulated damage from vibrations in a 2,000-mile rail trip was less than 10 to the minus 10th. So, that means it would take over 10 billion trips of 2 thousand miles before fatigue failure would occur. The application to disposal is that failures during normal conditions of transportation are considered nil and we also believe we can take these models and extend them to look at mechanical loading and seismic events in a repository.

Just briefly on this slide I show some reports and references to show there has been significant efforts by industry and the NRC over the past few years examining some of these same issues that I just described. It is recognized that the way decay heat is calculated is way too conservative. That thermal modeling practices also result in artificially high temperatures and more importantly, again, because of these lower temperatures and hoop stress very few degradation mechanisms are of concern during dry storage, including extended dry storage, and normal conditions of transport.

So where does that leave us? First it is important to understand as Laura pointed out that those equations from Hillner was work performed by Bettis Atomic Power Laboratory on Zircaloy 2 and Zircaloy 4 not on any of the newer cladding alloys. Bettis performed 22 static autoclave tests, with each test containing between 3 and 71 unirradiated specimens or coupons, of either Zirc-2 or Zirc-4. The tests were conducted at five temperatures between 271 and 360 degrees C, with almost half the tests performed at the highest temperature.

Oxidation was determined based on weight gain of the samples, and the maximum weight gain for any specimen corresponded to corrosion of about 0.11 millimeters or halfway through the thickness of grid spacer wall or 20 percent of the wall of a typical 17 by 17 PWR fuel rod. And this was exposure for 17 and a half years at 338 degrees C. The maximum exposure, as Laura said, was 29 years for two of the tests conducted at 316 degrees C and that resulted in only 6 to 7 percent of the cladding wall thickness corroding.

There was only one test run at 271 degrees C, which again is just slightly above that estimated peak temperature of criticality in a shale environment. It was performed only on four different coupons of Zircaloy 2 and had a weight gain corresponding to a corrosion of about .5 percent of the cladding wall thickness, so about 1.2 percent of the grid spacer wall thickness over 8 years. And Hillner et al. noted in that test, they were still in the stage one corrosion regime. Starting this fiscal year, we have been putting together the high-level ideas for a test plan needed today either take cladding credit for the safety case or to determine if grid spacer degradation will result in termination of a criticality event. Work by Bettis will need to be expanded to include the newer alloys, as shown here, especially those used to manufacture grid spacers. The alternative is we have to assume that the assemblies made with these alloys remain critical until conditions other than rod or pellet relocation result in termination.

Second, we see from the Bettis tests that long term tests need to be performed over a range of temperatures but especially in the range of interest, which for us is 150 to 275 degrees C. So that we can verify this Arrhenius temperature dependance and accelerating the test may not be possible. You can't just increase the temperature and expect to have the same result. Or artificially increase it. So, what we are looking at is the possibility of growing the initial or stage one oxide layers at a higher temperature and then decreasing the temperature so we can determine the stage two corrosion rate in a much shorter time period. It is also unknown if the rate increases, decreases or remains the same after corrosion beyond the 20 percent maximum that was observed in the Bettis test. That is, is there a stage three rate?

And third, as shown as Laura said this report by IAEA based on limited data says that the corrosion rate is faster by a factor of 2 to 20 for irradiated materials in a reactor than for similar materials tested under similar conditions even including a radiation field when this is outside of the reactor. And that is why, as Laura said, the rate was multiplied by a factor of two.

A recent book published by Olander and Motta, lists some of the possible factors why in-reactor corrosion is faster than ex-reactor under similar conditions. Some of these include the internal heat flux from within the rod as opposed to an externally applied temperature like in an autoclave test. The effect of the hydride rim on the cladding outer diameter might have some sort of effect on the corrosion mechanism. Radiation damage to metal and oxide layers and the effects of water radiolysis creating radicals and highly oxidizing species.

It will be necessary for a test matrix to consider these effects and determine the difference between the un-irradiated and irradiated conditions. One other factor is that lab tests including the Bettis test are traditionally performed statically as compared to the high flow rates in a reactor, and if the high flow rates increase the corrosion rate, then a static test would be much more applicable to what is expected in a repository, even during criticality events.

So lastly, as noted by the peer review of the TSPA site recommendation, as well as one of the questions posed by the Board, it is also important to understand the effects of water chemistry and how it changes over time and possibly effect the corrosion rates of the cladding and the assembly hardware. These changes could come from degradation of the canister, the basket, the spent fuel, in addition to contributions from other engineered or natural barriers.

The figure here shows the basket for one of the canister systems made by NAC international. And if you picture this lying horizontally in a repository, then as the basket walls corrode and weaken, they are stainless steel aluminum so they will degrade relatively quickly, the upper assemblies could collapse down on the lower assemblies and the question becomes if the mechanical impact and load could accelerate the relocation of fuel rods, so the geometry is no longer supportive of a criticality. And again, as Laura pointed out generic studies on a repository environment in alluvium has identified that fluoride enhanced corrosion may be possible. So, the test matrix needs to examine the condition described here as well.

Very quickly to summarize, it is fairly well accepted that cladding and assembly hardware, made of similar materials, will have lifetimes on the orders of hundreds of thousands of years in a repository under normal conditions namely temperature around 100 degrees C or less. However, there is minimal experimental data under relevant conditions to provide the confidence needed. Cladding failure rates in the reactor are decreasing and are expected to continue do so as the industry moves to higher burnups because of the implementation of these new alloys, accident tolerant fuel design and other improved designs and practices.

Testing today by the SFWST program shows that cladding integrity won't be challenged in storage, wet or dry, short term or long term, or under normal conditions of transportation. We are starting this year to develop this framework for a testing and modeling program focused mainly on the effects of potential criticality, specifically the higher temperature between 150 and 275 degrees C, as well as the potential concentration of elements, such as fluoride, to determine if cladding and assembly hardware corrosion may result in relocation that would permanently terminate a criticality. And as you have seen from the data, a testing program would need to be very long term and look at the differences between in-reactor versus ex-reactor.

Lastly a slide where we have the references for all the data mentioned in this report. With that, are there any questions?

BAHR: Thank you Brady and thank you Laura. Just Brady you talked about the newer cladding compositions. What percentage of the inventory of spent fuel that we have -- has those newer compositions versus much older fuel?

BRADY: I don't have the exact numbers but in PWRs the M5 and the ZIRLO, took the place of Zirc 4 almost exclusively very early on in the 2000s. Everything since then in PWRs is newer.

The BWRs still basically use the Zircalloy 2 although that is changing as well. And I do want to point out even separate from accident tolerant fuels there are other alloys currently being developed and other designs. It is constantly changing and improving. But I can look in some of the reports and get that number to you of what the percentage is.

BAHR: I guess the bigger question is what is the implication of the fact that -- we can't make all of these analyses based only on the newer fuel types because we do have a lot of fuel that is older that will go into a repository, might go into a repository first because it would be older and colder.

BRADY: Correct and I will echo what was said earlier about the use of the UNF-ST&DARDS database. So that looks at each canister that is loaded and looks at the inventory of it. We will actually know for each canister what material the assemblies are made out of and be able to judge accordingly.

BAHR: Okay. Thank you. Do we have questions from Board members? I see Lee Peddicord's hand up first.

PEDDICORD: Yeah, Brady and Laura thank you very much. Very interesting stuff. The takeaway seems to be at the end of life we have a hard time to make the stuff critical to reactor when you have all spent fuel it is even tougher. The question kind of building on your last point and what Dr. Bahr was discussing. So, everything going forward appears to make the issue less and less likely is the takeaway. As we pull the string and go backward in time.

Do you find there is increased -- I don't want to say vulnerability but a possibility of criticality or these effects as you get back into the very early fuel designs? I am thinking about PWRs when these were 14 by 14 arrays, larger diameter fuel pins for the boiling water reactors 7x7. Is criticality analysis sensitive to any of those parameters or does it not make much difference and mostly depend on these materials used?

BRADY: So, I was not directly involved in that analysis. Laura, do you have that.

PRICE: I was not directly involved either. I couldn't give you the answer to that. I am sure it could be Oak Ridge might be able to tell you. People who did the calculations on those DPCs containing those fuels could probably tell you the answer to that, but I don't know.

PEDDICORD: Where I was going with this maybe there is a small subset that might need a little more attention. We heard about these possible counter measures and injectable foam and stuff like that. So, maybe it would be really just the DPCs that would have these things early in use of nuclear energy that need the most attention. But that's speculation on my part.

PRICE: Right, that is one advantage I suppose. One thing that could be gleaned from using the UNF-ST&DARDS database. They have the evaluated reactivity of several hundred canisters. So, they can probably identify the ones that are more likely to go critical post-closure than others.

PEDDICORD: Okay. Thank you.

BAHR: Are there questions from Board members? If not, we will go to Bret Leslie of the staff.

LESLIE: Thanks Jean, and thanks Brady and Laura. Interesting presentation made me think quite a bit. So, you are talking potentially about some experiments, but it is mainly on the cladding materials and as I understood the five-year disposal R&D plan originally, the experimental plan was more looking at the fuel itself rather than the cladding. Are these two separate things or are they the same? Or what's one of the -- we had asked to hear a little bit about the spent fuel experimental path forward, but we haven't really heard anything. Is the cladding in these experiments a path forward?

BRADY: I think it is both. So, under the program as Laura said originally it was Pat Brady and I working on this. Under the disposal research side there is currently a consortium of the national labs working on a test plan for the fuel itself. I see, Dave, you probably have a better answer than I do.

DAVE: Well Brady, you certainly hit on the answer. And, Bret, there are two thrusts that are going on. Brady is part of both of those. Brady is one of our personal integration resources because we have him integrating from storage and transportation, R&D across into disposal research and anything that has to do with fuel, Brady is likely going to be involved with because of his background and expertise.

So, the cladding modelling development was the higher priority for cladding because we didn't have anything in place. Putting that in place and figuring out what are the gaps and where to go experimentally is proceeding, along in a slightly offset but integrated fashion, with the spent fuel degradation work which will look primarily at what is going on with the fuel pellet. But those two will be integrated at some point.

BAHR: I see Paul Turinsky's hand up. Maybe we can go to him.

TURINSKY: I am struggling a little with this Laura and Brady. There is two ways to terminate the criticality event, I guess. One is the grids collapse, and the second is the cladding itself opens up. And both of them would result in termination of it.

I didn't hear anything about grid materials which have different irradiation history than the rods themselves. Their oxidation is quite different. They get very brittle, which the NRC is now concerned about from blow downloads on them. Are those -- are the grids themselves going to be considered? What about Inconel?

What about the new fuel assemblies? They have Inconel grids at the top and bottom still, and all the in between grids and the ones for D&B grids the shorter grids, they are all Zirc. The top and bottom grids are still, I think, Inconel for structural reasons. They are in low neutron flux regions so they can do that. So how does this all come together?

And BWRs basically grids can go away, the rods are supported on the top and bottom by what I’ll call upper and lower nozzles, the BWR people call them something else in that. The rods will clearly sag at that, but they are, you know, grids are gone. But what you will have that sagging rods at that point. Not complete collapse. Lots of thoughts there and how they are going to be addressed?

BRADY: You are absolutely correct. It is a very broad range of materials that would need to be looked at. I think as Laura said, what we want to do to narrow the scope of this is to look at the models to say which packages are assumed and modeled to have the biggest likelihood of criticality and start with the focus on those materials to see if degradation will result in termination. Yeah, there is a lot more work to do than what was done by Bettis back in the 80s and 90s.

TURINSKY: Are you going to look at the grid materials? Does Inconel corrode so much slower that it is not an issue? You know what it is going to be there? Or don’t we even know that?

BRADY: We have actually started procuring unirradiated grid spacer materials. We want to get our hands on radiated ones. That's much difficult than getting our hands on rods because utilities don't really like taking their assemblies apart and we also don't want to necessarily have to take ownership of a full assembly just to get hands on grid spacers.

So, we do have some potential. For example, on an INERI Project we are working with South Koreans who have done a fair amount of work on the grid spacers. One of the biggest issues is, even more so than the cladding, those grid spacers are proprietary information to fuel vendors, and we are still trying to get nondisclosure agreements and things like that in place.

TURINSKY: When they collapse, if it is a BWR, there are water tubes and water crosses that are still in there full of water. Thank you.

BAHR: I see Dan Ogg's hand up.

OGG: Thank you Jean. And thank you Brady and Laura for the presentation. Brady, you presented a lot of data, historical data from years ago, as well a lot of the information coming from the HDRP project, and I assume that all of that data can be used to inform future modelling scenario or effort. Do you have models now in mind to use or you are already using or are you considering a number of different possible models and if so, what are those?

BRADY: Right now, the main focus is on the oxidation equations that Laura showed that Hillner showed. Again, the biggest issue with them is their lowest temperature was higher than what our highest temperature is. Trying to obtain data in the lower temperature range to make sure that Arrhenius activation energy is correct is one of the initial thoughts of what we need to look at. The lower you go in temperature the much longer you need to test in order to even see anything. That's some of what we are working through and struggling with.

OGG: All right. And will those like the Arrhenius equations will they be able to capture other characteristics of the cladding for example the hydride reorientation type effects or other effects that may be present?

BRADY: I think we could if we have a large enough test matrix. Part of what we need to go through is, again, how much needs to be done. As Laura showed consequence appears to be fairly small as she said. And we do have to look at effects on other engineered and natural barriers. And depending on those consequences that will dictate kind of how far do we need to go with this type of testing?

OGG: Okay. Thank you.

BAHR: I see Bret Leslie's hand up again.

LESLIE: Thanks. This is probably a follow-on question. But I am trying to get a better handle on the things that you have been describing and discussing recently in this presentation. Were these the same types of things the independent review team heard or is this beyond the scope of what the independent review team heard. Again, I am trying to understand the charge for that group and the scope of that group and kind of where that might be leading.

PRICE: I would say I don't think the independent technical review team heard any of the information you just heard about the cladding degradation work. With respect to the post-closure criticality, the presentation I gave earlier, they heard some of that, but they didn't see the results that I presented because those came out after they did the review. They did the review last summer and these came out in September of last year. They saw some of the earlier work we had done and could see the direction we were headed in. They also looked at our fillers work that is not being presented today and some of the other work we are doing that which Geoff alluded to which is not being presented today. Did that answer your question?

LESLIE: Thank you for the clarification, that's quite helpful in fact.

BAHR: Okay. Other questions from Board members or staff? Okay. Well, seeing none, we are pretty much on time here. So, why don't we get started -- move on towards the next speaker who is going to be Ned Larson who will give us a summary and status of storage activity. I see Ned, do you have slides? I see Ned, but I don't hear him.

LARSEN: Yes, do you see the slides?

BAHR: I see them coming up now. I see you have started screen sharing.

LARSEN: Very good. I will talk about the progress and summarize what we have been doing and working on. There we go. The standard disclaimer. I will save you some pain by not reading it out loud, but you have seen it once you will see it again. This material I am going to cover fall under the same items that the disclaimer covers.

What I want to make clear is when we do our research the NRC is responsible for dealing the cask and cask systems and issuing certificates of compliance on how they work and how they do. We believe NRC is doing a great job. We have no reason to question any of the casks or cask systems that have been approved. We are not testing on the casks themselves, but we are most concerned with what happens inside the casks and what happens to the fuel and hardware and things of that nature.

As a general statement on our priorities, the things inside the cask are our highest priorities. Our fuel is our highest priority, followed by cladding, hardware, canister and the rest of the cask. And those are the things we generally have our priorities on.

We have documented these priorities through our gap analysis. We updated it in 2019. It continues to be our latest one. We will probably be updating it again this year or later on in early next year, we will see. It continues to document everything we are doing and the priorities we have.

As we do our work, we continue to engage a number of different entities the industry and national labs, we still have a number of them, small businesses of course.

As far as universities the nuclear energy university program (NEUP), we have made 40 awards in storage and transportation ourselves totaling $40 million since the beginning of the program. And of course, for the international world, we still deal with ESCP and find them very valuable to us.

As far as the high burnup demo Brady talked about that a little bit. That continues to give us data, we continue to get the reports. I think he covered the materials there.

As far as sibling rod testing, we are still doing that. The labs are open again and we are back into our hot cells. It is good we are making progress there again. PNNL is doing all the work for our defueled cladding that is going on, and Oak Ridge is doing all the fueled cladding.

So far as we do our testing, we have learned nothing or seen anything that surprised us. I am not going to go through all of the data, graphs, and everything. But everything we have seen so far is within data that has been published already, or what the expectations were. From what we have seen, we believe cladding is sufficiently robust to deal with any problem that would occur in the future for storing or transporting from a cladding perspective.

We are also looking at, started to look at, seismicity and applied stresses there. We know that there is a lot of variabilities under seismicity issues. The geology, and the level of the location of bedrock, the amount of soil, the types of soil that exist, not only that but where the cask would exist on the storage pad, and that affects it also. So, as we do this, we are looking forward, we are not trying to evaluate the cask performance but trying to evaluate the fuel inside the casks. As we pull this together, we have been working with our modeling, PNNL has been doing some very good model working work with Sandia, that are our experimentalists and our modelers. As they model the numerical models, it has modified our test plan to some degree to make sure we get the most out of our shaker table tests as we do those. It has been hard to get time on the shaker table. They are in big demand. So, we want to get it right and we are taking our time there.

And so, we have been doing our numerical modelling there and we believe that we will be ready for it when it comes. We believe that the robustness of the cladding there will not be an issue in seismicity if we do have a seismicity event. We don't know exactly how much we will have, and we don’t know exactly what the stresses will be. We modelled them, but we need to measure them. With this, we will go ahead and pursue this, and make sure that we understand, and are able to measure the stresses and strains inside a loaded cask or similar type object.

Drop test. We did our drop test. We completed that work for the most part. What it found is that as we drop it of course, the stresses and strains that we experienced from a drop test are higher than what we saw in the normal shock and vibration from just a regular transportation test that we did. But as we do this even though they are higher than multimodal modelling and testing shows they are higher. The stresses and strains are higher. We still don't believe there is going to be a gross structural rupture of the fuel if the fuel is dropped 30 centimeters. We believe the fuel rods and assemblies, for the most part, will maintain their integrity. Still more work needs to be done in this area. What we have seen so far is encouraging, but more work needs to be done.

As far as aerosol dispersion as we do our work at Oak Ridge, every time they test their fuel rods, and we take the failure -- we take the rods to failure, and they break. Oak Ridge is able to trap and measure and maintain the dust and particles that come out of that fracture, and with that we have been able to understand what we should expect should a rod break inside the cask from a structural perspective.

Working with Albuquerque and Sam Durbin and Sam will talk about this more tomorrow, we are able to determine what the aerosols would look like possibly if we were to have a crack in the canister what it would look like and how much materials would move through there.

This is still very early in our testing. But so far, if a rod were to break, if we were to see a crack in the canister, we do not believe there would be much of a release from that event. More testing needs to be done, and we are continuing to do work in this area.

As far as transportation, I covered this in the past, but I just wanted to make sure when we did this. This is important to us to understand the stresses that we would experience as we ship rail, boat, and truck. Because when we compare it to our testing, we need to know how the two compare. And as we have continued our testing for our sister rods, the data is there you can see where the stresses we would experience from transportation would appear. And it is -- it just orders of magnitude lower than what we would expect. We just don't expect there to be problems from transporting any of the materials we have based on the strength of the rod and the rod behavior. The data that we have just continues to show us the integrity it will have, and we don't believe it will be challenged. We are moving from phase one testing to our phase two testing for our sibling rods to make sure we further understand the sister rod's behavior.

As far as stress corrosion cracking, this continues to be an important one for us. We have started again back in our laboratories. We finally bought all of the materials and testing equipment that we need. We have four of these -- four pieces of the test equipment at Albuquerque and Sandia. We have four at PNNL. This is a concern we have, and because it involves time, as we do our testing, some of the testing takes a fair amount of time. We ramped up on our lab equipment and our labs are working on it now and that work is continuing to do well. Charles and Rebecca will be talking about that more in detail. But so far we have been pleased to see what we have found.

We are not only working to understand corrosion, stress corrosion cracking, but we are also looking at other materials such as coatings and other materials to even prevent even the possibility of stress corrosion cracking. And working with universities through the NEUP programs and other activities, a number of tests and activities that have been proposed by the universities to continue to work closely with our national labs and continue to make progress in that area.

Also, we believe that there are some materials that show real promise as far as preventing and remediating stress corrosion cracking. But again, more work needs to be done in this area and more of this data will be shared with you.

As far as thermal and drying activities these continue to be important to us. Remember we took our one sample from North Anna when we were drying to see if the cask really is as dry as we hoped it would be. That didn't work as well. So, at this point, we set up kind of a test program to see. We have started some laboratory bench scale testing type activities, along with modelling, so that we could at least get some data in the laboratory. We are doing some modelling to make sure we understand how it is working and what is going on.

We also are doing work to make sure we understand -- to see if we can see inside the cask once it is sealed to see if we can determine and see if we can see water in there using sensors and remote sensing. We are doing work in this area to try to understand that also and take advantage of that, if it is at all possible.

We don't believe there is a lot of water in the cans. However, small quantities not expected may result. We don't know. But the third bullet is the most important. Assumptions about potential quantities of residual water have not yet been corroborated with the field experience. We just don’t have data on that, so we are doing the testing on that to understand the thermal and the drying activities and how it behaves. Again, much more work needs to be done in this area also.

Canisters, we became the owners of 15 canisters this last year. Six of them -- we delivered a bunch to Sandia, to PNNL, and ORNL, and EPRI. They are putting together test plans for some of these to put them the best possible use we can. Thermal and drying, corrosion cracking, seismic, cold spray, and all that stuff. So many of the test plans are still being put together to use these cans.

We do have one on the field canister deposition demonstration. This one is where we will take a can, we are putting -- and this will be dealt with in more detail where we are putting a can, putting heaters in to understand how much salt can be collected on top of our cans, in the vicinity of our cans, when they are hot and cooling. At what point, do we start collecting the salts on our cans. This is going on now. The test plans are in place. Our can is being instrumented right now. And we continue to make progress on this area. It is believed some salts will collect, we believe that will be the case, but the full-scale testing must be done to know exactly how much we would be experiencing. We just don't know until we get the data from this test.

And then the other thing we are doing is we continue to work on accident tolerant fuel. Fuels from the advanced reactors. Again, we don't expect any high burnup fuel from the events -- from the accident tolerant fuels until 2025. The advanced reactors we are talking possibly even later than that we could possibly get our hands on them and put them in our test cells and starting testing on them. As we continue to move progress there. We coordinate with those programs and activities, and we hope to get that data soon. A lot of the data is -- a lot of the tested fuel is moving out of the Oak Ridge hot cell already. I just got notification that we have already filled some of our cans that have to be sent offsite. We believe our test cells or hot cells will be open and ready to do this other work by if time we need it. With that being the case, I covered a lot of materials fairly quickly. Any questions, I guess?

BAHR: We have plenty of time for questions. And I see Dan Ogg's hand up and Steve Becker. Let's start with Steve since he is a Board member, and then we will go to Dan.

BECKER: Ned, thank you for a very wide ranging and interesting presentation. As I understood it, most of your focus was on what could be characterized as more or less normal conditions or situations or at least situations that are not extreme.

LARSON: Correct.

BECKER: We live in very interesting times. I think we will all agree. Today, with the world's situation being what it is. I have a sense many members of the public are asking more questions and have more questions about scenarios that are, for lack of a better term, are unusual or abnormal. I am wondering if you could share with us what light the work you are doing sheds on more unusual or more extreme situations.

LARSON: No. That's a good question. Right now, you are correct. We have focused on normal conditions of transport. What would happen under normal situation. And that's because, you know, 99 percent of our fuel will be shipped under those conditions. We have had discussions with the Nuclear Regulatory Commission about what we would do if we did have an accident scenario, let's say. What we would do and how we would handle it. Of course, those events would be handled differently. Those may have to go back into pools and those may have to do other possibilities for handling. But we anticipate getting to that in the future. We still believe that we need to do some of those activities. What you are saying we believe is correct. We still want to start gathering data as we finish the normal conditions of transport to move to the next phase, which would be an accident scenario or something like that.

BECKER: It certainly makes sense to begin with the normal situations. Are you saying that in the future, you anticipate carrying out research related to more extreme situations such as accidents or intentional threats?

LARSON: Yes. I mean, we believe that. I am not talking about drop tests or talking about exploding cans or stuff like that. Our issue is still the fuel inside. So, coming up with events so that we understand the fuel will behave and the cladding and the hardware.

BECKER: So, you do anticipate getting into that in the future?

LARSON: Yes, yes.

BECKER: Thank you.

BAHR: So, as a follow up to that. Ned, you mentioned the seismic testing and what sorts of magnitude earthquakes are you, what range of magnitudes are you going to be looking at in those tests?

LARSON: As I recall, it has been a while since I read the test plans. But we will do -- there are several catalogs we will use typical recorded events and then we have the capability of increasing them. We can invent our own if we want to. I don't recall which catalog they have picked for the shaker table, but I can find that out and get back to you.

BAHR: Be interested in that, thanks. We have another Board member, Paul Turinsky, with his hand up.

TURINSKY: Ned, thank you, as always. For these canisters you have received in the salt test you are going to do on that, are you going to build an overpack to make it really look like a canister sitting out on a pad to get all of the natural…?

LARSON: Yes. No, that's a good point. It will go into a horizontal storage module. These are the NUHOMS that Orano has produced or Transnuclear in the past. We have the horizontal storage a place where we can use the horizontal storage module. And we will be doing them in that so we can just make sure we understand how the convection currents will go in and how they will behave under that circumstance.

TURINSKY: I assume you are doing this while you are still collecting samples from utility sites because you going to instrument this to -- well, it will have instruments versus what you find out a utility site.

LARSON: We have gone to the utility sites, and we have had a lot of success taking samples on the top of the existing loaded canisters. But we have had some real difficulties, the radiation field is so high. We do it with robots typically. We have had good luck with it and some bad luck with it. But we don't believe that the data has been as consistent as we would like if you are with me.

By doing it in a clean environment with heaters, we can pull it out and look at it, if we need to. We can do a lot more controlled sampling of the top of the can than we could under loaded conditions.

TURINSKY: And are you going to have things like temperature sensors, humidity sensors?

LARSON: Oh, yeah. There is wires all over it. We invite you to come out to Sandia and look at it. We are still wiring everything out. But you are right we will have weather stations in place. And we will have all of that stuff, so we know and understand all the boundary conditions as we start doing our modelling. Because that will be important to make sure the two coordinate with each other.

TURINSKY: And any concept based on the work that you have done out at the utility sites on how long before you get some useful data?

LARSON: You know, it never goes as fast as I hope. But, right now, I don't have an estimate for you Paul.

TURINSKY: Okay. All right. Thank you

BAHR: Do you have an idea of how long -- not how long it would take to get initial results but how long you expect to run these tests?

LARSON: Well, it will be -- once we get it in place, once we get our instrumented can in place we will run it, you know, a couple years, a few years just to make sure. And as we get the data, we may shorten it or lengthen it depending on what we know and understand as we start executing the tests.

BAHR: Do you have a budget in place to support doing it for a fairly long time? Maybe even a decade if need be?

LARSON: Ummm. Yes, and no. We believe we will. But, again, you know, Congress is the one who writes the check. We believe Congress has supported these activities in the past and they have done well in my opinion about putting the money in the right areas so we can do this work. But I can't tell you what Congress will do in 10 years.

BAHR: Okay. Thanks. Dan Ogg has had his hand up for a while. Thanks Dan, for your patience.

OGG: Thanks Jean and thanks Ned for an update on all the research, we always appreciate getting those updates. Following up on the comment about utility inspections of the canisters, of course they continue to do a number of those inspections as part of their aging management programs with the NRC and they have a database that they load all the results of those inspections into. And I just -- you probably told me this before, but I wanted to verify do you or Brady or someone from your group have access to those reports that are loaded into that AMID database?

LARSON: It is my understanding they do, and we do try to have somebody there when they do the sampling so we can understand what is happening and what is going on. It just helps us also as we consider that very valuable data.

OGG: Very good information and I know utilities are continuing to do those inspections and gather that data.

LARSON: Yeah, we are getting better and better at it. I will just say that.

OGG: Another question. Different question. Going back to your discussion of the sibling pin test or sister rod tests. You have told us in the past you hope to get your hands on some BWR high burnup fuel in order to do examinations. Can you give us a quick update on that effort?

LARSON: There was an opportunity where we thought we could, and that opportunity did not pan out. I will just say it. But we are still hopeful as we move forward. We are still always looking out for that opportunity. But right now, there is nothing on the books for that. We are still hopeful though.

OGG: Okay. Thank you.

BAHR: Thanks, Dan. I see Bret Leslie’s hand up.

LESLIE: Yes, Ned, thanks for a good overview. I have a question that goes to what's the relationship between the gap reports and the 5-year R&D plan. Let me give you a little bit of a background. I found that the five-year R&D plan was extremely helpful for putting out a narrative of your vision, what you are doing, and how things are related. So, I was curious why you talked only about the gaps but not about the R& D plan and maybe you can enlighten us a little bit.

LARSON: Sure. On the gap analysis, it talks about the technical aspects, what we want to do technically and the data that we want to gather technically. The trouble with the five-year plan is it is based on the funding level that we get. And we looked at that, and our ability to predict our funding level far into the future, we thought was limited. And if we didn't get what we had anticipated or hoped or wanted, then we would have to redo our five-year plan again. Even though the technical aspects weren't changing, the time and money were stretching out or being shortened whichever the case. So, we felt like we wanted to separate the technical issues and have that in a separate book, and a separate plan so to speak. And so, we still look at the gap analysis as the technical guidance as opposed to the five-year plan.

LESLIE: Okay. Thanks.

BAHR: Okay. Let's see. Are there any other questions from Board members or staff at this point? Okay hearing none we are running a little ahead of schedule and so just we don't get too far ahead in case there are people that are wanting to watch the live stream and have been paying attention to the agenda. I would suggest we can take about a 15-minute break right now until 3:50 p.m. eastern time. And at that point we will come back with Charles Bryan and Rebecca Schaller from Sandia National Labs who will be talking about canister corrosion. I am going to go off-line here, and we will be back at 3:50 p.m. Thank you.

[Break until 3:50 p.m. EST]

BAHR: Okay. This is our final presentation for this afternoon, and it follows on Ned's summary of storage and transportation activities, and we heard a little bit about the importance of corrosion testing and surface environments in that overview. Now we are going to have a more detailed presentation from Charles Bryan and Rebecca Schaller, and I will let you take it away.

BRYAN: Thanks a lot. My name is Charles Bryan. I am a geochemist at Sandia National Labs. And I am here with Rebecca Schaller, my colleague who is a corrosion scientist. And we are going to tell you about the work we are doing on stress corrosion cracking of spent nuclear fuel canisters at Sandia. Next slide.

Okay. So just a little background here. You know the United States has over 86 thousand tons of spent nuclear fuel right now, of which, 50 thousand tons has already been moved to dry storage systems. The dry storage systems are intended to be interim storage. However, since we don't have a repository pathway right now, the waste may stay in interim storage for quite some time, potentially 100 years or more.

In most systems the spent nuclear fuel is stored in stainless steel canisters and these canisters are in overpacks which are passively ventilated for cooling. And they accumulate dust over time, which could deliquesce as the canisters cool, to potentially lead to stress corrosion cracking.

Understanding stress corrosion cracking was determined to be a high priority data gap in early data gap analyses, dating back to 2011 and 2012. And what we are evaluating then is the potential timing and conditions of occurrence and the risk of canister penetration. Next slide.

So, the canisters look like this. They are either stored in vertical or horizontal overpacks. And in both cases, the passive ventilation means that air is brought in through the bottom, it passes over and around the canisters and deposits dust and salt, and then exits through the top. Again, over time as the canisters cool those salts can deliquesce to cause corrosion. Next slide.

Now, there are three criteria for stress corrosion cracking. Each of those has to be considered.

First the material has to be susceptible and both types of stainless steel 304 and 316 is susceptible to stress corrosion cracking. And in weld zones that are made more susceptible by structural and metallurgical changes due to the welding.

Second, you have to have high tensile stresses and we have shown, ourselves, and also through NRC modeling that tensile stresses are high in the weld zones. We built a full diameter canister mockup and cut it into pieces and evaluated the stresses and found there were high tensile stresses in the weld zones.

Finally, you need an aggressive environment. We know that chloride salts are present on the canisters. We have evaluated dust from several different sites and found that there are chloride salts present. As those canisters cool, the salts will deliquesce and eventually form corrosive brines. That means -- this is why, although this data gap is identified 10 years ago, it is still a gap. Because we have shown that we have the tensile stresses through testing and through building a mockup and measurement. And we know the aggressive environment will occur because we have seen chloride salts on the surfaces.

So, our goal -- our overall goals then are to evaluate what the risk is. What sites are at risk? When will corrosion initiate during the cooling of the canister? How will the corrosion damage evolve over time? And when will cracks actually initiate? And how quickly will those cracks result in crack penetration? Next slide.

There are a lot of people working on this because it is still a gap. Sandia is the lead, however PNNL is doing a fair amount of work as well. And Savannah River and Oak Ridge have small parts. And then the whole variety of NEUP’s that Ned mentioned are also working on it, primarily on mitigation and repair technologies, cold spray, coatings, various other approaches, peening, etc. EPRI has had a fairly large role in evaluating several of these different processes. They are currently doing a fair amount work with respect to mitigation and repair. And then we have at Sandia a variety of other collaborations with people that are helping us understand stress corrosion cracking and pitting. Next slide.

If you think about stress corrosion cracking, you can think of a timeline for dry storage canisters. Canisters are initially hot, and they are too hot for brines to form on the surface, so that is the incubation time. Once brines form, you will start to get corrosion. The corrosion will cause pitting and the pitting will increase in size and eventually you will get initiation of stress corrosion cracks. The cracks will grow, and eventually, potentially, could result in canister penetration. And any time after cracks form, you might have mitigation and repair processes.

Sandia's role -- we are looking at defining the canister surface environment. We are looking at the relationship between the environment and the rates and morphology of pitting and stress corrosion cracking. And we are doing crack growth rate studies and finally we are doing mitigation and repair studies with both cold spray and coatings.

Now, all of this is being done within the framework which is provided by a probabilistic model for canister stress corrosion cracking that we have developed here at Sandia. We can take those different parts of the timeline and we can parameterize them as different features events and processes that eventually lead to, potentially, stress corrosion cracking.

We have developed, over the last several years, we have developed a number of sub models in this overall model, including models for understanding salt composition, the brine composition and how it evolves over time and how its properties evolve over time. We developed canister thermal models and we have got weather models for all of the different sites. And, of course, currently, PNNL is working on an air flow and salt deposition model.

We have also collected a lot of data with respect to crack growth rates. And we have a crack growth model, although that model has not been well parameterized for atmospheric crack growth. I would note that we use this model not to estimate when crack penetration could occur, there are too many uncertainties. So, instead we use the model to try to understand what the highest impact parameters are, the ones which we need to focus our research on in order to get the biggest bang for the buck to really understand this problem. Next slide.

So, just to go through quickly what we will talk about here. Our current focuses deposited salt compositions and characteristics. That is always important for us to understand the environment. Next slide.

Magnesium chloride brine evolution. Because magnesium chloride brines form early by deliquescence, and in fact, will be the only brines on the canister if we are dealing with sea salts for many decades to hundreds of years. Next slide.

The Canister Deposition Field Demonstration. Again, one of the big unknowns currently, is how quickly salt accumulates on the canister surface. And salt load is a very important parameter with respect to corrosion. So, this is something we really want to understand better.

Okay and then discussing the corrosion issues. We are looking at corrosion in more realistic environments such as diurnal cycles and the presence of inert dusts and precipitates with different chemistries being present. For instance, the effects of nitrate. Next slide.

We are looking at pit to crack transition and some of the things that can affect that. Specifically environmental and material dependencies. And then, next slide.

We are looking at crack growth rates. We are currently evaluating crack growth rates under immersed conditions as we develop our methods and our technologies. And then we are going to move towards atmospheric testing, and this is occurring at Sandia and PNNL. Next slide.

Okay. Finally, we are looking at cold spray mitigation as a repair technique -- mitigation repair technique -- and begun a coatings evaluation program. We are looking at corrosion resistant coatings. Next slide.

Okay. So, with respect to the environment, why are we interested? We are interested in it because of its influence on corrosion. Specifically, some of the things we have looked at recently were the effects of dust and precipitates which could act to wick brines out on the surface of the metal producing a more uniform brine layer, which could enhance corrosion. We are looking at effects of chemistry, potentially to mitigate corrosion, the presence of nitrate. In many inland sites especially, but even in coastal sites, there is nitrates present in the salts we have analyzed.

Finally, we are looking at diurnal cycles. As day and night cycles occur in the atmosphere, in the external environment. Those same cycles are repeated on the canisters surface although they are shifted because of the higher temperature. And we are evaluating how that would affect corrosion on the canister surface. Next slide.

Canister surface environment. Some of the things we are doing right now is sampling at sites when the opportunities present themselves. We sample at sites and obtain samples from industry, and we analyze those samples.

Just a year and a half ago we got our first data from inland sites. Something we have been wanting for a long time. These data were collected from two sites which prefer to remain anonymous, so we are calling them sites A and B. These are ISFSI sites towards the center of the country, in the Midwest.

The data were collected -- the samples were collected -- using a vacuum crawler that crawled over the surface of the canisters and collected dust samples using a small vacuum device, which is shown in the middle there. The samples were then shipped to Sandia where we analyzed them chemically. And we also characterized them via scanning electron microscopy to understand their structure and mineralogy. Next slide.

So what the analysis shows, at these sites, this is site A. The salts are dominated by calcium and sodium, and in terms of the anions, by sulfate, and site A, carbonate and nitrate with very, very small amounts of chloride present. The chloride concentrations are very small, and they are probably tied either to road salts and road salt aerosols or to cooling tower emissions. Next slide.

This is site B. The chemistry is similar but different. The calcium and sodium are still the dominant cations. And now the anions are dominated by sulfate and nitrate rather than carbonate. And there is slightly more chloride, but still very low concentrations relative to what we might see at a marine site. In each of these cases the amount of nitrate present, and very low amounts of chloride, suggest stress corrosion cracking might not be an issue. Next slide.

We can also get distribution information, to some degree, from these sites. The samples were collected at different locations circumferentially around the canisters.

But we can see here what the dust loads look like in different places. This information is useful for helping us potentially parameterize our dust deposition model.

You can see dust loads as you might expect are highest on top of the canisters where particles settle out on to the metal and lower on the steeper walls or bottom of the canisters where particles have to impact and stick in order to be collected. Next slide.

The second thing we are really interested in evaluating is the stability of magnesium chloride brines on the heated canister surface. As we said, the magnesium chloride brines are the only brines that can form on canister for several decades to hundreds of years because of the heat of the canister. Even a relatively small amount of heating lowers the relative humidity on the canister surface to the point that only magnesium chloride brines can form, and sodium chloride brines won't deliquesce and form.

For that reason, we are evaluating the brines stability. We know that at high temperature magnesium chloride brines degas HCl. And they will convert to hydroxy-chlorides and even to carbonates and dry out. The question is how quickly does this occur and at what temperatures will this occur?

So, right now we are looking at this because it helps – it is important to understand -- in order to evaluate the timing of the corrosion initiation on the canisters, the volume of brines that might be present, the morphology of the corrosion. Because it turns out that it is strongly dependent on the brine composition and magnesium chloride brines create a different morphology than sodium chloride brines. And also, for interpreting field results and extrapolating to -- lab results and extrapolating to the field conditions. Next slide.

We are also developing a thermodynamic model for the magnesium chloride system. Only recently has data been published to allow us to develop this model. And so, we have done that and now we have a thermodynamic database, which we can use to predict what we might see on these canister surfaces. Next slide.

Okay. The other thing we are looking at. One other thing we are looking at, is the deliquescence of these multi-component salts on the canister surfaces. Nitrate may have some benefit in inhibiting corrosion. However, it has a negative effect and that is that nitrate containing brines – salts -- will deliquesce, multicomponent salt mixtures will deliquesce at lower relative humidities and therefore at higher temperatures. We need to understand this and currently the thermodynamic databases are not good enough to predict when deliquescence could occur. And the experimental date for multicomponent nitrate containing mixtures is limited to say the least. Next slide.

Finally, Ned already talked about the Canister Deposition Field Demonstration. We are doing a fair amount of work to support that right now. The idea for this is to sample the canister surface by hand. The reason for that is we have seen the results of robotic sampling and we know they are not particularly quantitative. We think we can do better by hand. We have been working on that here. We have been developing and evaluating hand sampling techniques and have achieved fairly good results in terms of quantitatively removing the salts from the metal surface. We are also looking at other things like how to mark these areas we want to sample to determine whether or not they will -- so we can go back to the same spots over and over again to collect the salt as it accumulates.

This experiment, again, there are three different canisters here. One of them will have no heating, one will be 10 kilowatts and one will be 40 kilowatts. They will be horizontal canisters and horizontal NUHOMS overpacks and the duration of the experiment may be as long as 10 years. We hope to sample on a yearly or bi-yearly basis.

We are also evaluating how to define the boundary conditions. We will have weather stations out at the site once it is chosen. We will also have aerosol sampling equipment out there. We have purchased aerosol sampling equipment. It is currently here at Sandia, and we are learning how to use it. It will be moved to the site, when a site has been identified, and when we have access to start sampling the aerosols in the air, so we know the boundary conditions are, the amount of aerosols per unit volume and particle size distribution using cascade impactors in the actual aerosols entering the overpack. Okay, next slide.

Okay. Now I am going to go ahead and pass this over to Rebecca Schaller and she will tell you about the corrosion.

SCHALLER: Okay. Thanks Charles. So as Charles mentioned I am going to discuss the corrosion portion of our program. I going to start by talking about our large-scale laboratory exposures that we are performing. And for these we are looking at more relevant canister conditions because we want to be able to understand both the pit damage morphology and size that we are getting as a result of these different conditions because that would inform the propensity, or the likelihood, for a pit to transition to a crack, and for stress cracking to occur under these relevant canister conditions.

And as Charles talked about one of the first exposures, we are interested in is dust exposure. So, specifically looking at the influences of inert dust on the resultant corrosion damage across these materials. And to explore this -- one second -- to explore this what we have done is looked at a large coupon exposure set where we have coupons, we have co-deposited both sea water and inert dust on the surface of and taken these stainless-steel coupons and exposed them in three different atmospheric environments in our laboratories. And they are going to be exposed up to two years of time, but what I have here are the initial results from our one-month exposure after these coupons have been in these environments. And we have an example comparison of our static low relative humidity condition versus our high relative humidity condition and compared to a diurnal cycle.

And it is important to note the differences here when we look at that static low relative humidity, that is going to be below our deliquescence point of sodium chloride. So, what that means is we only have a magnesium chloride brine present on the surface, and it is a very low volume amount of brine. When we look at the higher relative humidity condition, we have a condition where we are above the deliquescence point of sodium chloride so now all of our sea water constituents are in that brine, and we have a much larger brine volume on the surface in that case.

And the finally diurnal cycle that only went in and out of the magnesium chloride deliquescence. So, we didn't get that large brine volume on the surface. What you can see is a difference in the initial corrosion morphology across these samples. That that higher relative humidity condition, we see a large difference in distribution of that salt across the surface that may be an effect of the dust that is also there on that surface helping to spread that brine across there and enhancing the corrosion rates in that case. So, we will further look at these samples over this long exposure period to get a better idea of these corrosion damage morphology and statistics across these coupons.

The second environment we are really interested in looking at that is more relevant to these canisters is looking at the effects of other chemistries outside of the sea water brine itself. Charles had mentioned nitrates and what I have here is an example of nitrate to chloride ratios. The dots are points we measured at real ISFSI sites compared to ratios on these dotted lines just to give you an idea of where they sit. And what we are doing now is looking at full immersion exposure of coupons in the laboratory where we take these nitrate and chloride brines. We immerse a sample in this brine and apply a potential to that. We apply a potential and read the current response of our material and that will give us an indication of the propensity of that material to corrode in that specific solution. When we have very low amounts of nitrates in solution, we see a high current response, as we increase that potential, indicating those materials are corroding. But when we increase that nitrate concentration, we don't get a high current response indicating that we are passivating those surfaces. We are not actively corroding when we increase that nitrate concentration. One thing to note though is that this is very variable dependent.

So if we look at that same ratio that 4 to 1 chloride to nitrate ration which was previously passive, if we change certain variables such as temperature, or the scan rate, which would give us an indication of exposure time, we can actually get that corrosion response in that same nitrate ratio. So, what that just means is we need to further study the different variables that might be present across these canister surfaces to better understand how nitrate can actually act at these field sites.

The last relevant exposure scenario we are looking at the influence of diurnal cycles. Charles has developed for us an example diurnal cycle that we would have at a ISFSI exposure site. And the reason these are important is that these canisters aren't static over time. We want to look at what happens when we change the relative humidity at that surface on our samples and how that might exacerbate or slow down potential corrosion rates on the surface of that stainless steel.

And so, these are just some initial results of coupons exposed to a diurnal cycle for about one month. We are looking at the corrosion damage morphology across these coupons, specifically the pits that formed in them after this exposure cycle. And what we can see is that it is not just a function of the exposure cycle, but it is also a function of the material surface finish where we have rough surface finishes at the top increasing to a mirror polish at the bottom row and it is a function of the material type. We have our 3O4H on the left and we have 316L on the right. Our corrosion damage morphology is both dependent on the relevant environment it is exposed in as well as the material finish and material type.

So, the other thing that we are doing in conjunction in studying with these corrosion exposures to get an idea of the damage morphology and statistics of these materials in these different environments is also modelling this. These exposures take fairly long time periods up to 2 years in many cases. So, if we can combine this with modeling efforts to understand what the damage morphology or size that we might get due to corrosion in these different environments would be we can predict the propensity of that pit to crack transition to occur.

What we have applied in our case is the Chen-Kelly model where we are developing an idea of the maximum pit size that could occur in a given environment. To do so, we are assuming we have a hemispherical pit that forms in our material. It occurs in an atmospheric environment, so we have a uniform brine layer across the surface of the material. And what's going to happen here, is the pit is going to grow or the anodic dissolution is going to occur. And that will only occur if we have a balancing cathodic reaction to support that anodic dissolution. Meaning that our cathodic reaction has to be equal and opposite to our anodic reaction to get growth of our corrosion pit on that material. And that cathodic reaction is going to be dependent on the brine properties itself. So, the water layer thickness, diffusivity in that solution, the brine conductivity, et cetera. And so, we can model that here. We can plot our anodic demand, this line in red, versus the radius of that pit. And we can also plot our available cathodic current density for a specific environment. And where those two intersect is the radius of the maximum pit that I could grow in that specific environment.

And so, what we have done with our modeling efforts is we have modeled these max pits and compare them to the maximum pit sizes that we have measured in our actual atmospheric exposure environment. And what we have found is that we have fairly conservative estimates. These pits that we predict they are actually 1.5 times larger than the maximum pits we are measuring in these static exposures.

Over this year, we have looked at is some of the assumptions we have made in that model. And one of the assumptions is we have a static brine on the surface, and it is not a dynamic brine that is evolving on these surfaces. So, what could happen in these brines is that we could get precipitation of different particulate in that brine as corrosion processes occur. In this specific case of sea water, we might get magnesium hydroxy-chloride precipitates. If these are sitting on the surface that could block our cathodic reaction, so we would have a lower available cathodic current density to support anodic dissolution. Meaning, we might likely predict smaller pits in those conditions. When we include those assumptions into our model and actually calculate the maximum pit, our max pit we actually calculate is much more in line with the actual pit measurements we are getting from our atmospheric exposures.

Not only can we do this for one single environment, but we have begun to also parameterize this maximum pit size model. So, what I have here is an example of the maximum pit size on the Z axis. And we have relative humidity for a full range of relative humidities on the X and temperature on the Y. We are looking at this for three different loading densities. Loading density refers to the amount of salt I have on my sample. And in this case, we are looking at initial loading density on that sample surface. And as we increase loading density, we see an overall increase in that maximum pit size we observed across the material.

The other thing to note is the relative humidity. As we follow that trend of relative humidity, as relative humidity decreases on that sample surface, we see a peak in our maximum pit size predicted. That peak corresponds to basically right before sodium chloride would deliquesce. That is when we have a very highly concentrated sea water brine on the surface. As we go lower in relative humidity, sodium chloride would precipitate out of solution. We would have a very, very low brine volume and we would get much smaller pits in general.

The other thing we can notice is that as we increase temperature, we see a slight decrease in maximum pit size. So, what we are able to do now with these models is to predict maximum pit sizes across a much larger variable space. So, we can understand the propensity for a pit to grow in our material and how that pit might transition to a crack.

Now, going back to that idea of a pit transitioning to a crack. As you notice, most of this modelling has been based on hemispherical pit. When we consider the pit to crack transition most of the modelling that is done looking at stress concentrations around a pit in material has also been performed on a hemispherical pit. So, there is some maximum pit size, or the Kondo criteria, a critical pit size that we have to reach for that to transition into a crack based on the stress concentration around that pit.

In our materials, if you have noticed at all, we don't always necessarily grow a nice hemispherical pit. In some cases, we do. This is a cross section through one of our cyclic exposures where it is a fairly nice hemispherical pit grown in that material. But in other cases, we have fairly large irregular pits with very sharp disparities at the edges of that. What we are questioning this year, under what environments do those pits grow? What are the factors that might influence those pits? And do those pits have a higher propensity to transition to a crack as those disparities might concentrate stress in different ways?

So, to explore this idea, we are looking at U-bend testing. U bend testing, the reason we are doing this is rather than plain coupon testing, we can induce the stress into our material. We can then coat that material with our sea water or chemistry of interest. And then expose that in our atmospheric chambers and look at the resultant corrosion damage morphology as well as the pit to crack transition in these materials over these exposure times to gain an idea of whether different environments will affect both the pit growth and that pit to crack transition in different ways.

Once we have an understanding of that pit to crack transition, as Ned has mentioned before, we want to have an idea of how quickly that crack might grow in our material.

So, this is our crack growth rate lab which we have established this year at Sandia. We have four load frames now. And as Ned mentioned, there is also another lab at PNNL with a similar set up. What we have done at Sandia this past year is we have been able to do is calibrate these load frames – sorry -- we have performed the initial calibration of these load frames this past year. And we did that calibration testing in there. And what we have is a system called the DCPD system, which is a direct current potential drop system, which can measure that crack extension in situ as the crack is growing.

And so, on the bottom here we have the crack length versus time for different stress parameters we are inputting into that machine. And on the top here we have an image of that crack looking at the post-fracture surface of the crack. The crack was grown left to right here, and you can see these bands of color very light bands. We can correlate that image with the in-situ measurement to calibrate our machine and make sure the crack extension that we are measuring from our DCPD system is the real crack extension on our material. So, we do that in air and then we take that same DCPD system and measure crack growth rate in these more relevant environments.

So down here, I have an example of an exposure in a saturated sodium chloride environment. You can see here we had a very nice even cracked front that was grown into that material, and you can measure a crack growth rate of that over time. What we are working on this year is what happens when we are in magnesium chloride dominated environment because we don't have as uniform as cracked front growing into these materials with time. We are trying to figure out how to calibrate our DCPD system combined with our fractography measurements to better understand how these cracks grow in more relevant environments.

Finally, the last piece of the corrosion work that we are performing is looking at potential strategies for mitigation and repair. We are looking at this for three potential scenarios.

One would be a prevention scenario where we are looking at applying one of these strategies initially in canister construction prior to the use of these canisters.

The other two would be once the canister is deployed. Two different scenarios were ex-situ repair. The repair strategy can be applied to a canister that could be removed from the overpack and we could have easy access to the surface. And the third strategy would be more difficult where we would have to deploy some type of robotic application method. And that would have to be done in between the annulus of the canister and the over pack itself.

And so, the two different efforts that we have ongoing right now, are we have a collaborative effort with industrial partners and that is based off of our FY20 coatings report. And then we have a second collaboration with Pacific Northwest National Labs to evaluate cold spray as a potential mitigation repair strategy.

So, in terms of our industrial collaboration, we set up a memorandum of understanding, or an MOU, with four different industry partners. And we are looking at currently 11 different variants of coatings. These will be ceramic as well as organic based coatings. We have received our initial coupons back from these companies and we are in the process of testing both adhesion as well as scratch testing to determine the coating adhesion to the surface. We are also looking at the contact angle of water and brine on that surface. And finally, some initial corrosion testing and evaluation of these coatings. And this is going to be an iterative and collaborative process with these companies so we can optimize these coatings as we go throughout these testing procedures.

The second collaborative effort we have is with PNNL and this is evaluating cold spray as a potential mitigation strategy. And so, we are looking at cold spray, and one of the things we are really interested in, is looking at this technique as a potential patch technique. So, this is something that is going to be applied in in-situ repair we wouldn't be able to cold spray the entire canister, but we would apply a patch to areas of need.

Before applying to a patch, what we really are concerned with is how does that edge behave of that patch. We are looking at two different edge strategies. One is a blended edge with the cold spray is on the right on the sample and the base material on the left. It is a little more evident in the front sample where we have a masked edge. Again, the cold spray is on the right and the base material is exposed on the left. This would all be base material underneath here.

We looked at three different materials initially. Two nickel alloys as well as just nickel itself. In terms of accelerated corrosion testing, the first thing we did was just evaluate the cold sprays itself. We exposed the cold spray again in full immersion. We applied potential to that cold spray and looked at the current response of the cold spray. And this dark red line here is the as-sprayed condition. In the as-sprayed condition, we get these current spikes. These current spikes are indicative of metastable pitting in the cold spray itself. When we ground that surface or polish the surface, we remove those spikes, so we somewhat enhanced the cold spray response a little bit in the 0.6 M NaCl solution.

The second thing we did is wanted, like I said, to look at how that interface behaved. So, we immersed cold spray samples in a ferric chloride test which is a full immersion pitting test and looked at the interface -- sorry -- the stainless steel, the cold spray, and the interface, post corrosion. So, what these are, are top-down images, the left being the stainless steel, and the right being the cold spray, and this is just the interface between the two. And then we have a zoomed in electron micrograph of that interface region. And what you can see the majority of the corrosion attack did occur at that interface between the cold spray and stainless steel itself. And we see a difference in morphology of that attack based on the edge effect. When we have that masked edge, we get much more localized pitting, and we also see crevice corrosion right at where that masked edge occurs.

We also wanted to explore the influence of different cold spray materials. We have Inconel, nickel, and super C. And we can see that the material selection influences the corrosion response at the interface. Once again, this is the top-down view of that corrosion response where we see a large attack when we selected nickel as our cold spray coating. And on the right what we have here is a cross section through that interface region. And what is interesting to note about these is we not only got corrosion in the base material where the cold spray wasn't covering but we also got crevicing attack underneath that cold spray edge. That crevice attack was also somewhat dependent on the porosity of that material. When we had higher porosity with the cold spray, we got crevicing not only into the base material but also up into the cold spray. This is all highlighting that in terms of cold sprays and mitigation technique we need to focus further on our material selection as well as our processing parameters to better enhance potential edge effects we might get in corrosion.

So, I just want to summarize our overall work that Charles and I have presented today.

In terms of our environmental studies, we performed analysis of dust from in-service canisters, and we are using this characterization of these canisters surface environments to inform corrosion testing. We have also looked at magnesium chloride brine stability and using this to predict timing and temperature of when corrosion might occur. The extent and morphology of that corrosion damage. We are also developing brine deliquescence relative humidities as a function of salt composition. And this will tell us when, and at what temperatures, of brine the surfaces of the canisters when we start initiating corrosion on the surface of the canisters. And then finally we developed plans for this CDFD or this large-scale salt deposition field demonstration.

In terms of corrosion testing and modelling, we are performing this in more canister-relevant environments to get a better idea of the pitting and pit to crack susceptibility in these canister-relevant environments. And we are expanding our modelling efforts to account for non-static brine conditions as well as corrosion to better be able to predict pitting and essentially SCC that might occur in these canisters.

In terms of crack growth rate, we have installed and calibrated our machines. And are starting our initial testing in more relevant environments with plans to move to atmospheric environments for crack growth rate testing.

And finally with respect to coatings we have our two projects where we have our collaboration that we have initiated with industry partners as well as the cold spray assessment we are working on with PNNL.

With that, I would like to acknowledge this project is a very collaborative effort not only at Sandia but also with our university collaborators as well as our DOE collaborators. And I would be happy to take any questions.

BAHR: Okay. Thank you very much, Rebecca. Do we have any questions from Board members? I see Paul Turinsky's hand up.

TURINSKY: I have two questions. One is canisters -- are there any requirements now on surface finish and residual stress, maximum residual stresses, in the weld area?

BRYAN: I would answer that. With respect to maximum residual stresses, there has been no stress mitigation on most of the canisters that have been emplaced in the field. They were intended to be for short term use, and stress mitigation wasn't being considered. Now, over the last few years companies have started evaluating efforts for mitigating stresses. I am not certain what has been implemented at this time.

With respect to canister finishes, that varies a lot. I have seen canister in the field that have mill finishes, and some which were ground quite smooth. But in general, I would say mill finishes are the most common that you see.

TURINSKY: My second question is time. From being able to initially see a pit, to the initiation of the crack, how long is that time, and how long is the time once the crack begins to through-wall? And I know it is very variable on that. Do we have any feeling on those time spans?

SCHALLER: With the pit to crack initiation that is something we are trying to get a handle on that this year. I think those U-bend tests will give us some indication of how quickly that might occur under these atmospheric conditions.

BRYAN: I would -- sorry. I would point out that there is another factor there and that's salt load. You know our max pit size model limits the size of a pit based on the salt load at any given temperature and relative humidity. As more salt is deposited a larger pit may be able to form. As the canister cools and the temperature drops the humidity goes up a larger pit may be able to perform. There is no a priori answer to how quickly a pit grows, it depends on several different factors which will vary from canister to canister.

TURINSKY: What I am concerned about is once you get to that point where you have a crack does that crack grow really fast through the wall at that point? So, you really have to detect what is going on before?

BRYAN: There is -- we have a crack growth model, and this same model is used by the ASME subcommittee that developed an inspection procedure. The crack growth varies with temperature, it increases with temperature. However, there are limits at which you are likely to get corrosion on the canister. So, the answer to that is it depends on how cold the canister is. For the ASME code case, we limited the maximum crack growth to 2.5 millimeters per year. But again, that would be for a hotter canister, where it might be very unlikely you actually had brine forming.

TURNINSKY: Must be the residual stress that explains that.

BRYAN: Yeah, the assumption is you can only apply these crack growth rates if you have seen a crack. So, the conditions for crack growth have been established and then you can apply the crack growth rates.

TURINSKY: Okay. There is a lot to be learned about these time spans is the sense I am getting.

BRYAN: Right.

SCHALLER: Yes, but I also think with the understanding the pit to crack transition it is not just the time span, but it is the conditions that it is more likely to occur under. There are conditions you may not reach that critical pit size under, so you may not have to worry about that pit to crack transition. So, it is understanding which conditions could produce and that and time spans are still a question as well.

TURINSKY: Okay. Thank you.

BAHR: Okay. I see Bobby Pabalan's hand up from the staff?

PABALAN: Yeah, Jean. Thanks Charles and Rebecca for the nice presentation. You described very interesting studies and very comprehensive actually. My question is for Bryan.

I have two questions on slide 20, you mentioned that you developed a thermodynamic model for the magnesium chloride hydroxide water system. Can you say something more about the model, the valid range of concentration, and temperature, for example?

BRYAN: Sure. There is data available. Just recently data has become available for a variety of magnesium chloride hydroxides that form at higher temperatures, up to 120 C, I believe, is the maximum temperature. We parameterize the model up to 150 C that is because we were using EQ3/6 and use the temperature grid which is in EQ3/6.

But without that data, we wouldn't have been able to parameterize the higher temperature phases that form.

There are several different magnesium chloride hydroxides that form. A few at ambient temperatures, two or three, and then you get five or six once you get up to 120 C. Again, many of those you will never see on the canisters because the relative humidity won't be high enough to have a brine present. Unless they form by solid state transition, I don't think we are likely to see those. But at least we have a model where we can now predict -- what we are trying to predict is, what we have observed in our experiments is that when you heat up the brines, they will decompose and will degas HCl and convert to a hydroxy-chloride phase.

Magnesium carbonate phase is predicted to be stable but of course those are kinetically inhibited from precipitating. So, usually the pH goes a little bit higher, and you precipitate out your magnesium hydroxy-chloride phase. We wanted to have a complete enough database so we could actually predict the conditions under which the magnesium chloride brine would be stable.

Of course, something is pushing back on that is the actual concentration of HCl in the atmosphere. In near oceanic environments you will have a concentration of HCl in the atmosphere. So, the brine won’t necessarily degas. Our preliminary work suggests we will only get degassing above, something like, 40 C. Below that the magnesium chloride brine will be stable because it will be generating a lower HCl concentration than is actually present in the air already.

PABALAN: The samples from sites A and B indicate the dominant cations and anions are calcium, sodium, sulfate, bicarbonate, and nitrate. To apply this to an actual canister environment don't you have to add those other components to the system?

BRYAN: Well, yes and no. If you look at those brines, those compositions, if you try to calibrate them the first thing is the calcium precipitates out as carbonate. The calcium carbonate in the salts -- in the dust particles -- is very, very fine particles that dissolve instantly when we leach these salts.

So, if you actually take those and try to evaporate them to simulate what will happen during deliquescence, all the calcium is removed, and you end up with a sodium-potassium nitrate-chloride brine. We do need to include nitrate and that's what we are doing experimentally.

In terms of modelling, we can model the monovalent nitrate chloride brines relatively well. Once we start adding divalent ions you can't. You may be aware, the Yucca Mountain R2 database is not qualified for magnesium nitrate-containing brines for instance. So, the database really falls apart once we start considering divalent ions like calcium and magnesium.

PABALAN: Because of the much higher solubility of those cations.

BRYAN: Exactly. We will certainly try to evaluate them experimentally to try to understand the behavior and try to understand the corrosion behavior. But in terms of modeling, we don't have the resources to build a model that would be able to handle this.

PABALAN: This brings a follow up question. Have you looked at or considered using the mixed solvent electrolyte model that can provide you the capability to basically go up to full nitrate salts?

BRYAN: Maybe. I haven't looked at that. I know that it is based on the same thermodynamic data and so the limitations and the thermodynamic data mean that, for instance, if we use that model to predict our brine compositions, we get the same results. Okay. Within the limits we are trying to predict. We haven't -- I don't know if the data are available to extend it to more complex systems reliably.

PABALAN: I think there is a database available for complicated systems. You can take a look at it. The disadvantage is it is commercial software, so you need to pay a license fee.

The other question I have, is you plan to measure the deliquescence relative humidity of nitrate-containing systems. What experimental method do you plan to use, and up to what temperature, do you plan to use?

BRYAN: There are two methods we can use. We can use quartz crystal microbalance. The quartz crystal microbalance will tell us when the salts deliquesce. They won't give us any information on mass gain after that because as soon as the salts deliquesce, they decouple from microbalance, and we can't measure weight gain anymore. But they can tell us when the deliquescence occurs.

The second tool is an instrument which, fairly recently purchased in the corrosion group here, and it does measure weight gain as a function of relative humidity quite accurately. And it is not quartz crystal microbalance based, although it is quite sensitive, able to measure tiny fractions of a milligram. We will also look at that and look at deliquescence of the salt mixtures using that tool.

PABALAN: Up to what temperature do you plan to use?

BRYAN: I don't know what the temperature limits are on that device. For quartz crystal microbalance we can go up to probably 100 C depending on which quartz crystals we use.

PABALAN: Thank you.

BAHR: I see Dan Ogg's hand up. Dan.

OGG: Yes, thanks Charles and Rebecca. Could we bring up the slides and go to slide 19, please? And this is the salt loading on the two canisters at sites A and B. I'm not asking you the tell us what sites those are. But, can you tell us if you learned anything from the fact that site B had a great deal more salt loading on it than site A? Is that just the age of the canister or some other storage configuration that may have contributed?

BRYAN: What probably contributed the most is site A is the first time the sampling tool had ever been used on that particular robot. When they sent us the samples, we found that the Scotch-Brite pads and filters had been rather loosely emplaced. And we could see that dust had flowed around the filters. So, there were issues with the sampling, and we told them this. And they improved the sampling device, and also the emplacement of the filters and the Scotch-Brites, and were much more effective at collecting samples at site B. So, I suspect that site A was not very quantitative.

OGG: Okay. Did you check the storage age to see if that might have been a factor?

BRYAN: We don't have a lot of information on these sites. I think they were both around 20 years because both of them are undergoing their 20-year inspections. I don't know the exact age. I don’t’ know heat load. And I don't know anything else about these sites.

OGG: Okay. I was just curious about that. Thank you.

BAHR: Thanks. Yeah, I was going to ask about the age as well. Bret Leslie has his hand up.

LESLIE: Charles and Rebecca, as Bobby said, there was a lot of information and you guys have done a lot of work. Charles, you said something, and it was fairly late in your presentation, but you were talking about -- let me find my notes -- you were going to sample by hand. I understand this might be the canister field deposition experiment. And I understand you are sampling by hand at Sandia. But is that also going to be at the field deposition site itself. And the reason why I am asking this, is how would you correlate or extrapolate your sampling technique to what the industry is doing right now?

BRYAN: We are not trying to. We are trying to parameterize the PNNL dust deposition models. What we want to do is accurately sample the salts that are deposited on the canister surface, and we want to sample that through time.

The reason there are four -- each grid there contains four blocks -- we are assuming each will be identical. And then we can sample one block after a year, and one block after two years, and after five years. And we can treat those as being identical and look at the effects of salt accumulation over time. Dust accumulation over time.

Now all of our sampling sites are one side of canister. If the opportunity presents itself, we may be able to use robotic sampling on the far side of the canister and compare it to our results. But our goal is to do the most accurate sampling we can. And if we can -- if robotic sampling done on the other side canister it would have to be done in such a way as to not disturb what we are sampling.

LESLIE: Okay. I understand what you are doing but is there a way that -- I mean, I am just trying to see, you know, you have talked about the problems with robotic sampling. That is the testing technique that is being used now. If your hand measurement, say there is a lot more salt than maybe the robotics test, how can one look at the existing data and say whether there is an issue or not?

BRYAN: Oh. I don't know if there is a way. The robotic sampling hasn't been validated is the problem, hasn’t been shown to be quantitative. Based on observational data, we question how quantitative it is.

It does provide other information. You have a sample of dust you can find how much chloride is in it. You can do an analysis of the sample and can get a compositional information. We have seen for instance in the data from sites A and B that the salt loads are higher on the top of the canister. That is intuitively correct. But I would say that it is difficult to say -- to make any statement that the measured salt loads are identical or correct in terms of what is actually present.

LESLIE: Sure. Thanks. Thank you.

BAHR: Just another question on this hand sampling. Will you be removing the canister from the storage compartment to do the sampling, and if so, what might -- just that process of removal do to the salt distribution?

BRYAN: That's a good question. A very good question. Yes, we will be removing it. The heaters will be turned off and the canister will be allowed to cool inside the overpack for a few days. Then we will pull it out and sample it by hand.

We are concerned about the possibility of vibrations resulting in dust falling off of the canister which is why we are -- the skid will actually -- and potentially the rollers or the rails inside the over pack will potentially have rollers to try to minimize vibration as the canister is removed. We are evaluating the cost of having rollers placed inside the overpacks.

BAHR: Okay. Thank you. Have a question from Andy Jung from the staff. Andy?

JUNG: Yes, thank you so much for the presentations. Very informative. I believe your testing and modeling are very comprehensive.

I have a couple of questions. I can understand that as a precursor process for the stress corrosion cracking initiation you consider pitting corrosion as the mechanism. But there is another localized corrosion mode such, like as, a crevice corrosion that can serve also as a precursor for crack initiation. Like some metal contacts each other, such as the canister contact with the supporting rail in horizontal dry cask storage system. And vertical has some areas can contact. So, I saw that recent presentation from the ESCP, and a researcher from the Taiwanese present some pitting. The crevice corrosion can serve as a crack initiation. So, I am wondering if you are considering crevice corrosion in your model?

SCHALLER: We haven't yet considered crevice corrosion. The reason we are evaluating pitting corrosion is because it is the majority of the corrosion that we would expect to see in there.

In terms of pit to crack transition or crevice to crack transition it will be based off of a -- you know -- a damage mode depth or size. And looking at some of these regular shapes of pits can inform crevice corrosion because the damage morphology may look very similar to that. We can get some information from these other modes. But I think when we change just from this hemispherical geometry that may inform how crevice to crack initiation might occur as well.

JUNG: Yes, I read that actually pitting could be the other modes from crevice. But I saw that the literature, especially the EPRI report, for the long-term atmospheric testing. It turned out actually the crevice corrosion rate is higher than pitting corrosion from the field testing. So, I just I wanted to make sure you may have also considered crevice corrosion.

The second question is that in your modelling work, have you conducted some sensitivity analysis which parameter or which process could be highest impacts on the crack initiation and crack growth? And which parameter could be the most, largest, uncertainties have?

BRYAN: Yes, we have done a great deal of that. That's what we use our model for. And with respect to crack initiation, there are a variety of parameters that are important.

One obvious one is K_{1SCC}. The crack tip stress intensity factor that is necessary to initiate a crack. Again, this is actually a lumped parameter. K_{1SCC} basically assumes you have a hemispherical pit to start with. If you are dealing with an irregular pit, like we see in magnesium chloride, it may be that you actually require essentially a lower K_{1SCC} because you are focusing the stresses more because of that. So that -- we are using K_{1SCC} as a lumped parameter to incorporate things like the morphology of the pit as well. And other uncertainties, for instance like the uncertainty in the stress field. But that's one that is obviously very important and one which we are evaluating with respect to brine composition. That why it is one of the major things we are evaluating.

The other thing that is important is the crack growth rate itself. And the crack growth rate itself, we have evaluated the literature data and there is an order of magnitude or two in uncertainty at any given temperature in the crack growth rate. So, one of the goals, the goal of the program as a whole is to try to understand with more certainty what the applicable crack growth rates are over the range, temperature range of interest.

JUNG: Okay. Great. Thank you so much.

BAHR: I think we have time for one more question. I see Paul Turinsky’s hand up.

TURINSKY: I was just wondering because your experiment is going to be done on a horizontal or in a canister. Have you folks reached a conclusion that horizontal canisters, all factors being equal, are more susceptible than vertical canister orientations? I know the air flow is substantially different. One is a very annulus flow, and the other one is a much open environment.

BRYAN: We have not reached that conclusion. We do see, in general, there is more dust on the horizontal canisters on the sides of the horizontal canisters than there are on the sides of the vertical canisters. And dust load may be an important factor, but we can't say for sure whether one is more or less susceptible. Because dust load may not be as important as we thought. Especially when we consider that we may be getting initiation from very small pits, if magnesium chloride brines result in very irregular pits. So, salt loads may not be as important as we think.

It would be great to have another opportunity to set up another canister deposition field demo, but I suspect that Ned would push back on us spending the money and doubling the cost to have another test.

BAHR: Okay. Well, thanks to both Charles and Rebecca. And then the final thing we are going to do today is we are going to have public comments. These are comments submitted already in writing and Bret Leslie from the staff is going to read them. I will just mention that we will not be attempting to -- we welcome these comments and will put them in our record, but we won't be attempting to answer any of the comments in our meeting today. So, I will turn it over to Leslie.

BAHR: Well, if we are not able to hear the comments today, maybe what we should do is reserve those for the meeting for tomorrow. Is that acceptable Bret? He says yes.

We will call today's meeting to a close and hope that we will be able to work out the problems that we have, and we look forward to seeing everybody back here tomorrow at noon eastern time. And we will have another set of interesting presentations on additional work that the

Department of Energy is doing. Thank you all for your attendance.

(At this point, the meeting concluded.)