United States

Nuclear Waste Technical Review Board (NWTRB)

Transcript

Summer 2021 Board Meeting

VIRTUAL PUBLIC MEETING - Tuesday

August 24th, 2021

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>>JEAN BAHR: Okay. Hello and good morning and welcome to the US Nuclear Waste Technical Review Board summer meeting. I'm Jean Bahr, Chair of the Board. This meeting will focus on the US Department of Energy’s technology development program related to aluminum-clad spent nuclear fuel packaging, drying and dry storage. Due to the COVID-19 pandemic, we're holding this meeting in a virtual format. Mr. Kurt Wright of Precon Events will serve as the host for the meeting. I will introduce the other Board members and then briefly describe the Board and outline what we do. I'll tell you why we are holding this meeting and summarize today's agenda. I ask that as I introduce them the Board members will activate their cameras to come online and say hello so the audience can see who they are. I think at this point we should be switched to ‘Panel View’. Thank you. So I'll begin I'm Jean Bahr the Board Chair and all Board members serve part-time and we hold other positions. In my case I'm a Professor Emerita of Hydrogeology at the Department of Geology at the Department of Geoscience at University of Wisconsin-Madison.

Our first two Board members are only able to join us today via audio. First is Steve Becker. Steve is Professor and Chair of Community and Environmental Health in the College of Health Sciences at Old Dominion University in Virginia. And then Mr. Allen Croff. Allen is a nuclear engineer and Adjunct Professor in the Department of Civil and Environmental Engineering at Vanderbilt University, and you'll see both of them represented by the Board logo when they speak to ask questions.

Next is Dr. Tissa Illangasekare. Tissa is the AMAX Distinguished Chair of Civil and Environmental Engineering and Director of the Center for Experimental Study of Subsurface Environmental Processes at the Colorado School of Mines, and I don’t see Tissa’s camera activated … Tissa, are you there? Well hopefully he will be joining us shortly.

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

>>LEE PEDDICORD: Good morning.

>>JEAN BAHR: Good morning. Next is Dr. Paul Turinsky. Paul is the Board’s Deputy Chair, and he’s a Professor Emeritus of Nuclear Engineering at North Carolina State University and if for some reason during this meeting my link goes out, Paul will kindly take over for me in the role of meeting host. So, I just introduced five Board members plus myself, not the full complement of 11. Our other Board positions are currently vacant.

As I usually do at board meetings, oops, am I still here? Okay. As I usually do at Board meetings I want to make clear that the views expressed by 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 now 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 Nuclear Waste Policy Act to perform objective on going evaluations of the technical and scientific validity of DOE activities relating 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're mandated by statute to report Board findings, conclusions, and recommendations to Congress and 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 used by policy makers, Congress and the administration. And all of this information can be found on the Board's website, www.nwtrb.gov, along with Board correspondence, reports, testimony, and meeting including archived webcasts of meetings. If you would like to know more about the Board, a two-page document summarizing the Board's mission and presenting a list of Board members, can be found on the Board's website. And I just got a message that my connection is unstable… I hope it will continue to work. We will have public comment period at the end of today's meeting. Because of the virtual format of this meeting, we can only accommodate written comments. As you join the meeting today, on the right of your screen is a ‘Comment for the Record’ section where you can submit your comments. If you are viewing the presentation in ‘full screen mode’ you can access the ‘Comment for the Record’ section by pressing the ‘Esc’ key. Information on how to submit comments will be displayed during the break. Comments we receive before or during the meeting will be read by Staff member Bret Leslie in the order they are 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. Comments and any other written materials may also be submitted later by mail or e-mail to the points of contact noted in the press release for this meeting, and that is posted on our website. These will also 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 that 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 can be downloaded.

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 DOE spent nuclear fuel. In 2017 the Board released a report that recommended that DOE take several actions on management and disposal of its nuclear fuel including aluminum-clad spent nuclear fuel. During the public meeting in November 2019 the Board heard updates from national laboratory representatives regarding some of the same topics that we will hear more about today. As part of its spent fuel inventory DOE manages approximately 20 metric tons heavy metal of aluminum-clad spent nuclear fuel, primarily at Savannah River Site in South Carolina and at the Idaho National Laboratory. Much of the spent nuclear fuel is in deep water filled pools that provide cooling for the fuel. The water also provides shielding to protect workers and equipment from radiation emitted by the fuel. These spent fuel pools have been in use for many decades, and some have limited remaining lifetimes. Additionally, the Department’s only large scale spent nuclear fuel processing facility, the H Canyon of the Savannah River Site is projected to have an end-life in 2030s. So, considering the aging of the spent fuel pools and limited remaining lifetime of the H Canyon facility, the Department decided in 2017 to examine alternatives from moving aluminum-clad spent nuclear fuel from pool storage into long-term dry storage. As part of that 2017 decision, DOE identified several technical challenges associated with packaging, drying and long-term storage of its aluminum-clad spent nuclear fuel. These challenges are similar to those noted in the Board's 2017 report. The DOE tasked the Idaho National Laboratory to prepare an action plan to explore and address these challenges and this led to the formation of an Integrated Technology Development Program. Our meeting today will examine this integrated program. We'll hear an update on overall progress of the whole program, and then we'll hear presentations on specific research and development efforts in five technical subtask that are active today.

Today's meeting will start with opening statements by Mark Senderling and John Shultz from the DOE Office of Environment Management who will tell us about DOE’s support for technology development program on aluminum-clad spent nuclear fuel. Then we will hear from National Laboratory managers and researchers who implement the program. Josh Jarrell will provide broad description of the Integrated Technology Development Program and tell us how it all fits together. Then we will move on to specific technical presentations. First Rebecca Smith will describe research on the drying of aluminum-clad spent nuclear fuel and after a 20-minute break starting at 1:30, we’ll hear from Greg Horne who will tell us about research on radiolytic gas generated by aluminum-clad spent nuclear fuel. Next Anna d’Entremont will explain the use of aluminum surrogate materials to support radiolysis testing and Alex Abboud will describe Modeling and Simulation Results from aluminum-clad spent nuclear fuel in DOE standard canisters. Our last presentation will be from Evans Kitcher who will tell us about remote sensors being developed as part of Instrumented Lid Project in support of Dry-Storage. Then as I mentioned earlier, we'll have a public comment period during which staff member Bret Leslie will read the public comments we have received and we will adjourn the meeting at 5:10 pm eastern time.

So, much effort went into planning this meeting and arranging the presentations, and I want to thank our speakers for making presentations today. Thanks also to Board members Allen Croff, Paul Turinsky and Lee Peddicord who acted as the Board leads and Board Staff particularly Dan Ogg [inaudible] for putting the meeting together. Now it is my pleasure to turn the meeting over to Mark Senderling who will get the meeting started. Mark?

>>MARK SENDERLING: Thank you, Jean, and I want to give thanks to the Board for the opportunity to share some results and studies under Spent Fuel Technology Development program. I want to emphasize that maintaining a safe and efficient approach to DOE spent fuel is a priority at spent fuel storage sites here in EM. To review the bidding, EM has been funding dry-storage technology studies since 2017 when the Department issued a report that recommended studies be conducted relating to dry-storage of aluminum-clad and non-aluminum-clad spent fuel. This was consistent with the NWTRB report of same year 2017 as Jean has mentioned entitled *Management and Disposal of US DOE Spent Nuclear Fuel*, which also pointed out areas for study. In 2019 Dr. Connelly from INL provided the NWTRB some preliminary results of spent fuel technology development work that was on going. Today we will show the progress that has been made since then regarding drying, managing and storing spent fuel. We look forward to the interactions and discussions. EM values the NWTRB relationship and continued collaboration with the Board and with NE.

First up is Dr. John Shultz from my office to address the three questions raised by the Board. Hitesh and I will stay online after John's presentation, along with John to answer any questions. Thank you. John?

>>JOHN SHULTZ: Okay, if you will bear with me, I am trying to get my video started. It says ‘the host has stopped the video’… Ok start my video.   There we go. Alright now, let’s go to, there we go. Share my screen. Can you see the presentation now?

>>JEAN BAHR: Yes, we can see it now. Thanks John.

>>JOHN SHULTZ: Well, thank you for the opportunity to give a presentation on the progress we made on management of aluminum-clad spent nuclear fuel. I am relatively new to the office as far as managing the spent fuel. Before this I worked at the National Energy Technology laboratory in Morgantown, as a research engineer there for about ten years. So, I got some experience in the field and I'm back, I feel more at home with this Technology Development effort, very similar to something I used to do when I worked at the National Laboratory. I like to briefly cover, before we get started, the three questions that were presented to us to be answered by the Board. What are the plans and goals of the DOE program to support research on aluminum-clad spent nuclear fuel? That’s question 1. On what schedule does DOE plan to move aluminum-clad spent nuclear fuel to dry-storage at Idaho National Laboratory and Savannah River Site? And what is the anticipated funding level for this fiscal year and beyond [inaudible]. Well, just to let you know how TD is already organized, there is a TD office that we have that ultimately is the say for the sign-off for the budget and everything, but our office is charged with managing this particular effort, the technical aspects of this particular effort of Technology Development of spent fuel. I'll go into how TD or R&Ds are organized in EM. If you'll notice.. first bullet points out that we have a TD office, EM-3.2 and they provide general guidance, oversight of technology work within EM. But that office has a very small budget, as you see $25 million. And of that budget, most of it is Congressionally-directed and the work we're talking about today on spent fuel, that is part of that congressionally directed funding; year over year since about 2017 we received about $5 million a year from congressionally directed authorization for this spent fuel work.

Our office, the EM-4.23, that's the office I'm in, Mr. Senderling is in, Hitesh, and others we provide technical oversight of the aluminum-clad spent fuel scope of work, we discuss that with the TD office, EM 3.2 and all get together and figure out for each year which tasks we would like it pursue for that subsequent year and budget allocation for each task.

What are the plans and goals the program to support research and development? As I mentioned we work with the TD office in regards to find the scope each year, as a matter of fact we will be meeting next month to figure out the scope of work for this next year. INL TD office, myself and others will meet virtually probably but will come up with a proposed scope of work and plan for the next year. Now everything we're doing is in accordance with some of the studies mentioned earlier, particularly 2017 NWTRB report and our own Spent Nuclear Fuel Working Group report that was also published in 2017 I believe, which provided kind of a framework or, number of lines of inquiry for research going forward.

For the results to date, as was mentioned earlier in 2019 Dr. Connelly provided some preliminary results but we're much further on in the process now, we got what we believe is a pretty good news story of the work that's been done so far, the results and the details of the research will be presented later by Josh Jarrell and principal investigators, but we think we have a good news story to tell. For example, we show that presently as aluminum-clad spent fuel in vented interim dry-storage is safe for extended Dry-Storage. L-basin material can be safely placed in sealed in canisters without corrosion or degradation, et cetera. As we move forward, particularly from this fiscal year beyond we'll be wrapping up existing studies more or less research activities and moving into more operational studies of how do we actually remove, dry, and store actual aluminum-clad spent nuclear fuel. That will be the focus as we move forward, operational concerns and finalizing some of the research activities that's been on going for the last four or five years.

Okay the second question the Board asked, what schedule does DOE plan to move aluminum-clad spent fuel to dry-storage? Well almost all aluminum-clad spent fuel at Idaho is currently in dry-storage and that material in the basin 666 will be emptied by the end of 2023. Some Advanced Test Reactor fuel will be managed in accordance with the signed agreements and the key here is we're moving forward with a project to make this fuel road ready in a dry-storage road ready configuration. We're just in the initial steps of that, the first part of this would be an external pad, to put the canisters on and in addition to that a large, relatively large packaging facility also would need to be built. But the first step is this, we got CD-0 out on the streets now to build a storage pad and so as time moves forward we'll be with budget constraints in mind, constructing a facility to retrieve, package and then store on a pad in road ready configuration, aluminum-clad spent fuel.

Savannah River, that's a little more complicated the Savannah River, because there are a number of things going on at the same time. Current baseline is to dry and store aluminum-clad spent fuel on site. However, that would require a new capital project and that's… that's potentially problematic, particularly since there is something else called the Accelerated Basin De-inventory proposal out there, which if implemented and approved would process all fuel through H-canyon and end up sending it over to DWPF [Defense Waste Processing Facility] to be vitrified and eventually disposed of as High Level Waste. So it’s a more complicated picture at Savannah River because there’s other things going on and agreements in the works that have not yet fully approved that kind of complicate that picture.

Anticipated funding level for fiscal year 2022 and beyond we presume that the $5 million that has been congressionally appropriated will continue and, as I mentioned earlier, we will negotiate with INL, Savannah River National Laboratory, TD office and our office as to what is the appropriate scope of work, what the tasks should be and the timeline for deliverables for those tasks. We do that every year. So that's the end of my presentation, are there any questions before I turn it back over?

>>JEAN BAHR: I see that Lee Peddicord has raised his hand.

>>LEE PEDDICORD: Uh, yes, may I go ahead?

>>JEAN BAHR: Yes, please Lee.

>>LEE PEDDICORD: This is Lee Peddicord, Board member, thanks John, very interesting presentation. Just to help me understand ..so I have the impression your TD budget is pretty limited each year, uh and um, then if I understood correctly some of it, a lot of it is congressionally directed, did I understand that correctly?

>>JOHN SHULTZ: That’s exactly right. The overall budget for the office, that’s the TD office, EM 3.2, is $25 million or so.

>>LEE PEDDICORD: $25...

>>JOHN SHULTZ: Now of that 70 to 80 percent every year is congressionally directed, which means there is a budget item in the budget that tells where that money will be spent more or less..and for what. For example, of the $25 million, $5 million, year over year, is for spent fuel R&D, written that way for Idaho National Laboratory to perform research on spent nuclear fuel, it is relatively brief, maybe one paragraph, small paragraph but that's what it is and so there is, filter research, there is a number of project that is are like that…

>>LEE PEDDICORD: So I guess my question to you then is do you um get to have a dialogue let's say with Congress as they are directing the 70 or 80%? and where I am ongoing with this, are you able to surface your priorities where you think the TD work should go? And finally, you do have this remaining balance you talked about kind of $5 million I guess you get to decide. I'm just trying to get a feel if you guys have the flexibility to meet all the needs you feel your identifying with the way the TD budget is setup and again being Congressionally directed?

>>JOHN SHULTZ: Well we have been fortunate on the spent fuel portion of this, the $5 million,… I think we have been doing well with that. It is in line with what we as an office would suggest or recommend, I believe. I don't think that, I can't speak to the other TD work that EM would like to do. Seems to me that $25 million isn't a whole lot for the rest of the EM.

>>LEE PEDDICORD: Yep.

>>JOHN SHULTZ: I think $5 million is reasonable and we have had very good luck coming to agreement on what should be done as far as the TD work so I don't, I don't have a problem with either the amount or the scope of work or the tasks that were selected. I think we have been fortunate. Savannah River has been involved in this, and it hasn't all gone to Idaho for their use, they have shared with Savannah River, because they happen to have some of the

experts at Savannah River National Laboratory who can contribute to this. So it's been more of a team effort than one might assume given it's Congressionally directed money. Everybody’s worked diligently and done a good job. EM's program overall I don't.. I think it might be a little light and we don't have the opportunity to --

>>LEE PEDDICORD: Well so that was the loaded question I was going to finish with, could you use more money and would you like to have more flexibility in doing that? And you can pass if you want.

>>JOHN SHULTZ: I can't speak on behalf of… Kurt Gerdes is the manager of TD office, from our office perspective, looking at their office and what they deal with, I would agree with the contention that they could benefit from an increased level of funding. Like I say I'm not their office, but I would say they could benefit from that and $25 million for the entire EM doesn't seem like a lot.

>> [KURT GERDES]: John this is Kurt, do you want me to weigh in on this?

>>JOHN SHULTZ: Oh year, Kurt, I didn't know you were on this, please do.

>> [KURT GERDES]: The bottom line is we are woefully underfunded, for what we need to do based on site priorities based so like John said that for spent nuclear fuel we have been fortunate in that yes, it is a Congressional directive but have been able to, to do the work that's needed. The priority work. Based on, you know, other companies and research has shown that, you know, what would be reasonable to spend on R&D is roughly 3% of an overall budget, and it has been quite a few years since the mid-90s since we have had anything close to that for a Technology Development budget. The challenge is looking at the highest site priorities and saying ‘what can we do to make a difference’, and the sites do have a little bit of funding that they also put towards Technology Development but we're still, you know, way short on terms of addressing all of the needs that we need to.

>>LEE PEDDICORD: Very good, I appreciate that. That's really a good credible answer I think. Thank you, I appreciate, thank you.

>>JEAN BAHR: So next I see Paul Turinsky with his hand up. If we can get Paul on to camera.

>>JOHN SHULTZ: Okay.

>>PAUL TURINSKY: Can you hear me now? Let me see if I can get my video.

>>JEAN BAHR: I think we have your sound Paul, but I'm not sure if we have your video.

>>PAUL TURINSKY: Yeah, yeah, I just got the message. Start my video. Just a little different than normal. Okay my question is a little, little further out in time and that is for the spent fuel, the aluminum spent fuel that's stored at sites that will not reprocess at that site, it implies transportation sometime in the future. Now, one of the issues with transportation is to understand the state of the fuel more mechanically-oriented, structurally-oriented at the time of transportation. Is there any thought processes going on ..longer term planning of collecting the data that's required to do assessments of the integrity of the fuel during transportation? It would seem some of that data may be an opportune time to do that while you are working with samples currently.

>>JOHN SHULTZ: Well um, you know somebody can correct me here but once we got it packaged and road ready configuration on a pad, I don't believe there is going to be any further opening up of that container to investigate structural integrity. However, we are doing, we are looking at the structural integrity of spent fuel, actual pieces of spent fuel, that have been degraded over time. So we are gathering data on that, that very thing. And we do have some severely degraded fuel that will need to be repackaged… short answer is once it is in the container on the pad, ready for road-ready storage as in agreements with state of Idaho, for example, I don't believe we are going to go back and look at any structural issues, it is what it is and it is going to stay that way until sent to somewhere either interim storage or repository. We are doing investigations of the structural integrity of certain fuel pieces and elements and stuff now. Josh would be the one, Dr. Jarrell would be the one to give you the details on what's been done and we can surely follow-up with some e-mails to you,

reports and stuff that have been done if you are curious about that.

>>PAUL TURINSKY: On the commercial side, that’s been a big study that’s been done on high burnup fuel, on measuring properties into the fuel, developing mechanical models, actually evaluating the stresses on the fuel during transportation. Shocks and all, that may occur on that and I guess I didn't see a similar sort of effort and I would be a little more concerned with 93% enriched fuel than 4% commercial fuel on reconfiguration for criticality and things like that.

>>JOHN SHULTZ: Why don't I talk to Dr. Jarrell and see in that particular area that you are interested in what, what is the status and what's being done. I know that like I said, that specific issue of once it is in a configuration, knowing how it behaves during transport look I don't really know myself if that's been addressed. I know they've looked at the actual fuel plates removed from storage or removed from pools and their degradation and what caused it. The surface corrosion, that kind of thing. But as far as transport issues, honestly I'll have to get back to you on that.

>>HITESH NIGAM: Yeah I can add a little bit to that… this is Hitesh Nigam, I work with John in the Environmental Management program with the spent nuclear fuel office and yes, transportation is a future thing as you mentioned and it will require addressing. As you know we have spent aluminum-clad fuel at both Idaho and Savannah River Site. Savannah River Site is still sitting in wet storage. We still haven’t decided what the proper path forward for that is. John mentioned this accelerated base in the inventory mission that we're considering, which will process all the aluminum-clad so if that goes through we do not have to worry about transportation. But our base case is to put that fuel in dry-storage.

Once we have moved the fuel from wet to dry, that's the stage we will be focusing on transportation to make sure that fuel is packaged in a way that it can be transported to a repository. Similarly in Idaho most of the fuel is already in dry-storage, but it requires repackaging. And that needs to be in road ready dry-storage conditions and Idaho feels, as we move forward with it and that we're getting started with building a storage pad and a packaging facility in the future, and as we move that fuel from current dry-storage to road ready dry-storage, that issue will be addressed at that time. So yes, transportation is a little bit up ahead and we're kind of trying to resolve the issues that are needed today, which is basically getting all this fuel in dry-storage conditions so it can be safely transported and disposed in the repository.

>>PAUL TURINSKY: Yeah, I mean the key thing is you need to know the properties of the fuel mechanical properties um to evaluate basically its structural integrity during movement, during transportation.

>>HITESH NIGAM: Yes.

>>PAUL TURINSKY: Just wondering if it has been thought out yet what the plan is for getting that implementation.

>>HITESH NIGAM: I know we have worked with the Board on this issue in the past I know Office of Nuclear Energy is heavily involved in the transportation issues and they are looking at the commercial fuel and we're working closely with them.

>>PAUL TURINSKY: Okay, thank you.

>>JEAN BAHR: Steve Becker, you can get your microphone on, thank you, Steve.

>>STEVEN BECKER: Thanks Jean. John, very interesting presentation um very helpful and informative. I have two quick questions, um first I want to circle back to Savannah River when you were discussing Savannah River you mentioned the need for a new capital project and said that it was a bit, I think the term you used was ‘problematic’. Is this due principally to funding issues raised by Lee, or are there other issues as well?

>>JOHN SHULTZ: No, I haven't been advised by anybody, my personal opinion.. it's capital projects in general DOE, large ones, this would be a large one, are difficult. Their funding profile, getting the money and then getting it started, getting through the CD process is challenging. That's what I meant to convey. Not referring any kind of internal information or study, I'm just saying that to do that, to take that and build a new capital

project in general, in DOE, is a challenge… versus this other strategy that Hitesh mentioned, I mentioned, Accelerated Basin De-inventory where everything would just be processed, you know, then sent over to be made into glass logs, right. That's a different strategy altogether. You don't have to worry about additional capital project. It's all the fuel would be run through H Canyon, dissolved, and then eventually the waste generated from there is sent to be made into glass logs, you avoid that complicated expensive capital project if you process material that way. Does that answer your question?

>>STEVEN BECKER: Yeah so basically you are saying just the complexities that would go with any large capital project?

>>JOHN SHULTZ: Yeah I've been involved with several large capital projects and I mean oh gees, you know it can bite you… it can bite you.

>>HITESH NIGAM: All of that is true, what John mentioned but small problematic issue is with the capital project dry storage capability at the Savannah River Site is, what we are doing today which is doing technology development work on aluminum-clad spent nuclear fuel that we are talking about today…one of the issues we have had for many years, can aluminum-clad fuel be safely dried and stored for long-term storage? and that was one of the reasons small technical issue that is still holding that up and these Technology Development work that we have been doing for last several years and we'll be talking about later this afternoon kind of indicates and gives us some better results on how best to store aluminum-clad fuel. Because dry-storage of aluminum-clad fuel has been in question. So there is that one small technical issue, beside that of course any new capital project, as John mentioned, can be very troublesome and expensive, when we did a cost estimate about ten years ago or so it was over a billion dollar for a dry-storage capability. I am not sure what the cost estimate is today. So, it is an expensive proposition to implement.

>>JEAN BAHR: Thanks for that clarification, Hitesh. I see a hand raised by Staff member Dan Ogg, I see we have about four minutes left in this, so I'm going to call on Dan, if there is any other Staff or Board members who have an urgent

question, get your hand up now so I can get you, get you on if we have time. Dan?

>>DAN OGG: Thank you, thanks John for the presentation. Um I think this is for Hitesh or maybe Mark Senderling, so on this decision regarding the Accelerated Basin De-inventory project I assume that requires some documentation for example maybe an amended ROD for EIS at SRS, can you confirm that and can you tell me where that is in the approval process, do you have an estimate of when that may be approved?

>>HITESH NIGAM: Yeah, I can answer that, Mark can correct me if I'm wrong. Yes, we are, we have prepared one, we have prepared a supplement analysis and amended Record of Decision, which will amend the decision that we made in 2013 which was to process limited amount of fuel. Now we're proposing to process all the fuel, including non-aluminum-clad fuel it is not approved yet because the Department has not made a decision and we're not sure. We're still waiting for management decision on what they want to do, once they make a decision that this is a best decision for the department will probably be approved by EM-1 assistant secretary for Environmental Management.

>>DAN OGG: Do you have any sense of the timing on that?

>>MARK SENDERLING: This is Mark. Hitesh and John are exactly right, this is under evaluation, no decisions have been made on it yet. We are getting as far as along the process as we possibly can and waiting for that decision. I don't want to provide any schedules, but, you know, we are actively looking at it. Thank you.

>>DAN OGG: All right, thank you.

>>JEAN BAHR: Okay. Any other questions um I don't see any hands raised, so thank you John and Mark and Hitesh also who contributed to the answers to the questions and I think we'll now move on to our next speaker who is Josh Jarrell from Idaho National Lab who is going to review the research program on extended dry storage of aluminum-clad spent nuclear fuel. So, if we can get um Josh on to the screen.

>>JOSH JARRELL: Can you hear me and see my slides?

>>JEAN BAHR: We can, okay. I am now going to go away and let you, let you take over Josh, thanks.

>>JOSH JARRELL: Thanks for inviting me to present on this topic um again my name is Josh Jarrell I'm the manager of Used Fuel Management Group at Idaho National Lab, also program manager for this activity. I've taken over that role from Mike Connelly who did present at the Board ..kind of the initial set of results on extended storage of aluminum-clad fuel back in 2019, so really excited to kind of tie the project together, give an overview um, we do have several of our PI’s talking the rest of the afternoon, so I'll try not to steal any of their thunder but also give an overview as requested, of how the program ties in. So, let's just start with, let's see if I can move this thing forward. The program in general as Dr. Schultz mentioned, this is a congressionally directed activity, each year the $5 million mark, which is what it has been historically and we expect to continue to be, is directed and we Idaho National Laboratory worked with Savannah River and both EM offices to define that scope each year.

The idea is really to look at spent fuel complex wide with that idea that we want to reduce risk and uncertainties and inform decision making. Now Idaho is the project lead for that, so we definitely want to focus on some of the Idaho issues but again the idea is the whole program is looking to benefit the entire spent fuel complex. Also, I'll just note that we do, when appropriate coordinate with NE on some of their spent fuel activities, and I wear an NE hat occasionally as well. And out of that earmark we focus on kind of three main areas, again sometimes we add areas depending on each year, but really the underlying goal is to reduce the risk and uncertainties associated with, with planning and management of spent fuel. And the two big areas spent fuel that we have looked at so far sodium-bonded fuels and aluminum-clad spent fuel. So, I think at least half the budget over the last four years have been focused on aluminum-clad spent fuel are, if not more. So happy to share some of the work we have done over the last few years.

So, you know, the reason we focus on aluminum spent fuel is it is different than traditional, commercial spent fuel... the main that really comes into question about extended storage transport um is Dr. Turinsky brought up earlier, disposal is its susceptibility to corrosion. So aluminum during in-reactor operations as well post-discharge, develops corrosion layers and these corrosions layers are actually oxyhydroxide layers which essentially means there is water bound in those corrosion layers and um when you start thinking about how material will perform over the long-term, there is a concern the water will be released in some form, several decades down the road, so when you think about how you predict and safely management you need to make sure you include what that looks like and so one of the big areas um is that the water can actually break apart in radiation fields and release hydrogen so, when we talk about radiolytic gas generation, that's one of the big questions.

And then the other question maybe goes back to the structural question brought up already is how does the corrosion degrade away, how does it perform, does it change in material properties?... all very important things. So the aluminum fuel, because aluminum is generally I would say just less robust than zirconium there are some questions that have came up, both with the Board brought up in 2017 report and was identified by quote/unquote DOE action plan from 2017 as well.

The other big piece, again this was brought up already today, aluminum-clad fuel is mostly HEU, highly-enriched uranium, versus the commercial fuel, which is less than 5% enriched that makes it even more important to be able to understand the geometry and minimize geometry changes that may lead to situations where subcriticality cannot be ensured. So that’s also very important when we think about making sure that we effectively predict and manage the material.

The last big thing about aluminum fuels versus the power zirc-clad materials is the diversity. Aluminum fuels are for a range of research and test reactors, production reactors and have been deployed domestically but also at foreign reactors and so basically, you know, not every fuel looks the same, not every fuel had the same um in-reactor operations, not every fuel had out-of-reactor storage as well. That's when we have really noticed is, you know, domestically with the Advanced Test Reactor, which is at Idaho which is aluminum-clad reactor as well the High Flux

Isotope Reactor, HFIR at Oak Ridge, that material once discharged has been managed very closely, pH in the pools has been maintained, it has been very well watched I guess what I would say. That's not always been the case for material that we recover from foreign reactors and so we need to understand that the aluminum-clad fuel really is a much more diverse set when compared to some of the commercial zirc-clad material.

And in addition to the diversity, just like the rest of the fuel um we're storing the material for longer than we initially expected. So, I think there was a brief conversation on what the fuel is but again normally most of the fuel is Idaho, Savannah River or Hanford. The vast majority at Idaho is in dry-storage. We still receive some material from ATR, generated each year. Savannah River the vast majority is in wet storage and they continue to receive additional aluminum-clad spent fuel from Oak Ridge, from HFIR as well as other foreign and domestic research reactors as appropriate.

And then Hanford has aluminum fuel basically all in dry-storage and they are not continuing to receive aluminum fuel. And because we're managing it for longer and longer we need to make sure that we don't have any, any safety concerns, effectively manage it. And so this whole program was led out of the material at Hanford though in a sealed system in a facility that really design life was through 2040, at Idaho it’s in a vented system, a little bit different, but it’s design life was through 2035 and again we need to think about managing this material just like commercial fuel, just like non-aluminum, non-commercial fuel for much longer than we anticipated and so this whole project was developed to make sure that we… we could adequately defend the storage approaches for this aluminum-clad fuel.

So, this slide was actually presented back in 2019 by my predecessor, Dr. Connelly and it just really describes the fact that this program was a multistep program and we're going through it um step by step. So, step one, identify the problem, that's that 2017 report, the action plan. And it really identified basically five, key technology gaps. And over the last few years we have been addressing those gaps. So, we have been doing laboratory studies to close those gaps. With the idea that we want to develop this preliminary technical basis for dry-storage. So as far as in step two, laboratory studies, most of the PIs will talk about the work that they have been doing and that's in step two. Drawing studies, radiolytic gas generation studies, we also spent a lot of time in understanding what real spent fuel was, um what it looked like and then develop surrogate materials, basically non-radioactive materials that we could use for testing because of the cost, costliness of doing full-scale spent fuel testing and then we're moving today from step two, which is what we're going to talk about today, to step three, which is more the validation, verification step. And so Dr. Evans Kitcher will talk at the end of the program on some of the instrumentation that we're developing for future basically characterization, instrumentation. And we're moving into this phase where we're pretty confident with our technical basis and we’re just looking to strengthen that with sort of the, what we're calling V and V to move forward. So um let me just speak to the five technical gaps and the tasks that were identified and the tasks we have been doing to close those gaps.

The first one was looking at how those corrosion layers, those oxyhydroxides perform in dry-storage without radiation. So, task one is no radiation effects, just how, are those corrosion products, those layers going to change in some way to release hydrogen without radiation. That work was led by Dr. Ted Lister and is pretty much complete, so I’ll actually give the summary we provided the Board a whole list of reports on each of these tasks.

Task two is… how does radiation impact those corrosion layers?, again hydrogen is released in radiation fields because of the water trapped in those layers so Dr. Greg Horne will talk all about that radiolytic gas generation, that's Task 2. The third question was, can we actually model both vented and sealed systems? I call that the modeling and sim task so Dr. Abboud will talk about the work we have been doing with modeling and sim. And then the next one was drying. Again these match up very well you’ll note with some of the NWTRB recommendations from 2017, drying being a key one. Rebecca Smith will talk about the drying in aluminum-clad fuel. The last gap was trying to really understand what the real fuel has been doing and so we actually have two separate tasks here um one in Idaho which was task four looking at ATR fuel, one was at Savannah River which was task 6 looking at the fuel there. It also in this area we developed the surrogate materials and then we kind of transition to a um, I don't want to give away Dr. d'Entremont key activities here, but looking at basically in situ monitoring of some of the fuel so we can get real-time measurements. So those are kind of some of the gaps, these are how the tasks filled in those gaps.

So let me dive in to key results and I don't, I won't give the results for the ones we're presenting today but Task 1 was already complete, it is not on the docket. And basically it was looking at, in a just regular environment, no radiation, how do these corrosion layers these oxyhydroxides change? Bottom line, in the bottom left you see some weight change, it was very, very, very minimal sort of weight changes in basically a range of environments that we analyzed. And so we really don't believe that we will see a significant amount of hydrogen production outside of the radiation field, um, generate, outside of the radiolytic gas generation, from the radiation fields, … and so we basically closed out this task, there is again quite a lot of characterization and work that was done but the bottom line here is we don't think it was much of an issue.

Having said that, we do see a lot of radiolytic gas generation and we need to make sure we quantify that. So that was Task 2 and again I won't steal Dr. Horn's thunder here, but quite a bit of work going on both in Idaho and at Savannah River trying to quantify both the basically quantify the hydrogen generation from these representative fuel um, the representative surrogate materials as well actually getting real results from real fuels that are undergoing radiation.

Move on to task 3, again won't give you any results here but Dr. Abboud will be talking about modeling and sim. I guess what I would highlight is that the development work for the modeling and sim tool, the software is really unique and novel, it was really a new coupling of, computational fluid dynamic models with reactive chemistry including some dose effects, so it was a really unique way to put together the tools to basically be able to predict future performance and coordinate that with all the data that we are getting from all the other tasks, so we did Dr. Abboud developed both sealed systems which is the picture on the far left, as well vented systems the CPP603 facility models, I think today he will talk about the sealed systems mainly again those reports were provided to the Board beforehand.

Task 5, this is the drying work. Rebecca Smith will be talking about that later today, and the idea again based on Board’s recommendation as well as the action plan from DOE from 2017 we need to think about drying specifically looking at elevated temperatures and so she will talk about the work that we have been doing both with vacuum drying as well as forced gas dehydration.

Let's see here, the next task is understanding of real fuel, so task 4 was complete, it was led by Phil Winston at Idaho National Lab where we collected ATR material, in boxes which actually didn't have fuel in them but were in the reactor and attached cut off after in-reactor performance as well we pulled material out of dry-storage from the 603 facility and basically scrape the surface to understand what those corrosion layers were. We were able to do a lot of, all sorts of characterization to understand what that material is um for the most part it was boehmite, sorry the different oxyhydroxide corrosion layers different sorts of types mainly we expect to see boehmite, bayerite and gibbsite. We saw all of those for, majority boehmite. The thicknesses were very thin, 2-6 microns, really thick, sorry, really thin corrosion layers there. And this is actually both expected and very supportive of the ATR approach, so ATR actually pre-boehmites their elements, so basically what that means is intentionally creating a boehmite film before it goes into the reactor to inhibit other corrosion growth both in reactor and then in wet and dry-storage and so basically what we saw is anything that was pre-boehmited really didn't have any additional growth. The only time we saw additional corrosion from this material was in situations where we had scraped off the boehmite either accidently from where we used a pick or something to pick up or move the fuel, so those were the spots where we saw a little bit of additional corrosion, but overall we pulled fuel that we had never really pulled out of dry-storage, 20 years in dry-storage and it looked really I would say almost the way we, you know, pristine basically what we would expect and so that, that report was provided to the Board as well.

As far as Savannah River goes and looking at the material there we did pull material out of L basin… again, we saw a mixture of boehmite, bayerite and gibbsite. We did see some thicker thicknesses, again it comes down to diversity of this material. It wasn't all pristinely um kept in wet storage after the fact and so we did see quite a bit of differences both the thicknesses of the corrosion layers as well the different types boehmite, bayerite and gibbsite, they all had different makeups in given fuel we actually saw mixed layers so that was very interesting. Somewhat expected again, given the diversity, but we did pull several different fuel types at Savannah River.

So that was pretty much the end of that piece of task 6. But the other piece of task 6, and this is what Dr. D’Entremont will be speaking about is basically the development of a mini canister for gas sampling of these materials. Again, I won't steal thunder here, but it is really good work and we got a lot of good results and probably continue to use this system in the future.

So that was kind of the high-level overview of all the tasks in that um what I will call laboratory studies um, you know, again we have been working on this for four years now. INL and Savannah River are very closely, been working together but we did think if we're going to really rely on this data for technical basis to load this material into dry-storage, we wanted to make sure and get independent review, back in FY20, over a year ago, we brought PNNL in to provide independent technical review of both the radiolytic gas generation and the approach that we took to calculate what we call the G-values um as well the modeling approach that we took. Again, PNNL has quite a bit of expertise in spent fuel modeling as well as basically radiochemistry and corrosion. And overall, the review was very positive, very supportive, basically modeling was reasonable, well-supported results and the approach for radiolytic gas generation would provide a defensible technical basis.

They did, as any good independent reviewer would do, provide some suggestions for activities, all of which we are taking into account and are currently wrapping up or have already completed, specifically looking at aluminum, the different types of aluminum and look at how the corrosion of the different types might impact the results. Looking at the different thicknesses of the corrosion layers, they suggested additional surface characterization basically more pretty pictures which we agreed with and so we're doing. Then more transparency in the modeling approach and again we have actually done that, clarified some of the modeling approach and that report was issued and the Board should have access to that as well.

So, this all came down to um that we have done several years of work, got independent review and we are to the point now where we are pretty confident on a couple key conclusions. One is interim storage in vented systems of aluminum-clad spent fuel we believe to be safe without any corrosion, degradation challenges, for extended dry storage. You know, more than 50 years, several decades down the road. Similarly, we really think aluminum-clad fuel can be safely placed in sealed systems, in sealed canisters without corrosion issues, without over-pressurization issues and without flammability issues and I think um Dr. Abboud will talk about some of these things. But the reason that we are so confident in these sort of conclusions is the modeling and sim we have developed we really believe we can rely on to predict performance going forward. And so one of the things that I think is really important is when we have developed the modeling capability while we focused on a few, few specific um fuels and a few specific configurations. The data we have collected from all of these other tasks really lends credence to the fact that we should be able to model lots of different fuels as well. And so we believe at this point that we have gotten with the lab studies and again I won't get into all the conclusions from the different tasks, but we really feel like dry-storage, sealed systems, dry-storage vented systems for extended storage, all right 50 + years, we're pretty confident we have enough data and at least this year kind of wrapping up enough data to provide that sort of preliminary technical basis um for this material.

So that's the end of my presentation and I will take any questions.

>>JEAN BAHR: Okay thank you very much, Josh, I um think we can go back to the um panel view now so I can see -- that

will let me see raised hands and I would like to remind people um that when you ask a question, please, or if you answer a question please make sure that your video is on because that will bring you on to the screen where we can all see each other. So, I now see Tissa with a hand raised so Tissa if you can unmute yourself and turn on your camera and welcome to the meeting. Tissa was a little bit late I believe.

>>TISSA ILLANGSEKARE: Yes, sorry I sort of misread the time. These are a good presentation. My question, the answer may come later, this is a question related to models. So, my understanding is the model for handling computation fluid dynamic and chemical models. So, the computational fluid flow, physical processes more short-term comparatively, the chemical processes very long-term. So, my question is a detailed question, is a more detailed question, may not answer now but later to come up, how do you uh look at these processes, couple these processes in a way so that make this long-term prediction with the chemical behavior coupled with the more short-term behavior of the physical processes?

>>JOSH JARRELL: Sure. So, I would say very carefully um but uh, you know, I think it’s the multi-physics, multiscale um is challenge, was very challenging, that's one, I actually don't want to ask this question because I want Alex, Dr. Abboud to answer this later. It was an novel sort of approach to deal with different time scales, different length scales you have to deal with to be able to accurately predict it. Again, one of the reasons we thought it was so important to get this independent review... the sort of coupling never really has been done to our knowledge before. And so, so the different reactive chemistry and how we reduce sets to make effective run times and all these sorts of things, I'll let Alex speak to later. But what you bring up is exactly right, that's why it was so challenging.

>>TISSA ILLANGSEKARE: Yes, so there is a follow up, you also mention this will come up in the last talk, the instrumentation, the modeling data. So, you will get, seems like you are going to get some real-time data, so the real-time data has to go into a model so I'm little familiar with this subject. So, one of the issues is that when you have this real-time data come to a model, the model efficiency

becomes a problem, so I'm assuming that you, you are probably looking at the issue of coupling the real-time data to the simulations.

>>JOSH JARRELL: Correct, there’s a couple different pieces of instrumentation and activities we have ongoing. One, Dr. d'Entremont will talk about, that's this -- what we call a Mini-Canister in situ, and we have Dr. Abboud will also talk about how we are modeling and planning to couple that together. Um, there’s another talk by Dr. Evans Kitcher about full scale instrumentation and that’ sort of --, we also have a different modeling and sim conversation around that. But, absolutely, what you bring up is exactly right. It's always challenging to couple experiments and models together in a defendable and efficient fashion.

>>TISSA ILLANGSEKARE: Yeah, yeah, thank you very much.

>>JEAN BAHR: Okay. Thanks, Tissa. Lee Peddicord, if you can turn on your microphone.

>>LEE PEDDICORD: Here we go. Um thanks very much Josh, very nice. Good work, I think it was sort of impressive. I wanted to come back to your noting the diversity that you're seeing in the aluminum-clad fuel and um, you know, and you pointed out you’re getting some of this return from foreign users, international reactors and so on. The question I had then is, compared to, IVER, ATR, ...is this even the history of the irradiation sufficiently well-characterized to your needs and are you seeing different or unique um mixtures of the boehmite, bayerite, and gibbsite in these fuels or maybe you don't quite know what all they have been doing since they left the US originally. And, if this could be answered later we can defer that.

>>JOSH JARRELL: No, I’ll just bring -- yeah, so yes, some of the foreign research reactors have more corrosion that we have not actually physically characterized. I mean visually you can see, I mean there are some challenges. When we evaluate the ones we have looked, we do see mixtures of these different boehmite, bayerite, gibbsite kind of things. It does bring in the question sort of what, what sort of history it has. That's the first thing. But one thing I think is really important to note, at least for the modeling and sim is, we look at a range of thicknesses with different sort of, with different sort of corrosion layers um and even

with much thicker corrosion layers the models don't appear to be, predicting, you know, significant pressurizations, flammability concerns, anything like that. Yeah, the vast majority of, I'll call it the well-behaved fuel, ATR, the HFIR, those things, we don't expect that be an issue. But, some of this I'll call it cats and dogs where we may not have as much information as we'd like. What we are trying to do with the models and the data is provide sort of the bounding approach. One of the things I think we'll talk about at some point is going forward we might want more real realistic datasets so we can try to predict that a little better. But, again, the goal for us is more of an engineering,… hey, can I load this fuel into a sealed system. And so we try to take -- add layer upon layer of conservatism so even if we might not know if it's bayerite or gibbsite and if it is 10 or 20 microns still pretty confident we're not going to have an issue.

>>LEE PEDDICORD: Okay, very good. Thank you, appreciate it.

>>JEAN BAHR: Paul Turinsky? If we can get your camera, Paul.

>>PAUL TURINSKY: Josh, what other materials are associated with the fuel other than aluminum? There's structural materials that holds things together. Some of the fuel rods are perforated so that the actual meat of the fuel, oxide, uranium oxide or whatever it may be, zirconium, uranium compounds are exposed and has there been any assessment of the impact of these other materials, non aluminum materials, from storage?

>>JOSH JARRELL: Yeah, so first I think the vast majority of these materials are aluminum-based even the structural materials are aluminum, I mean a whole reason for that, in reactor operations you like the aluminums, thermal reasons -- a whole bunch of things from this perspective. I don't think there is a lot of other sort of non aluminum materials in the fuel elements. I mean you don't have, you know, integral poison rods like you would in the commercial space. So generally they're aluminum. I haven't, we haven't looked at the fuel meat still just aluminum-uranium fuel meat for most of the stuff if it is exposed, we haven't looked at that. I think it's probably -- I would expect similar performance maybe I'll just defer maybe we haven't thought about all the kind of one-offs in those areas. I

think this work really is the vast majority of the aluminum fuels and I think we kind of bound that.

>>LEE PEDDICORD: Okay, thank you.

>>JEAN BAHR: I see Dan Ogg’s hand raised.

>>DAN OGG: Yes, Josh I’ve got a little more technical question for you, since you are presenting task number 1, um and this spills over into the work that Greg Horne is doing on gas generation, but in your slide, slide number 7, you say that the based on task number 1 there is no indication of hydrogen generation from the corrosion process. But, in Greg Horne's slides and some reference there is there is a discussion of some level of hydrogen generation from the corrosion process and even the possibly of consumption or -- or depletion of the oxide layer. Can you explain that a little bit or if you need to pass it off to --

>>JOSH JARRELL: I would have to defer, defer to Greg's slides um --

>>DAN OGG: We can cover it when Greg gets to his presentation. We just saw it to be a little bit of a difference in results between those two. Just trying to understand.

>>JOSH JARRELL: Yeah, so um if Greg doesn't cover it, I'll make a note to circle back with the Board maybe offline or others to get into that space. It looks like Greg -- Greg raised his hand so he might be volunteering to save me.

>>GREG HORNE: Well I don't know, let’s not get too ahead of ourselves. But, now, I think Dan, what I think what I specifically refer to is radiation induced contributions from the oxide layer. And, I believe, unless I heard incorrectly, what Josh has said regarding Task 1 is just, if you have the corrosion layer sat there you just leave it out on your garden, is when hydrogen is going to come from it. The answer is, no, from what we found. You need a radiation driver for that to contribute. Does that make sense?

>>DAN OGG: Yeah, I think, it is just that we saw the language in many of the reports, it says “consumption of the oxide layer” and I think that's what threw us off I guess

what you are meaning really is consumption of the water that's in the oxide layer.

>>GREG HORNE: Well, it's both, that will be brought up in my slides. I don’t want to diverge. It’s, like you have water absorbed, absorbed on to the surface, but you also have oxyhydroxide layer, both of which can be radiolytically consumed. I'll show that later, and I’m sorry if there’s confusion there. Definitely, without radiation your corrosion layer is not contributing to hydrogen production.

>>DAN OGG: Okay.

>>JEAN BAHR: Okay, um, I see Dave Shoesmith’s hand up, I'm going to defer to him, I know Bret has a question as well but I want to try to get Dave in.

>>DAVID SHOESMITH: Again, this may be more appropriate to come later, but based on that last discussion, what state does the drying process leave the surface in? Are you potentially susceptible to more corrosion because you got rid of the boehmite or you’ve at least got rid of the water in it, and therefore it’s nowhere near as protective and, if you then subsequently have an aqueous exposure of some kind that the underlining aluminum is able to produce hydrogen. Do you know that is the case or not the case, rather?

>>JOSH JARRELL: Um I'd have to think about that a little bit more um --

>>DAVID SHOESMITH: It may be appropriate to question it later.

>>JOSH JARRELL: Maybe, but one thing I will note, um, it looks like Bob Sindelar may want to jump in on this one, one thing we want to note is, we focused on extended dry storage of this material. That sort of scenario, I think you’re describing may be a lot after extended dry storage, may be a disposal sort of scenario and that may be something we haven't thought about as much. Bob Sindelar has his hand raised. Do you want to add something Bob.

>>BOB SINDELER: Yeah can you hear me?

>>JEAN BAHR: We can hear you.

>>JOSH JARRELL: Yes, Bob, we can hear you.

>>BOB SINDELER: Okay, so Josh gave the right answer, Greg gave the right answer, I'm just going to underscore and repeat it. The -- when you have the existing oxide on there, if you have attendant humidity; this was the Task 1, Ted Lister studies if you have humidity over already corroded layer, you’re not going to cause any more corrosion, and therefore hydrogen generation. Now I think, was this Paul's question that when you dry. Well when we dry our best understanding to date is we are not going to decompose the boehmite during our drying. This is part of the details now but your mind’s going to have to be here sooner or later, so you are not going to decompose the boehmite what we are doing in drying getting rid of physisorbed and converting some of the trihydrated oxides, the boehmite, gibbsite, so we’re losing some of that. So the protective nature of oxides is really that boehmite, when that's on there that's the thickest oxyhydroxide type and no more corrosion, that's why ATR pre-filmed. You form that on there and what they’re compelling results in this test show that after even additional storage and discharged pool of ATR, no more corrosion. But what we are affecting is getting rid of physisorbed and then the tryhydride oxide. So the question, well, I still have my same question. What about, is that more susceptible to um corrosion from just um remaining humidity? Um, sure. It may be, but it's going to be so minor when you look at this the overriding concern, and this is what was modeled, tested by Greg Horne, modeled by Alex, is that there’s a lot of water in the hydrated oxide layer and that's your source of hydrogen. The amount of hydrogen that can come from little bit of residual of humidity is minor. So hopefully that gives some clarification to the set of questions and if not we can carry this on offline later.

>>JEAN BAHR: Okay, thank you. And that was Bob Sindelar in case we need that for the record. Bret Leslie has a question and this will be the last one because we will need to move on to the next speaker.

>>BRET LESLIE: Thank you Jean, I have to go back to something Paul stated and this deals with conclusions that Josh has made. In our report back in 2017 said because the DOE standard canister is a multipurpose canister in safety

for transportation as a decision by NRC, while storage at the site is a decision by DOE, how do you know that it is going to be road ready? In other words, if you package without certification or approval from NRC, will it truly be road ready? Thank you.

>>JOSH JARRELL: Yeah [LAUGHTER] you know, I would say that is um outside the scope of aluminum-clad, of this sort of talk, but um I will just kind of weigh-in on road ready and the DOE standard canister since I think, uh Dr. Peddicord also brought this question up, maybe it was Dr. Turinsky. But historically DOE stance is that the DOE canister will be what the aluminum-clad fuel will be placed in, and that will be placed in the transportation package. DOE has had conversations with the NRC as recently as I think 2007 um in conversations on the DOE standard canister and what could be credited in transportation space. And I would agree that with the definition of road ready would involve a regulator saying basically that they would be able to um transport this material down the road in a transportation cask. So I don't think, I would be very reticent for DOE to load material and claim it’s road ready without having some agreement with the regulator, the transportation regulator in this case, um that what they loaded would be approved, whether that was a formal Part 71 COC or not, you know, I think that's debatable but road readiness definitely is defined by the regulator that will provide the transportation approval. And so, you know, I would just be very careful when we say road ready.. that includes that some sort of agreement with that regulator.

>>JEAN BAHR: Okay. Thanks Josh, I think we need to move on to our next speaker now and that's going to be Rebecca Smith from Idaho National Laboratory and she is going to be talking about the drying process of aluminum-clad spent fuel surrogates. So if we can get Rebecca on camera and get her slides cued up. And I'm still seeing Bob Sindelar's phone icon on my screen, I'm not sure if -- he’s, I see Rebecca, but I'm going to un--, let's see. Oops.

>>REBECCA SMITH: Thanks for the opportunity.

>>JEAN BAHR: There we go, okay thank you now that you are here I'm going to leave.

>>REBECCA SMITH: Okay. Thank you for the introduction and the opportunity to talk to you here today. I'm Rebecca Smith, I've worked for the National Laboratory here in Idaho for about 30 years now. Primarily in the areas of spent fuel disposition and materials performance. I'm here today as the INL task lead and principal investigator for the drying experiment where we looked at drying aluminum spent fuel surrogates. This was a collaborative effort with INL, Savannah River National Laboratory provided us with some review and consulting advice, and Holtec International and University of South Carolina were also key contributors to this effort.

You can see here in this image, the drying vessel…give you a sense of scale. This is an engineering-scale experiment. So, you can see the insulated drying vessel modeled after the DOE standard canister here in the image. I’ll start with kind of a road map of where I am going with the presentation, talk a little about the purpose of the experiment, the design, and some of the broad roles of the contributing institutions, continue with some process observations and how that corresponds with the simulations from the models. We'll look at an assessment of the relative effectiveness of the drying processes themselves. Talk a little bit about proposed follow on work and end with the summary conclusions.

So um for objectives as Josh indicated we're looking at extended dry storage and the concerns being vented vs sealed storage for drying, the real issue is if you are going to seal it up, do you have confidence that you removed enough moisture to do that without causing pressurization or other radiolytic problems. So we wanted to do some experimental work to try to understand how successful these drying processes are. We're looking specifically at the removal of bulk water, but also considering physisorbed and chemisorbed water. Industrial processes have been removing bulk water for quite some time. The chemisorbed water is little more challenging to really understand how effective drying process is on it. We're specifically comparing the two major nuclear processes for drying, the forced helium dehydration and the vacuum processes. So our collaboration, INL was primarily responsible for the planning and coordination, we produced the chemistry surrogate and shipped it off to New Jersey and to the University of South

Carolina folks for them to conduct the experiment and do the analysis. University of South Carolina was instrumental in developing the experiment design, specifically instrumentation and analysis techniques associated with the experiment. They provided objective oversight of the testing on the grounds at Holtec. Holtec provided the fabrication work for all of the major components. We used their training facility for their forced helium dehydration process, for the location and the operation of the actual drying experiment. And they were gracious in providing their, the benefit of their industrial experience with drying.

If you look at the images here you can see the unjacketed vessel, the um DOE standard canister was the basic dimensions for it and we were looking specifically at a single full sized, fully loaded type 1A basket. Between a third and a half height of the standard canister with enough head space to accommodate the instrumentation and so forth for to accommodate that above the basket itself. You can see the viewing ports, you can see the um vacuum connections and so forth and we had the ability to do optical spectroscopy through the view ports. You can see looking down into the vessel there is the basket that allows for the loading of ten assemblies into the single basket load. The basket itself was the Type 1A style basket that needed to be amended to have improved mass transfer. They included a false bottom to the basket design. You can see the basket sleeve here.

So, the experiment concept is basically to take the geometry and model that as closely as we can for the details of the fuel and the drying vessel environment and also to use a chemistry surrogate where we grew a corrosion layer on plates that could be inserted into the geometry surrogate so that we would get a more realistic understanding of how the processes actually affect that corrosion layer. The 12-inch corrosion surrogate plates were immersed in a vessel and the corrosion layers grown as I indicated in Idaho….The geometry surrogates of the fuel assemblies can be seen here being disassembled so that the plates after one of the experiments were packaged off and shipped off for analysis. One of these plates or one of these assemblies, assembly number 4 has the capacity for simulated decay heat. There is a custom resistance heater inserted in between several of the plates here, substituting out some of the full length approximately three-foot-long plates within the assembly. That provided simulated decay heat at that assembly location, so we looked at that and its influence on the thermal properties within the drying canister during the drying processes. We added a known amount of bulk water so that we could see basically all of the moisture removal capability in a controlled manner for the processes tested or process parameters tested.

The overall layout of the assemblies within the basket, the assemblies are identified by number so you'll see this particular schematic come up rather a lot. The ones with the red elements here um those red plates, the full length plate has been replaced with a number of the chemistry surrogate plates which are shorter. There were cases where there was some,… how do I want to say…, blanks that were used so that not all of the um full lengths were chemistry surrogate, but it gave us a good, good control and a good distribution of information for the performance of the oxide layer in the range of um experiment drying parameters tested.

So I talk about modeling in two different aspects within this presentation and I want to try and offer a little bit of clarification…. Alex Abboud was involved both with advisory capacity and the modeling for the drying processes, and both the forced helium dehydration and the vacuum drying processes were modeled so there are models from a drying process perspective and that's in addition to the sealed and vented dry-storage models that Josh Jarrell introduced earlier. So from here we had a lot of thermocouples and instrumentation that were allowing us to track the performance during the test and I'll move from this discussion of the overall experimental design into the actual real-time data from the drying processes.

So looking at the thermal performance first, you can see on the top, the assembly average data for a couple of representative tests, one forced helium dehydration test and one vacuum drying test. You can see that the forced helium dehydration the model input or model simulation data on the bottom both give a nice tight representation of the thermal behavior of the assemblies. The vacuum drying test, the model captures the extremes fairly well, but doesn't necessarily pick up on some of the subtleties of the behavior. You see the heated assembly, assembly 4, the internal and external temperature profiles are um, they are the highest but there is a bigger gap between them than we might have expected from the model. Another thing that might be of interest is that assembly 1 is very closely over written by the assembly 10 external performance, and for the vacuum testing that makes sense because they are symmetrically-oriented. Most of my data is specific to assemblies 1, 4, 7 and 10 and those are the assemblies that housed the chemistry surrogate, so we have the best information in terms of the overall performance for those assemblies and again you'll see this layout come up repeatedly to help follow along where the positions of these assemblies are with respect to the syphon line, which is down here and the vent line which is over here.

So, the modeling and tracking of mass transfer is a little bit easier or is a little bit more difficult than the temperature, than the heat transfer um but we did look at the humidity data to compare that between the actual observed experimental data and the model. And what you see on the left is a combination of the relative humidity data in blue for these same two representative forced helium dehydration and vacuum drying tests. And in red there is the model, the simulation results for the parallel set of drying parameters. On the right you see the model output for the individual assemblies for both of those conditions. So, this red line is basically the average of the lines over here. And you can see with the vacuum drying testing um the humidity data does vary with the cyclical pressure holds, vacuum pressure holds, the model is not refined enough to capture that level of granularity so the red line really just shows an empirical average for the behavior within the drying vessel.

So, we looked not only at the bulk water which is represented pretty well with the humidity data and the modeling data here, but we also consider physisorbed and chemisorbed water and here you can see basically the model simulation for the behavior at these assemblies in the last um, as the last 10 or 20 grams of chemisorbed water is removed in the course of the drying processes. Again there is some overlap between assembly 1 and assembly 10 performance and you get the indication with the modeling results that the forced helium dehydration is a bit more effective at removing the chemisorbed water, that is empirically based on our other small sample data and this is based on TGA work so um I'm going to talk a little bit about the post-experimental TGA analysis now to kind of get, move from the process and simulation data into the um effectiveness of relative drying processes. The thermogravimetric analysis used a small section of the chemistry surrogate plates, you can see here that's one of the specimens is cut away in this manner. Those specimens are about 14 millimeters by 15 millimeters so they are not very large and you can see one here loaded for ready for introduction into the, into the TGA.

The TGA basically gave us weight change results relative to a control so the important thing to understand here is each batch of corrosion plates, each batch of our chemistry surrogate has a control sample that was not processed through the, either of the drying processes under any of the specific parameters tested. But it was used as a baseline, as a control to understand how much water is associated typically chemisorbed moisture without active drying. The data here, these numbers are indicated by 1 minus the surface area-normalized TGA results from the specimen that's been dried divided by the specimen that, or the average of the control specimens. So, you are getting a sense here of how the overall drying processes, how effective they are at removing that chemisorbed moisture. The TGA analysis was able to take these specimens over a controlled, much larger controlled temperature range to effectively remove all of the moisture from the chemisorbed layer.

This first graph indicates the overall behavior of these tests with time. There is sort of intuitive sense that if you run a drying process for longer that you may be able to remove more chemisorbed moisture, but there is not really that strong of a correlation here. There is actually a relatively weak time correlation for the drying processes with the removal of chemisorbed moisture. So you get a sense in this image to the right where we show the same behavior relative to the control on the basis of the maximum temperature achieved at that assembly corrosion plate location and you see essentially a cluster of the vacuum drying results here and a cluster of the forced helium dehydration results here and there is a very tight correlation between the ability to remove that chemisorbed moisture with the maximum temperature that, that particular assembly location sees during the drying test. This is particularly evident in forced helium dehydration test 9 where you have a single test where the assemblies saw a range of maximum temperature exposure and there is notably more chemisorbed moisture liberated when it gets up close to or above 220°C.

We also looked at the behavior with scanning electron microscopy and we could see a change in the morphology of the crystal structures as the sample achieved that maximum temperature approaching 220°C. So, the image on the left, all of the specimens both from the undried controls and the drying material or drying surrogate material chemistry surrogate that did not achieve 220°C, it looked with, looked like this one on the left with more distinct crystal structure and those crystals fuse together once they were, once they were liberating more of the chemisorbed moisture.

So, here's um kind of an indication of some of the issues that we might want to, that we might want to address with follow-on work. I would say I think that these results give us a bit more confidence that we can actively dry the chemisorbed moisture with a forced helium process and whether or not we want to pursue these items of work really depends on how we want to use the model. There are some things within our experimental system that may have been less than optimal. The siphon tube location wound up mismatched with the port that it needed to, needed to join with and so a more optimal siphon tube location would likely serve to improve the drying results a little bit. The vacuum drying model could certainly be refined a little bit. Holtec has a secondary chilling process that was not used for this experimental work. It's intended to dry the helium cover gas for when they actually seal their post-drying operations and since we were handling our surrogate materials in air after the drying process it really didn't seem to make much sense to try and use the secondary chiller and also, we were looking at a specific test matrix to identify the response with the test duration and the temperatures and so forth. So, using that chiller really was kind of counter-indicated given our test objectives. The vacuum drying is disadvantaged because of the heat transfer, lack of heat transfer in a vacuum, relatively speaking. So, the forced helium dehydration certainly provides a more effective means of removing the chemisorbed moisture. One of the things that might be improved in terms of vacuum drying performance would be to improve the heat supply to the vessel wall. Some of the vacuum drying tests used spacer disk and bulk water trays to enable them to image whether the liquid bulk water was boiling or freezing and at what point in the drying process that was happening. So, there would be some benefit to including that in the model and possibly doing some additional work with the forced helium dehydration to account for that as well. Although, the forced helium dehydration simulations did give us closer representation of the behavior during the real-time observations. One of the other things that we have talked about is modeling asymmetrical loading and that is basically the model is assuming that the assemblies are symmetrically loaded within the basket and variations in that could certainly affect the temperature profile particularly when you are looking at the decay heat contributions and relative behavior of the chemisorbed moisture removal. Additional validation work would narrow differences between the models and the experiments and might also be in, ...might also be beneficial to have validation work that's more independent of the model development, some of our model development really followed the experimental evidence rather than the other way around to be able to use the model as, as a tool for projection of the behavior it would be good to have some independent validation.

And if we were to actually pursue all of the above, that would probably be nominal two-year effort to address those issues. And I don't know that I would necessarily call them issues, I do think that we can certainly engineer around what we have currently and it's very much a question of how do we want to use the models to improve our confidence in our actual dry-storage behavior.

In conclusion, removal of the chemisorbed water is insensitive to the drying duration, relatively speaking, neither the forced helium dehydration nor the vacuum drying TGA results correlate particularly closely with the run durations tested. I interpret this to mean that if you run the drying test or the drying operation until the bulk liquid is removed that you will have whatever level of chemisorb moisture remaining you are likely to see regardless assuming the temperatures at that end point have allowed those independent, those individual assemblies to achieve that maximum temperature that they equilibrate to during the drying run.

The removal of chemisorb water is very sensitive to temperature, particularly approaching 220°C and so if it's important to remove the maximum amount of chemisorb water, forced helium dehydration is certainly more effective at removing it because of the improved heat transfer with the helium gas.

And of course, as I just talked about in the previous slide the process drying models could be improved to provide better predictive data, there are some opportunities to tune up some of the details there and give us a,.. a clearer understanding of how close our models can get to the output. I think that if we're really looking for bounding data, I think we're probably pretty close to there, if we want to use models in more predictive manner, then there is probably more work worth doing.

This work has been completed with many partners and I do want to recognize several of the most significant individual contributors, professor Travis Knight was Principal Investigator at University of South Carolina and he was most helpful in assisting with the experiment design and with advising his students that helped with the modeling as well as the conduct of the experiment and the analysis. Nate Cooper was our initial student with the main point of contact. He developed the forced helium dehydration model. Robert Demuth handled the SEM and TGA analysis. And Jonathan Perry was our point man for the conduct of the experiment, vacuum drying model, development from the previously developed forced helium dehydration model and also the simulation for validation of the models. Holtec Government Services, Nick Parisi was our Project Manager on ground in New Jersey and Garrick Stafford was our engineering support; both were quite helpful in getting the experiment conducted and keeping everything moved forward. Garrick answered many, many questions for us. We certainly appreciated their participation. Here at INL, as I mentioned before Alex Abboud was our modeling consultant and

Tim Yoder handled the production of the chemical surrogates. Review and consultation was much appreciated by Bob Sindelar and Anna d'Entremont at Savannah River laboratory. We have a number of publications associated with this work and they are listed here for future reference. We do have one journal article that is still in progress, so there may be more to come in this regard. And with that I'll take any questions.

>>JEAN BAHR: Okay, thank you very much Rebecca. Very interesting presentation. I have a question for clarification in the modeling of the drying behavior, you compared data on relative humidity, but the model generates remaining bulk water. Is there a one-to-one correspondence between those --

>>REBECCA SMITH: So, the correspondence is when you are looking at the sum of the parts. This red line reflects the cumulative residual bulk water. That is a pretty good consistent measure against the, against the humidity but it's basically benchmarking the relative humidity against the amount of bulk water remaining in the vessel at that point in time. Does that make sense?

>>JEAN BAHR: Yeah, I guess it is just are you, are you really comparing apples to apples or are they sort of surrogates, is there a way for the model to actually generate output that is what you measure? Or do you have to --

>>REBECCA SMITH: This is as close as we could come.

>>JEAN BAHR: Uh-huh.

>>REBECCA SMITH: Again, there is certainly… are places where we might improve upon the model and things that we might imagine up in terms of monitoring but given the data that we have from the actual tests this was the most representative manner we could present it.

>>JEAN BAHR: Okay, thank you. Are there questions from Board members, maybe we should switch to the 'panel view' at which point I might be able to see if there are hands raised. You did such a great job that everyone, everyone is happy with the presentation. No questions.

>>REBECCA SMITH: I see a couple of hands raised though.

>>JEAN BAHR: You do, okay somehow, I'm not seeing those on my screen so let's see if I can um okay um someone says that Paul and Lee have their hands raised for some reason, I'm not seeing those but let's go to Paul. ‘Gallery view’. Uh-huh. Okay.

>>PAUL TURINSKY: Very nice presentation. Two questions, one is could you say a little bit more about the variation of the coupons, the thicknesses, compositions of the oxyhydroxides

>>REBECCA SMITH: I can get into probably unnecessarily deep level of detail on these, but I'll give you a quick run down on, on the, are you interested more in the chemistry surrogate or in terms of how the chemistry surrogate works within the geometry surrogate of the entire assembly?

>>PAUL TURINSKY: The former.

>>REBECCA SMITH: Okay so the chemistry surrogate um we had what Josh Jarrell referred to as task 6, that looked at kind of an overall how do we get a chemistry surrogate to work with that we can produce on a consistent basis and use for our test material. And so we worked with them to come up with kind of a formula and we did, they started with smaller coupons and then we were able to expand the overall immersion process to grow this surrogate on a larger scale with the 12 inch plates. What we did was effectively cultivate as large of possible, as possible a layer of oxide which turned out to be primarily bayerite and we were able to do this over about a six-week period. So those plates spent about well at least five I think um and preferably six but, in some cases, a little bit longer than six weeks immersed in a water bath and the water bath is basically a room temperature water bath and we really didn't control the chemistry tightly other than to start with nano-pure water in the first place and maintain the immersion level so that the plates did not basically wind up being unexposed at the top. So that's kind of the nuts and bolts of the corrosion --

>>PAUL TURINSKY: Yeah, how about the thickness of the plates and the morphology of the plate versus what you actually see on fuel. Because you are doing accelerated --

>>REBECCA SMITH: So, the morphology compared to the fuel what we saw, and I'm sure it depends on exactly which fuel you're talking about. I think they saw a bit more of the bayerite and maybe the gibbsite material in the examination that they did at Savannah River. The materials that they tested that they looked at from our ATR fuel both from specimens taken from end boxes at the end-of-life when the end boxes were cutoff at the ATR canal as well as specimens taken after a period of dry-storage in IFSF. And those were largely the boehmite which is, it has less of the chemisorbed moisture to begin with. I think that ultimately drying at temperatures only as high as 220°C were not going to remove that last layer of chemisorbed moisture you see in the boehmite regardless. So, I think that what we're using for a surrogate is probably conservative in the sense that if we were to see more gibbsite that moisture would be released at a lower temperature, so we would likely get more of it off. And then if it's just boehmite that's probably not going to be affected without taking the fuel to a high enough temperature that you are likely to begin to damage the aluminum in the same place.

>>PAUL TURINSKY: Related to that, if you are doing vacuum drying is there enough to gain heat in the fuel that would be packaged to dry it to the higher temperatures that you desire?

>>REBECCA SMITH: This is why the forced helium dehydration has an advantage, I would answer that question in two ways. It depends on the decay heat in the fuel in the first place.

>>PAUL TURINSKY: Yeah.

>>REBECCA SMITH: And the real problem with that is you wind up with a much less uniform distribution of temperatures so it may or may not and you have no good way to know.

>>PAUL TURINSKY: Okay so the preference I gather is going away from any preference towards vacuum drying at this point.

>>REBECCA SMITH: If you're trying to remove chemisorb moisture, so if you are going to a sealed system, I think there are real benefits to using the forced helium dehydration. If you are going to a vented system then

whether or not you have some chemisorb moisture doesn't really matter because you are not controlling that tightly at your end of processing anyway. And the vacuum drying with minimal decay heat is not going to approach that 220-value so we're probably not going to affect the chemisorbed moisture as much at all.

>>PAUL TURINSKY: Okay, thank you.

>>JEAN BAHR: Okay, Lee --

>>LEE PEDDICORD: Thank you, very nice presentation, nice work. I really enjoyed hearing about it. A couple of real quick questions on slide 6 where you are showing the specimen size for analysis there is a little notch it appears in the lower right-hand corner ..what's that?

>>REBECCA SMITH: Slide 6, let's see. 6. Notch?

>>LEE PEDDICORD: Well, I don't know there is a little square outlined in black --

>>REBECCA SMITH: Yes, so if you look here these are the bolt holes where the 12 inch chemisorbed corrosion plate was inserted into the assembly. This corner here is the specimen that was used for TGA analysis.

>>LEE PEDDICORD: Okay, okay.

>>REBECCA SMITH: So, this is the 14 millimeter by 15 millimeter specimen that was actually tested in the TGA post drying run.

>>LEE PEDDICORD: Yeah, got it. Thank you then you know you've looked in detail at these two approaches do you have a conclusion or, you know, a feeling of which was more easily implemented for the large number of canisters that the DOE is going to have to put through a drying process? Is forced helium dehydration more complicated, more costly? What's your feeling on this?

>>REBECCA SMITH: Um (sigh) you know I think it may depend on the price of helium. I do think that there are serious advantages to having the helium heat transfer medium because that does such a much better job of maintaining a uniform temperature. So, if you do have a wide distribution of

decay heats it gives you a better sense that you're getting the whole load dried more uniformly.

>>LEE PEDDICORD: Well, I was also thinking, you know, about the infrastructure you would have to set up, you know, I have really no concept of how much equipment goes into the two different approaches and I'm just wondering if also might be a cost delineator between the two?

>>REBECCA SMITH: This, I'm going to have to say we need to take a closer look at some of the radiolysis data and see if it we feel like our actual fuel inventory can be bounded with the moisture removal that's achievable with the vacuum drying.

>>LEE PEDDICORD: Yeah, this may be --

>>REBECCA SMITH: From cost perspective you may be right. It may be of benefit to go with vacuum drying and certainly if you are looking at a vented system then the helium isn't going to gain you much. But to get something that's truly road ready and have the confidence that you've removed at least a significant fraction of the chemisorbed water then the forced helium dehydration certainly offers advantages.

>>LEE PEDDICORD: Well and you are right, I mean this kind of needs an overall system look, it would seem and where you have to get to that. Kind of leads to my last question, which might be unfair and you can tell me that. Your very first sentence as we are doing these drying studies and getting the water out, and so on, so the question comes to mind how much is enough and this comes to mind because Josh talked about the diversity, the variety even went over to the foreign aluminum-clad spent fuel and do, do we, do you know what the right target is even for where you want to get to?

>>REBECCA SMITH: My personal opinion is that we're homing in on that. And I think that perhaps the radiolysis discussion will lend you some insight there.

>>LEE PEDDICORD: Thank you very much, enjoyed it, good work.

>>REBECCA SMITH: You're welcome.

>>JEAN BAHR: Thank you very much, Rebecca. I think we're right at time where we are supposed to take a 20 minute break. So it's Eastern time currently 1:32 try to reassemble at 1:50 eastern time. So we'll see you all soon and I will wait for the the moderators to tell me when I can turn my camera off without causing glitches. Thank you Rebecca. (break)

>>JEAN BAHR: Okay, well welcome back. I hope everyone had a good break, and our next speaker is going to be Greg Horne, who is going to be talking about radiolytic gas generation due to aluminum-clad spent nuclear fuel corrosion layers and we have heard a little bit about this already, so I'm looking forward it to, and it looks like Greg is on the screen, and so that means that I can mute myself and go off the screen and turn it over to Greg.

>>GREG HORNE: No worries, thank you very much. Okay. Thank you very much for the introduction, Jean, my name is Dr. Gregory Horne, and I'm the Director of the Center for Radiation Chemistry Research Idaho National Laboratory. Now, today I intend to have a chat with you all about Task 2’s contribution to understanding the radiolytic production of molecular hydrogen from corrosion layers on aluminum-clad spent nuclear fuel. Now, before we dive into nitty-gritty of the science behind all of this, what is the actual fundamental knowledge gap that we’re trying to address here, and why is it important we understand it for the safe evaluation of extended storage of our aluminum-clad spent nuclear fuel?

Now we have heard a lot already today, so I don't want to keep going over the same information that we have already shared, so there is a lot of aluminum-clad spent nuclear fuel in our inventory and a lot of it has to some extent undergone different degrees of corrosion on the surface. Now these corrosion layers are typically thermally and chemically induced and have an effect upon the integrity of the clad units. So, now because of this there has been extensive studies into thermal and chemical corrosion processes leading to these corrosion layers and their effect on the fuel itself. However, there is a significant knowledge gap on the effects of radiation on these corrosion layers, particularly with respect to the formation of molecular hydrogen gas.

Now, why do we care about molecular hydrogen gas in this particular scenario? Well besides the fact the hydrogen gas can build up and form flammable gas mixtures, it could also potentially cause over-pressurization of the standard canister design that we want to stay intact. It may also diffuse into the aluminum alloys themselves and cause embrittlement. So having a handle of fundamental understanding of corrosion processes that are responsible for the radiolytic formation of hydrogen gas, is essential for the development of predicted computer models that Dr. Alex Abboud will talk about later, which will in turn allow us to predict the formation of this gas, lifetime of extended dry-storage.

Now I talk about radiolytic hydrogen formation, well, what do I actually mean by that? How are we forming this hydrogen, so here we have a very conservative reaction set on processes responsible in this particular example for molecular hydrogen formation. Now, I don't expect any of you to remember all of these processes, this is purely to demonstrate that what was once a very simple four-component system, aluminum metal, a corrosion layer, possibly some absorbed water and a gaseous phase rapidly becomes a multicomponent system, basically a witches brew as soon as you start subjecting it to irradiation. Two up processes such as water radiolysis which stimulates a whole slew of species, it can directly generate hydrogen and it also generates these species, you can see my cursor, the hydrated electron and the hydrogen atom. These can then go on to promote subsequent chemistry. A lot of these actually results down here in the formation of more molecular hydrogen. And then we have irradiation of the bulk metal itself which leads to formation of excitons which can diffuse basically to surface of the material and induce further radiolysis which may or may not lead to hydrogen.

And then there is this question of the corrosion layers themselves. And basically, these aluminum oxyhydroxide corrosion layers are a molecular hydrogen source. You can irradiate them and generate hydrogen atoms and molecular hydrogen which has been extensively demonstrated in literature by powder studies of these mineral phases.

So, we have a lot of these mechanisms going on, so what if we know what these mechanisms are, what is our actual role here? What are we trying to provide you with?   And the way I would sum that up is, Task 2 aims to provide quantitative experimental data and insight into the radiolytic formation of molecular hydrogen as a function of a whole variety of parameters that are expected under storage conditions. And those parameters, from my perspective, are absorbed gamma dose or absorbed radiation dose, corrosion layer, you heard mention a couple of different, boehmite has been mentioned, bayerite has been mentioned, there’s also gibbsite, which we'll get to some of that later. We then have the gaseous environment, now what I mean by that is, what is the gas surrounding our fuel cladding when it’s undergoing irradiation and how does that impact hydrogen formation? And we have relative humidity of that gaseous environment. And finally, we have temperature during irradiation. How does that impact hydrogen formation? So, Task 2 aim was to address ideally all of this and provide sufficient information for the development of predictive modeling capabilities.

So how did we go about doing that? Now, we designed a very systematic and intense experimental protocol for not only providing reproducibility pre-corroded specimens but also to guarantee the gaseous phase in those samples and to provide an extensive dose range over which to evaluate our systems. So, we purchased a whole bunch of aluminum alloy 1100 coupons, these were -- you can see these in the top left-hand figure. And we irradiated basically in two batches; there was the pristine batch, when they arrived, we cleaned them up and sent for irradiation through the rest of this process, which I'll follow-up with very shortly. And then there was the pre-corroded specimens. By that what I mean is…we took half of the coupons we received, we loaded them on to this bespoke glass rack and submerged them in boiling apparatus and subjected to temperatures of about 95 degrees for about 29 days. And, that's how the formula was that was provided to us by Task 1 who had done this thermal evaluation on how much they could grow a corrosion layer. So, this guaranteed us, within reason, guaranteed is a five-ish micron corrosion layer, which is representative of some of the fuels that are in our inventory. We would then take pre-corroded and the pristine samples, load them into these glass tubes that you can see here and connect them to a schlenk line. Once attached to this schlenk line we can manipulate gaseous environment that eventually locked into these samples. So that would be air or ambient air, nitrogen, argon, and helium, helium being the proposed gas for backfilling these canisters. Now, once we’ve got our gas in there, we then wanted to dictate or fix the actual humidity which we did, and we investigated 0%, 50% and 100% relative humidity. Once all of these system conditions were fixed, we then flame sealed them with a propane torch giving us these flame sealed glass vials loaded with ampules. We also have baseline controls with no ampules to guarantee that the amount of hydrogen we were measuring was not some artifact from say the humidity in the air. We will pitch them out later. We then took these sample vials and then subjected them to gamma irradiation using a couple of different irradiators. The one you see here is our new [inaudible] services gamma irradiator. Regardless of the gamma irradiator used, the procedure was exactly the same. Basically, take your samples, you put them in you irradiator chamber, you click ‘go’, you subject them to an intense gamma irradiation field for a given length of time which corresponds to a specified dose, and then you retrieve them for analysis. Now, the majority of analysis, headspace analysis because we were trying to determine how much hydrogen we're forming that was achieving gas chromatography of which you can see one of the instruments to the top right here and typical calibration chromatograms in the bottom right. We also did some surface characterization techniques using scanning electron microscopy, which I will refer to as SEM from this point onwards and x-ray defractometry to get an idea of the composition and the properties of our surface area.

So that in a nutshell is what we have been doing for the past couple years. What were the results? What did we get? First off, I want to start with the actual corrosion layer. Did we actually get a corrosion layer, what was the composition and how thick was it? The top right figure is our XRD pattern and these are all typical data, atypical but truly were, top right is X-ray diffraction data pattern of which you can see that we were detecting signals for boehmite which would be the pink triangle, not triangle, diamond sorry. Blue squares for bayerite and actually seeing some aluminum metal itself which are the green stars. We know our corrosion layer is composed of a mixture of bayerite and boehmite. Then took our sample and chopped it in half effectively so that we could actually take a series of SEM for cross-sectional analysis which is your bottom right figure down here to determine the thickness of our corrosion layer. And these cross-sectional images as you can see allow to calculate average of 5.3 microns which was basically our target, as I said is relatively representative of some of the inventory. We then also took SEM images of the surface of our samples to actually get an idea of the morphology of these systems. So you'll see in figure A and B, the top left-hand figures here that we have this overriding sort of pretty cubic bayerite phase on the corrosion surface. What I hope you can see, these are magnifications of same sample. A dark little pocket in figure A, in the middle of that pocket is this needle-like boehmite phase. We have an outer boehmite phase and we have a needle-like boehmite phase underneath. This was subsequently confirmed by doing what we called acid strips or sort of acid dip. So, we dipped these same coupons into phosphoric acid to selectively dissolve the bayerite phase even behind nothing but the boehmite phase which was then confirmed again using SEM.

So, we were successful. We achieved a corrosion layer. We know its morphology and have an idea of its thickness. So, what happens when we irradiate these systems? So, I'm going to start with the dose dependency; you will see the same figure a lot but trying to deconstruct it piece by piece. We’re going to start with gamma dose dependence. What you have here is a plot of concentration of molecular hydrogen on the Y axis and as a function of absorbed dose on the Y, on the X axis, apologies. Within which you have four data sets. I assure you there are four even though one is just a straight pink line, corresponding to helium data, black squares, nitrogen data, blue triangles, Argon data, red circles, and air data, ambient air data, just a straight pink line which we will touch on very shortly. Now, all of this data is for pre-corroded samples. So there’s no pristine samples here at the moment, to 50% relative humidity. I just picked one of many figures that we have that demonstrate what I want to convey. What I hope you can appreciate is that for all systems, ignoring the air system, that there is a linear response to absorbed dose. Meaning, the more we irradiate this system, the more molecular hydrogen we're actually getting. Which should come as no surprise because typically the more energy you put into a system, the more chemical change you are going to see. Now what is surprising is within the dose range investigated, we did not see a rollover point, sort of the steady state yield, which would be achieved in one of two ways or a combination of the two, either by consumption of the majority of molecular hydrogen precursors, so the bound water, oxyhydroxide layers or that you built up a sufficient concentration of molecular hydrogen that it can become involved within the radiation chemistry. Clearly, we haven't hit that regime yet.

Now this is something we should look at in the future, that's by the by. Currently, within this regime there is a linear response to molecular hydrogen. Now I hope you can also appreciate that there is a very clear difference between the different basis of environments. So, for ambient air we measured nothing, it was below our detection within limits if it did exist. That's a consequence of oxygen in air scavenging the precursors to molecular hydrogen which there is a ton of literature out there for this behavior in aqueous systems, organic systems, et cetera, et cetera. And what oxygen tends to do, it tends to chew up, tends to react with the hydrated electron and the hydrogen atom which I showed you before allow for the formation of molecular hydrogen. So, we're getting nothing from air.

If we irradiate these coupons in nitrogen and helium atmospheres, you get what we found recently to be equivalent yields of molecular hydrogen, which is great because helium is what we want to use. It’s what we're planning on using. But, these aren't innocent bystanders, these gases. I'll explain what that is in a second. They’re not innocent they are inhibiting hydrogen production to an extent, not as much as oxygen, but to some extent. An example of that would be in the helium atmospheres, Penning ionization. Helium has such a high ionization potential that when it does receive some irradiation energy that is imparted into the system, it forms these excited helium states, which don't want to be excited and decide to pass that energy off on to the next thing it bumps into, which could be molecular hydrogen, because there’s not much else floating around in gas phase or it can impart that damage, that energy, sorry, to the surface. Either way you can visualize a mechanism whereby excited helium states passes energy to molecular hydrogen, which then results in ionization potential and potential decomposition. Okay? Now the reason I can guarantee they are not innocent bystanders is that if you have a look at the Argon data set, red circles. You were getting twice as much molecular hydrogen generated in an Argon system than you are in a helium system. And that is because Argon is a noble, relatively inert gas, but it’s ionization potential is not as high as that as helium. It is very, very close to that of hydrogen. So any energy it does receive, it isn't really passed on to the hydrogen, if it is it is marginal. So we are getting the most hydrogen in our Argon systems.

Now the data I showed you so far has focused on corroded samples, on corroded samples.. Does the corrosion layer actually have an effect? Because we get all the same trends in pristine and pristine sample scenarios, but what is the magnitude difference? Well, here is a plot once again of molecular hydrogen concentration on the Y axis and absorbed dose on X axis within which are two data sets. We have a black squares data set for pristine coupons and we have a red circles data set for the corroded coupons and this was taken from one of the earlier plots. And these are coupons that have been irradiated in Argon atmospheres at 50% relative humidity. Now there are two key observations to take away from this. The first is that at relatively low doses I would argue the rate of hydrogen production is basically the same, the pristine and corroded samples. Now what that's telling you is that on our pristine surfaces you are getting absorbed water and we will also have absorbed water on our corroded surfaces. So, at relatively low doses, water radiolysis appears to be producing the bulk of the molecular hydrogen in these systems. However, as we continue to increase the dose received by these systems, you'll start to see that these data sets deviate in the amount of hydrogen produced. Now what that tells me, and what we believe this to be a case of is, from the pristine data set we're starting to deplete this water, absorbed water content inventory whereas in the corroded systems we have these oxyhydroxide corrosion layer, which not only provides a porous environment for potentially more water to bind, but also a source of molecular hydrogen you can generate hydrogen atoms in these systems. So, the prediction we have at the moment based on these data sets and others that are similar that, as I said the trends are the same in all the permutations for the most part, is that our pristine data set will reach a steady state yield long before the pre-corroded data set. So, corrosion layers are important for molecular hydrogen generation, when there is radiation field, to be clear, only when there is a radiation field that corrosion layers are important for molecular hydrogen generation.

Now all the data I have shown you so far has been at ambient temperature at about 30 degrees C. What happens when we increase the temperature? and this figure demonstrates that. We have a plot of the volume of molecular hydrogen on the Y axis and the temperature in which this coupon, these coupons were irradiated along the X axis. Now these are pre-corroded coupons in Argon environments at 0% humidity. And what I hope you can appreciate is that basically ambient temperature and 100 degree C the volume of hydrogen produced is essentially the same. Okay? Which tells me that beyond any real kinetic effects occurring here because typically if you take a condensed aqueous base and basically Arrhenius parameters, you increase the temperature 10-fold, you double the reaction, that kind of behavior. We're not seeing that. We're seeing a marginal increase in molecular hydrogen production. However, when we irradiated 200 degrees C you see this drastic increase, three to four fold increase in the volume of hydrogen measured. Now this is attributed to two phenomena. The first is that below 170 degrees there isn't any real mineral phase transformation. Whereas as you get to approximately 117 above you start to convert, convert bayerite into pseudo-boehmite and then into boehmite. Now previous studies in the literature have shown that from the powder studies, boehmite causes a higher radiolytic yield of molecular hydrogen than bayerite. So, you have this chemical transformation of bayerite into boehmite which in theory should up our radiolytic yield of hydrogen. The other effect that is sort of going on at the same time is that we're effectively annealing our sample. What I mean by that is, the higher temperatures are allowing for the release of trapped hydrogen and trapped hydrogen atoms where ambient temperatures in 100 degrees for example, when we measure the headspace we're only measuring what has managed to diffuse out of the surface or out of the oxyhydroxide corrosion layers. Whereas at 200 degrees C what we're seeing is release of those hydrogen atoms that may have been bound may have formed more hydrogen and also release of any trapped molecular hydrogen in corrosion layer microstructure.

So, temperature during irradiation has an effect, and this may also have an impact on the findings that we just reported by Rebecca. If we pretreat our samples, if we do a pre, a pre-treat using high temperatures and we convert our bayerite to boehmite, are we going to see more molecular hydrogen? I don't know, we haven't investigated that from our perspective as of yet. We’ve just worked with the samples with the corrosion layers we can generate using the methodology I discussed before.

So, temperature has an effect. But, what about humidity? Well humidity, here we have a plot of concentration of molecular hydrogen, Y axis, absorbed dose, X axis, within which we have three more data sets all of which are for pre-corroded coupons in an Argon atmosphere. Now the three data sets correspond to 0% relative humidity, black squares. 50% relative humidity, red circles. And 100% relative humidity, blue triangles.

Now the first observation to take from this is that relative to 0% relative humidity, if you increase the percentage of humidity, you increase the molecular hydrogen yield. That should come as no shock. The more water that could potentially bind to the surface, the larger your inventory of hydrogen, molecular hydrogen precursors. The second observation which is a little bit more subtle here, but becomes more obvious when you start to compare the G-values which is shown in the legend, as we get past 50% relative humidity we don't see this linear relationship with humidity. What we see is it sort of begins to decrease. Which at first you might say, well Greg, these yields are pretty close, maybe it’s within the error, but no, this is an actual phenomena. In the bottom right-hand figure, which is reproduced from the publication that is referenced above it, J.A. LaVerne found for similar aluminum oxyhydroxide powders, that once you go beyond a certain water content threshold, your system stops behaving like water absorbed onto a material and starts behaving like bulk water. And what do I mean by that? What I mean is beforehand we got a little bit of water radiolysis on the surface, decomposition of the oxyhydroxide layer, forms hydrogen, hydrogen escapes, get into the gas states. Maybe there’s some trapping after heating. If our water starts behaving like a bulk system, the hydrogen has to migrate through the water and if it is migrating through the water it is now susceptible to other water radiolysis processes that can consume hydrogen. So we’re kind of sort of forcing a limit on how much the water content can contribute to enhancing the yield of molecular hydrogen.

So, with that, what I hope I have shown you is that understanding the fundamental radiolytic processes responsible for molecular hydrogen will help us to inform the types of modeling that Dr. Alex Abboud is going to talk about shortly. I’ve shown you that hydrogen yield increases with absorbed dose. I’ve shown you that it is very sensitive to the gaseous environments around it. So much so a little bit of oxygen could completely switch off the hydrogen, but a shift in apparent inert gas, can certainly increase or decrease radiolytic hydrogen yield. I've shown you a corrosion layer does have an impact upon the yield of molecular hydrogen and hopefully further studies into trying to establish that steady state yield will further emphasize that difference between burning up, if you will, the absorbed water inventory and then the corrosion layer. I've shown you that humidity relatively increases the yield molecular hydrogen up to a certain point. I think. Oh. and I've also [LAUGHTER] also shown you that temperature has a significant effect above approximately 170 degrees C. Now we didn’t investigate at 170 degree C, we investigated 200 but around that number, that magnitude we’re starting to see significant enhancement in the yield of radiolytic hydrogen, probably because we're converting a relatively lower molecular hydrogen source into a higher molecular hydrogen source.

Now what are our plans -- as with all good science, it sort of asks more questions than it answers. And one of them I’ve touched upon quite a few times is this concept of a steady state yield. You need to go to much higher doses to establish that yield. Because at the moment we can provide G-values which is effectively a rate, but rates are dependent upon inventory, okay. And as was highlighted earlier in today's meeting the inventory varies from pond, from country, from reactor so we need, we need a better handle on that and to get that we need to establish those steady state yields. And what would be very good is to be able to correlate steady state yield which changes on the surface within the corrosion layer. Because that way you could do a quick analysis of the corrosion layer and say look it’s this thick, it’s this composition, we're going to see a steady state yield of ‘X’ in 25 years. That's the kind of knowledge that we want to be able to have to be able to inform the models.

And then the other major question is that all the data I presented thus far is for aluminum alloy 1100 which is prevalent in the ATR fuel inventory. However, there are other alloys especially in the US inventory, another one is 6061 so shown down here marked 16B; 6061 is another prevalent alloy. The difference between 1100 and 6061 is 6061 has a higher percentage of chromium and magnesium built into it. Do those affect the composition of the oxyhydroxide corrosion layer? And if so, does that influence radiolytic hydrogen production? Some of this is currently underway, we are looking at 6061 at the moment and should hopefully have data to present in the not too distant future. These are questions to ask ourselves. Is what we're finding representative for different alloys?

Now with that I would like to thank the US Department of Energy Office of Environmental Management for supporting this research which certainly could not have been achieved without everybody shown on this slide, especially Dr. Elizabeth Parker Quaife, top left who designed the majority of the methodologies that I presented and executed the experiments.

And with that I would like to thank you all for listening and for giving me the opportunity to present our results today and I will leave you with a slide of our currently, our current deliverables, mixtures of reports and peer review publications and I will be happy to take and answer any questions from you. Thank you so much.

>>JEAN BAHR: Thanks Greg. I have a question to start, the hydrogen released from corroded samples, sample [inaudible]--

>>GREG HORNE: I'm sorry Jean, you are breaking up just a little, I apologize.

>>JEAN BAHR: Okay let's see if I can --

>>GREG HORNE: I'm sorry, I apologize.

>>JEAN BAHR: No problem. The comparison of, uncorroded examples, and the corroded ones did show at least from the corroded one not even doubling of the hydrogen released maybe 25% more at the upper limit. Do you want to comment on what that mean, [inaudible] how important is the corrosion layer ultimately if we are getting a lot of hydrogen corrosion?

>>GREG HORNE: So, my, to answer that question, from this figure there is let's say a 25% increase with the corrosion layer present. So, let's say that's the number. And let's say that number is not significant. What I, what I am concerned about is that as we continue to irradiate these systems, we burn through the absorbed water which is all that can be present on pristine. There is nothing else except just a boring oxide layer with no hydrogen source. And I think, I think some of the, one of the talks later on might actually address this. But we are going to achieve a steady state very early on with the pristine samples whereas I believe because of that corrosion layer we're going to reach a higher steady state yield molecular hydrogen because we haven't even really got to burning, I say burning -- radiolyzing the oxyhydroxide corrosion layer. We’re just starting, I believe, to see the difference, about 25% difference; we’re just starting to go see that and that's for a 5 micron layer. Some have thicker layers, some have thinner layers, some have very localized chunky deposits on there. That's a significant inventory in my opinion. But we won't be able to see, if you will, the exact magnitude of that change until we hit those steady state yields. Does that answer your question?

>>JEAN BAHR: Okay, yeah. Thank you. I see that --

>>GREG HORNE: Thank you for your question, Jean. Most appreciated.

>>JEAN BAHR: I see that Tissa has his hand up. So if we can bring him online.

>>TISSA ILLANGSEKARE: Great thank you very much and this is not in my area but it is very clear presentation. So, I have very simple question, in your humidity effect, the first one is looking at the chemistry, there is no humidities imposed on the system are constant during the

process, is that kind of 50% humidity, 50% maintained throughout the experiment. Is that correct?

>>GREG HORNE: I, not exactly. Not exactly. So, once we flame-seal these systems, we flame-seal them with that specific environment. Now what happens after that is completely up to the radiation. I have no control over that.

>>TISSA ILLANGSEKARE: Okay.

>>GREG HORNE: So, the 50% is not constant, some of that atmosphere is going to be undergoing, is going to be binding to the surface and undergoing radiolysis. One of the things I forgot to mention… I apologize for cutting you off. One of the things I forgot to mention is that we irradiated all of these system permutations without a coupon present. And for every humidity permutation there was no hydrogen present without the coupon. So there is.. you need that interaction with the coupon otherwise it would be constant, yes.

>>TISSA ILLANGSEKARE: Okay, so my question is then knowing the sort of nonlinear behavior, why did you select three humidity values? Why didn’t you select a 4th?

>>GREG HORNE: I selected three to make sure, to ensure that was the trend being seen. Now a lot of these previous powder studies that are out in the literature, they are fantastic radiation chemistry studies but it is for a free flowing powder which is by no means representative of a relatively thin corrosion layer of that mineral or powder on a relatively thick piece of metal. So what I wanted to ensure is, are we going to see the same trends? Are we providing the same surface area? because I can guarantee in some of these powder samples, let's say 1 gram of sample. That surface area is exponentially larger than what we are providing with a piece of aluminum-cladding. So, it was picked to investigate that. I did kind of expect it, but I would not have been surprised if I was proven wrong. I'm a very fallible human being.

>>TISSA ILLANGSEKARE: [LAUGHTER] Thank you very much.

>>GREG HORNE: No, thank you for your question.

>>JEAN BAHR: Next we'll go to Paul Turinsky.

>>GREG HORNE: Hey, Paul.

>>JEAN BAHR: We can see you.

>>PAUL TURINSKY: Okay very, very simple question. And we'll use this figure that's up now. If all the hydrogen that's available gets converted to H2, okay, what fraction of it are we looking at here when we're up to about 1000 kilo grades.

>>GREG HORNE: I'm glad you asked that question, I cannot honestly give you a proper answer for that, but the reason for not being able to give that answer is, if you remember I mentioned that our corrosion layer is composed of some mixture of bayerite and boehmite, okay. I don't have a technique currently to be able to tell you that, well that layer is 52% bayerite and 48% boehmite. Now, you can say, Greg, you may give me a worst case scenario..say it’s all boehmite and I could do that and I might go do that, but I don't have that number at hand. I think we have done that in past just don't have it at hand. What I would prefer is being able to say well that's 100% boehmite, worse possible yield you can get is, I don't know, 50 millimole, let's argue it is 50 millimole or something, I would love to be able to do that, but I can't right now. And I don’t think currently especially with these nonhomogeneous chunky deposits that I’ve talked about we can do that in process conditions if you will, do that on the fly. But I will go and try to come up with a couple of numbers using some assumptions and send you that information, Paul, if that would be of use to you. I apologize for not having that ready, I should have thought of that.

>>JEAN BAHR: I think it would be informative if we know, you know whether this represents 10% or 5% or 8%, that would be --

>>GREG HORNE: I thoroughly agree with you, yes. I will work on that for sure.

>>PAUL TURINSKY: Okay and you have models, are your models predicting saturation with production and removal rates are equal in that [multiple people speaking].

>>GREG HORNE: I will Alex answer that question if he is either listening or he is on after me I believe. I don't

want to speak for him and put words into his mouth and have him have to dig out the hole I've created for him. That should get answered very shortly I believe.

>>PAUL TURINSKY: Okay. And related to that is if we look at a thousand here and we look at -- I know it depends on irradiation time and cooling times and all that, are we talking about a year, a hundred years?

>>GREG HORNE: That, that I also need to calculate. We have that number somewhere; I think this value is not too far off purely because a lot of these things we're transferring have been sat in a pond or been sat on the going decay. So the field itself is relatively low compared to it just being pulled out of the reactor and dumped in there. So cumulatively I think we're not a million miles away. I need to go and dig that number out.

>>PAUL TURINSKY: Yeah, because maybe in practice if it is like a 50-year storage time you never get to equilibrium.

>>GREG HORNE: Oh thoroughly, and maybe that's great because we can use these G-values. The question then comes in 50 years' time when we decide we want to continue extended storage and not change canister. How much longer can we extend it, just how much longer can we extend this?

>>PAUL TURINSKY: Your grandchildren can do that experiment.

>>GREG HORNE: Yes, I can keep them in business, family business, stretching that line out. [LAUGHTER]

>>PAUL TURINSKY: Thank you.

>>GREG HORNE: Thank you for your question.

>>JEAN BAHR: Yes, thanks, David Shoesmith.

>>DAVID SHOESMITH: Hi, Greg. I'm always convinced it would be better to listen to presentations before you read reports because your presentation answered a number of questions, I had based on some of the reports. But one particular one I have, I'm familiar with radiation effects on oxides from point of view uranium dioxide, but where does the oxygen go? Hydrogen, where does the oxygen go?

>>GREG HORNE: Yeah, there has to be mass balance. We believe that's binding to the surface, forming more oxide. The reason for that, weren't just measuring hydrogen, we were looking for, we were looking for anything we could go after in addition to hydrogen and we are not detecting any oxygen. Now there is some speculation, maybe it forms peroxide which then condenses onto the surface and oxidizes the surface. So, I believe it all goes to oxidizing the surface further. So, the gas phase.

>>DAVID SHOESMITH: So, in fact it's causing more corrosion.

>>GREG HORNE: That would be one way of interpreting that or it's converting the oxyhydroxide layer into more of aluminum oxide. I don't think we have burnt through the oxide to reveal more aluminum metal at this stage.

>>DAVID SHOESMITH: I'm not sure I see the chemistry there. I'm sorry, can you hear me?

>>GREG HORNE: Yes, I can hear you, David. Yes. I certainly can, yes.

>>DAVID SHOESMITH: I'm not getting an indication that you can. I don't think that's enough, I think you in your model here you either produce OH- or you produce radical species which is going to actually react. So, there should be some balance between hydrogen production and ongoing corrosion of the aluminum, I guess. I don't see how that will be soaked up by the oxide, which is not changing oxidation state. So, the composition has to be producing a corrosion process or --

>>GREG HORNE: There has to be some migration to the surface is what you are saying?

>>DAVID SHOESMITH: Yes.

>>GREG HORNE: Yes, I can very well believe that what I definitely know is it's not leaving the surface, or if it is not for very long if it's a peroxide for instance, it definitely goes back to the surface and there's further oxidation there.

>>DAVID SHOESMITH: I know coming up in the presentation of modeling there is an assumed reaction which is clearly

covers up a lot of potential reaction steps because it will be, reaction if it actually occurred. But this is effectively telling us that the production of the hydrogen is in fact driving the corrosion of the aluminum.

>>GREG HORNE: I agree, the only reason I was pushing forward the oxide to start with is because in a lot of the old powder studies, a lot of them are out of Notre Dame, they also don't measure, from memory… they do not measure an oxide species in gaseous phase. They are not generating liquid peroxide in there. So, the only thing left in their system is more of the oxyhydroxide powder, they did not do aluminum metal. I do agree with you, there will be some extent of further corrosion of the aluminum metal underneath but there is something else going on with that corrosion layer that is not as straightforward as we're looking at it. Maybe I'm wrong but just based off previous studies on powders compared to what we're finding, we're finding the same thing within reason, different magnitudes obviously.

>>DAVID SHOESMITH: I don't wish to prolong this discussion which is very interesting for us but not necessarily productive for this meeting, but the alternative is OH- , even that is not really an alternative because you have a redox process. That would be quite dangerous because solubility of the oxide would go right up and you’d start opening up the porosity. Now there’s a lot of work still to be done here, which is not to discredit what you have done, I think is very good. You’ve driven me into reading a whole pile of papers I wouldn't have read before. So I just wanted to make a point that I don't think this game’s over in terms of the radiolysis. The drying process might make it irrelevant eventually, but in terms of the understanding of what's going on, I don't think it is there yet.

>>GREG HORNE: No, I agree, that is one of the reasons we want to try to correlate the steady state hydrogen yield with changes in the surface itself. Because within, within the dose ranges we have currently seen using SEM, we didn't see any changes in the oxyhydroxide, oxyhydroxide corrosion layer. We didn't see any significant changes. But I think that's because we're predominantly burning the water out and not the layer --

>>DAVID SHOESMITH: Because the physical changes in the oxide, you can't dehydrate an oxide unless it starts eventually to look like mud or refraction.

>>GREG HORNE: Yeah --

>>DAVID SHOESMITH: Go far enough yet, is that the point?

>>GREG HORNE: Either not far enough or we weren't using the correct techniques, so I currently have a fantastic certain materials characterization postdoc who is going to be throwing every technique under the sun for us to correlate those changes. But what we were doing originally before we pose that, it was sort of half-way through our study we posed the same question, is where the hell is oxygen going? and it only be going one place. Why are we not seeing it in the SEM image we would take along the way? We'd take coupon, have a look at it. No real changes to the initial pre-corroded um microscopy image. But there has to be, I agree with you thoroughly. I'm not saying -- there has to be.

>>DAVID SHOESMITH: If I could just squeeze in one more question before Nigel gets his chance, how sure are you that a large dose for short time see short have a small dose for a long time, because that's what --

>>GREG HORNE: The, the -- I am relatively sure, based on the dose rates we used. So, between ourselves and Savannah River National Lab, we covered three, four, maybe five different dose rates because what we were trying to do at the time was we wanted to get these higher doses. But we had.. once this project started, we had an irradiator that had gone through two half-lives so relatively low dose rate. Our mew irradiator arrived which had, depending upon where you were from the sources you can get up to 7 different source rates, Savannah River had two different and then we did round robin study, the data we got it from within experimental layer agree so I don't… I think from fundamental perspective, yes, dose rate has an impact. Okay. But I think from what we're seeing at least in the gas phase, the dose rate does not appear to be changing our answer at a given dose. Does that answer your question?

>>DAVID SHOESMITH: Yes it does, yes, I have always been opposite opinion for long experiments but that's fine. You did answer my question, yes. Thank you.

>>GREG HORNE: Okay, thank you, David.

>>JEAN BAHR: This will be the last question for Greg.

>>GREG HORNE: Hey, Nigel.

>>NIGEL MOTE: Greg, thank you for a nice clear presentation. Great to see a Manchester grad presenting results.

>>GREG HORNE: Oh fantastic.

>>NIGEL MOTE: So, I’ve got a question about the presentation of results, and I would like you to go to slide 12 if you will.

>>GREG HORNE: You'll have to tell me what slide 12 is, unfortunately, this template doesn’t --

>>NIGEL MOTE: Humidity dependence.

>>GREG HORNE: Yes of course, there we go.

>>NIGEL MOTE: Okay so every one of your lines is drawn straight, indicating a linear relationship between dose and hydrogen yield. Is there a good basis for that? because I'm always nervous when someone draws a straight line through three points on a graph and three points are not on the line.

>>GREG HORNE: I thoroughly agree with you. We so we yes it's good for the higher dose rates, it is not very good for the 0%. Now the reason that these were drawn as or fitted as straight lines was to purely convey the differences, so you are correct. Is linearity being maintained throughout this dose range for all these systems? No. You can fit an exponential for the 0% but then trying to compare the systems in a meaningful model, manner becomes difficult. And now not that I have a problem with the difficulty, but trying to convey it so that it's accessible by a whole host of different scientific backgrounds does become difficult. So you are 100% correct. It is relatively inappropriate for the 0%, however for the 50% and

the 100%, I think we have some higher data points. I think I cut if off just for arbitrary reasons. There’s a very good linear fit.

>>NIGEL MOTE: Okay, so the message is look at the relative position of the lines, don't look at the shape of the lines.

>>GREG HORNE: Just purely for demonstrating today's parameters, correct, yes.

>>NIGEL MOTE: Well, if it can, Part 2 the first line of text says, ‘higher H2 yields with increasing relative humidity’, yet on that graph, the 50% relative humidity is higher than the 100% relative humidity.

>>GREG HORNE: And that was my caveat during presenting this, is you would argue the general trend is it increases, maybe that should be worded better. It increases with relative humidity up to 50% for sure. But then once you get to 100% humidity regime we this this is starting to act more like bulk water, and that’s where now the hydrogen has to diffuse through the water phase to get to the gas phase and more chemistry can occur, which can inhibit its yield. And very much does. You see in for instance just pure water yields are lower than you are getting for some of these powder studies purely because you have that water phase and additional chemistry between escaping into the gas phase. Does that make sense?

>>NIGEL MOTE: Yeah, yeah it does. Okay. All right. Thank you. Appreciate it.

>>JEAN BAHR: Okay.

>>GREG HORNE: Thank you, Nigel, thank you very much.

>>JEAN BAHR: Thanks very much, Greg, and I think you can stop sharing your screen we're going to move on to our next speaker who is Anna d'Entremont from Savannah River National Lab, and she is going to be talking about radiolysis testing of aluminum-clad spent nuclear fuel and surrogates in a Mini-Canister Environment. So, if we have, we have Anna's slides and don't, I don't see Anna but maybe she, oh there she is. I have a funny screen here. Take it away, Anna.

>>ANNA D'ENTREMONT: Can you hear me? Hi, I'm Anna d'Entremont from SRNL and so I've been working on with SRNL side team on task 6 and what I'm presenting today is on our scale-up radiolysis testing which is a different radiolysis testing campaign than Greg just presented. Another step. So, can you move to the next slide please?

Okay. So, most of this has already been introduced by various other presenters mainly Josh and Greg. But the focus of this task is on, of this work that I'm presenting is on the radiolytic hydrogen generation. So, we're looking at samples with these aluminum oxyhydroxides on the surface, bayerite or gibbsite associated with lower formation temperatures and boehmite which has less water than bayerite or gibbsite but forms at higher temperatures. I think the reasons why hydrogen gas can be generated was also already thoroughly covered by Greg, but we're expecting the sources of water in these canisters that we're considering to be residual liquid water or water vapor, physiosorbed water that is absorbed to surfaces in the, of the fuel or other surfaces in the canister and then the aluminum oxyhydroxides with the chemisorbed water. And so, we're trying to get more data on the expected yields and the rates of production which are needed for performance prediction to feed into models like Alex's and also of course just assessing the safety basis for this dry-storage in general. Next slide please.

So, this particular work is our mini-canister radiolysis testing, and the goal of this is to measure radiolytic gas generation in a canister-analogous environment and also, to do testing that allows for online monitoring, meaning we can take multiple measurements over the course of the same irradiation on the same sample. So, in comparison, the work Greg just presented was using these flame-sealed glass ampules which mean you load a sample, you load the desired gas environment, you seal it shut and irradiate but once you crack it open you get one data point for that sample and ampule and so here we're trying to get a more continuous set of data points.

So, to do this we're doing, we're using a stainless steel vessel, so much, much smaller than a real dry-storage canister, but same material. It's going to be under a helium cover gas, which is the expected gas that's being discussed for actual dry-storage. And then, we're testing either surrogate coupons with lab grown oxide layers, similar to what Greg was doing, or in our latest test that we started we're using a reactor-exposed material. It is aluminum material that came from an actual reactor environment that has been in wet storage.

So, this data will allow us to evaluate impact of various factors in the environment such as, is it subjected to a drying process beforehand? and also provides us with a good data set with time and dose in order to benchmark models for Alex's work. Next slide please.

So, our mini-canisters are commercial, commercial steel canisters. They are about 7 inches high and little under 3 inches in internal diameter. There is a lid that seals on the top with a gas sampling port so you can see the picture on the left-hand side that tube coming off the top. That allows us to sample while the canister remains sealed shut. And so that is our sample that was from a reactor environment, which is why it is decked out in all its radioactive material stickers. This entire sealed canister is placed inside a cobalt-60 irradiator that provides gamma irradiation to stand in for the radiation in a spent fuel environment. We measured the dose rate inside an empty canister and determined that to be around about 38 kilorads per hour of gamma. And we're testing at ambient irradiator temperature a little above little room temperature, it is about 23.5 degrees Celsius. So we sample our gas at intervals during this test. And then those samples are, the gas composition is assessed using a gas monitoring system. We’ve got two column gas chromatograph using Argon and helium gases in the different columns, which allows us to more easily look at different components since we got a helium cover gas, that effects how they show up in the different carrier gases. Our Argon column is particularly long, they made it 20 meters long for which allows us to get a greater separation between the hydrogen signal and the much larger signal from the helium cover gas. So that doesn't just completely swamp out our hydrogen signal. This system has been shown to give us sensitivity to hydrogen down to 100 parts per million in helium and our sensitivity for nitrogen and oxygen was shown to go down to 20 parts per million. Next slide please.

So, this is just snapshot of our overall um online monitoring system. This overall system was actually developed for a future monitoring of an actual ASNF dry-storage demo canister. The idea being that once we have nailed down how we think this should be dried, et cetera, um that a standard canister could be loaded with actual fuel and um monitored in this similar fashion to how we're monitoring these mini-canisters over a much longer period of time to make sure that everything is progressing in the way we expect. So I'm not going to go into detail on this larger system but the key points here for the mini-canister setup are that we got a system of valves, tubing and a pump that allow us to draw out a sample of known volume, basically evacuate a section of tube, open the valve to the canister, let the gas fill that section of tube and now we have sample of known volume, measure the pressure and we can calculate how much gas we drew out. I already described our gas characterization set up to measure the hydrogen, nitrogen and oxygen concentrations which is what we're primarily focused on in this, in this mini-canister work. Next slide.

One more please. Okay so first of all I'm going to describe what our, what sample, the samples we’re like going into irradiation, what we started with. Starting with our surrogate coupon assemblies. So, these are non-radioactive surrogates, they are just aluminum plates, which are aluminum parallel plates held together by a bolt. We’ve got aluminum spacers to maintain gaps between the plates and then this whole assembly is used as a single sample. The idea of this design is that it roughly approximates the type of geometry we would see in a spent fuel assembly um such as ATR or something like that, that has a set of parallel plates. It also allows us to maximize the sample area in our mini-canister. So, on the bottom left there you can see a drawing of the assembled surrogate assembly looking from the top down in a mini-canister. So, we vary the width of these parallel plates to so the whole assembly conforms to that cylindrical profile and that allows us to basically fill up the cylindrical canister with sample. We want to maximize that area because it will allow us to get signal faster, and also Greg was alluding to this, we expect that at some point the gas environment as hydrogen builds up may approach a point where we get a steady state. And so…by getting more sample area in the canister we're hoping to reach that rollover point as quickly as possible. So, after this surrogate assembly was put together, it was corroded in the lab. It was immersed in room temperature water for more than 30 days. I think the actual timing on these, the two assemblies I'm going to talk about were I think 41 and 36 days. So this is a very similar preparation to what Rebecca did … for the chemistry surrogates in the drying tests. After corrosion, we removed a small sample from the outer most plates on, from one of the outer most plates, on each of the assemblies for characterization purposes. So, on the right-hand side you can see results of that characterization for a corroded assembly. We did X-ray diffraction and we found that it was primarily bayerite which is what we expected for a room temperature immersion. We got scanning electron microscopy, so you can see a couple of those images on the right-hand side. It's a layer consisting mostly of these small ROD-like crystals that seem to bundle together into larger structures. And we also did a cross-section and did SEM of the cross-section to see how thick the oxide layer was and we found it seemed to average about 9 microns thick. And in the grand scheme of things that is a very thin layer but it is comparable to some of the layers we were seeing from actual reactor exposed material as you'll see in a few slides. Next slide please.

So, on the right-hand side here you can see a couple photos of an actual surrogate assembly, next to an empty canister and then inside that canister you can see it pretty much fills it up. So, after corrosion these assemblies are loaded into the canister, the lid is sealed on and then we have a residual error elimination process that we applied which was to connect it to a vacuum system through that sampling line and draw a vacuum on the canister interior for twelve hours. And then we did four helium purges meaning we filled the canister with helium and then drew a vacuum and then repeated that process several times. After the residual error elimination, we back-filled the canister with helium to 25 PSI absolute, which was the maximum inlet pressure for our gas chromatograph setup. And I just want to note here that the reason for using this long vacuum was we want to get all of the residual air, particularly oxygen, out because as Greg noted the presence of oxygen suppresses the hydrogen generation we're trying to measure. We did not expect the vacuum to significantly impact the aluminum oxyhydroxide we formed as Rebecca discussed. That only seems to be driven off at higher temperatures. So, we just, it was primarily intended to get the air out of the system. We have done two tests with the surrogate assembly so far, so the first one we refer to as our ‘as-corroded’ assembly and that was prepared exactly as I just described with the loaded residual air elimination back fill. Our ‘as-dried’ assembly was.. we included an additional step which was heated drying, because we did want to try to drive off some of that chemisorbed water. Next slide please.

So, here's our procedure for as-dried assembly. That one was loaded into its canister and the lid sealed on in the same way. But we then put it into a furnace where it was, with the sampling line left open and heated to 220 degrees Celsius while exposed to the lab air through that sampling line. And we held it at that temperature for four hours. After four hours we connected the sampling line to a vacuum system and drew vacuum on the canister for one hour while it continued to sit in the furnace and then after that it was closed up, removed from the furnace and it went through the same residual error elimination step of twelve-hour vacuum and the helium purges and helium back-flowing procedure as this assembly. So, on the right-hand side on the top this is just a picture of our, the end of our sampling line as the canister was being heated in the furnace and we could see visible water dripping out of the sampling line indicating we were driving off significant amounts of water. We also had cut that sample from an outer plate after it was corroded, that sample was put into the furnace next to the canister, so it experienced the same temperature history as the, as the loaded canister. And we characterized that after drying to check with what the impact on oxyhydroxide layer was. XRD indicated that we saw some reduction in our, the height of our bayerite peaks in the formation of some distinct boehmite peaks which indicated that we had in fact partially transformed this oxide, partially dehydrated this oxide from bayerite into boehmite, not completely still significant amount on the as-dried assembly but got partial conversion. That was also confirmed, further confirmed by scanning electron microscopy. We found, we observed some very fine cracks in the oxyhydroxide layer, which is something we have previously observed for the, for dried oxyhydroxides, due to contraction because the boehmite is denser than the bayerite. Next slide please.

So, our third sample is our reactor-exposed sample. This is material retrieved from our L-basin at the Savannah River Site. This piece was irradiated in the Missouri University Research Reactor. It was cut from a fuel assembly but the piece that was cut off does not contain fuel itself, which makes things a lot easier for trying to do experiments on it. We know that it was in the reactor for 113 days, temperatures greater than 60 degrees Celsius. We don't have a specific history for this piece in terms of wet storage, but we know it was in the basin for some period less than 18 years, and the basin temperature is about 30 degrees Celsius. In the image there the MURR cropping at the upper left of the right-hand figure is what we're talking about, that small piece. Next slide please.

We characterized the oxyhydroxide on our MURR sample. XRD indicated that both bayerite and boehmite was on the surface, SEM showed a relatively uniform blocky oxide which you can see on the bottom left and cross-section SEM indicated that the layer was about 10 microns thick on average. The morphology is a little different but the thickness is actually very close to the surrogate assemblies. For this sample we chose to do no drying except drip drying in air. And the reason for this is some recent ampule data has shown that vacuum drying, the vacuum for twelve hours, as we did for the surrogate assemblies can, does have a significant impact on the radiolytic hydrogen production. We assume this is due to removing physisorbed water, and we do not want to disturb anything about the water loading on our reactor-exposed sample since we have this, we have several pieces of reactor-exposed sample and we don't want to waste any of that before, without getting all of the data we can out of it. So, we want to get first bounding hydrogen generation rate as this comes out of the basin. One thing to note is that this is a very small sample compared to the surrogates, surrogate assemblies. The image on the right shows a top down view in the canister, and you can see there is a lot of empty space there. In contrast to those assemblies were designed to fill it up. So, we expect to see less overall hydrogen generation going and its going into a larger gas volume which will tend to dilute the concentration. Next slide please. One more.

Here is our results for our surrogate coupon assemblies. On the right-hand side that plot shows the cumulative hydrogen generated in micromoles as a function of the dose. The red circles are ‘as-corroded’ assembly which only had the 12-hour vacuum. The ‘as-dried’ is the blue squares which had 12-hour vacuum and a heated drying step. Obvious take-away there is that heated drying step had a major impact. The hydrogen generation rates were lower, right from the beginning for the thermally dried assembly and also it appears to be leveling off either a plateau or much lower hydrogen generation rate. We don't… it's not quite flattened out yet. We’re not sure if that's heading for a steady state or a, just a much lower hydrogen generation rate at this point. What is obvious is that ‘as-corroded’ assembly is already produced three times the hydrogen and is still climbing much more rapidly. What this tells us is heated drying of our ASNF canisters to 220 degrees Celsius has the potential to greatly reduce our hydrogen generation in the canister, and Rebecca's work has shown that getting that, a canister to that temperature with forced helium drying appears to be feasible. Next slide please.

So finally here are the results of our reactor exposed material test on the left-hand I'm showing it in comparison to our surrogate assemblies. That plot shows the hydrogen concentration measured in our sample, as a function of the time. And so, the red circles and blue squares are again the two surrogate assemblies, with just 12-hour vacuum and the 12-hour vacuum with heated drying. And then those green triangles at the very bottom are our first few samples from the MURR. That's also pulled out on the right so you can see them a little better. The main take away here is we are observing a small but increasing hydrogen concentration off of the MURR. It is smaller than for the surrogates at the same time but that's expected because it is a much smaller sample. We're going to have to do some more analysis to do a rigorous comparison between the hydrogen generation of the MURR, which is this much smaller sample to our surrogate assemblies. Preliminary calculations suggest that the hydrogen generation per unit surface area from the MURR is a bit greater than for ‘as-corroded’ assembly. But that's not really a red flag considering that it's consistent with our, it is consistent with what we now know about the impact of having that drying, that vacuum step. Initially we were expecting it wasn't going to impact the chemisorbed water at all, looks like has greater impact on physisorbed water, did not do that for the MURR… the fact it seems to be producing a little higher is completely consistent with our current state of knowledge about the behavior from the surrogates. Next slide please.

So, in conclusion our surrogate tests demonstrate that an elevated temperature drying step has, can greatly reduce the radiolytic hydrogen generation. That data can be used in combination with the results of our drying experiments in order to select drying parameters that could be appropriate for actual dry-storage application. This work also contributes data to the modeling effort. We got data for hydrogen generation rates over time in that sort of rollover that we are seeing for the ‘as-dried’ assembly that can be used to compare to model predictions. This also gives us hydrogen generation rates for the dried condition to go into a model of that could estimate canister conditions for a canister that was dried. We are continuing, we only have preliminary data for MURR, we are continuing to measure, continuing to sample that to get a hydrogen yield curve for that material, and we're going to need to do the calculations to compare that to the surrogate generation rates. Next slide please.

I'm not going to go through this list, but this is a list of our reports that has been put out on the instrumented lid work, which was what that monitoring system was originally developed for and led into this mini-canister effort. And one more slide please. This is a list of reports that have been put out so far on the mini-canister experiments themselves. So that's all I have, thank you for your attention.

>>JEAN BAHR: Okay thank you very much Anna. Just for clarification the reason that you're calling the surface area comparison of the MURR ‘preliminary’, is that because you're still doing those experiments or because there is some uncertainty in how you're evaluating the surface areas of the materials or

>>ANNA D'ENTREMONT: I'm calling them preliminary because they were very rough quick calculations based on estimates

of the surface area. We need to, I need to check numbers more carefully and make sure I got the right surface areas for both, that we got right sample volume because this calculation depends on the gas volume in the canister as well. There are just several factors that need to be refined and checked before I can before I can draw any more certain conclusions.

>>JEAN BAHR: Ok thank you. I see Lee Peddicord has a hand up so if we can get Lee.

>>LEE PEDDICORD: Yeah, thank you Jean. Just a quick question kind of for clarification on the MURR samples you talked about. I understand it correctly that these are samples that were taken from fuel rods but not necessarily the fuel portion of the fuel rods, or were they samples like from otherwise inside the core, adjacent to the core. So just to clarification because I kind of missed it.

>>ANNA D'ENTREMONT: Um this one we removed several samples from the basin and this was, a few of them came from non-fuel aluminum materials in the basin, but this one did come off in actual fuel assembly, it's an end fitting --

>> LEE PEDDICORD: End fitting yeah so the one question comes to mind is being on end fitting, maybe this is not so important but this would not have seen the doses or fluences of aluminum, say, adjacent to the fuel. Is that an issue, does that make any difference? What do you think?

>>ANNA D'ENTREMONT: Um it's definitely something we need to take into account. I think one of the, one of the questions we need to consider there is, ‘is it going to have the same buildup of oxyhydroxide that we would see on a fueled section?’ and I don't have an answer to that right now. Um and one of the reasons that this, this MURR sample is nice for this specific test is because that thickness is the same as what is on our sample so we're expecting, a large part of why we want to, we want to test a reactor-exposed material is to see how it behaves relative to the surrogate materials.

>>LEE PEDDICORD: Okay so --

>>ANNA D'ENTREMONT: If it behaves the same we have a reasonable amount of confidence the measurements we’re

getting for surrogates with various thicknesses are representative.

>>LEE PEDDICORD: Okay yeah I see Bob Sindelar is whispering back things through the chat. Very nice, thanks for the presentation. Interesting and well done.

>>ANNA D'ENTREMONT: Thank you.

>>JEAN BAHR: Paul has his hand up.

>>PAUL TURINSKY: What experiments do you, do you have planned now that you developed this apparatus?

>>ANNA D'ENTREMONT: We're discussing further experiments. The scope hasn't been pinned down yet but we're continuing the irradiation of the MURR to get more data points on that. There is definitely a potential for some useful data from another surrogate assembly with a different drying condition for example a drip-dried condition to match the MURR. Or a… greater drying to see if… how far that can be driven down. Also, we got some ampule tests going that were not presented in this current meeting that we're looking at some of those other reactor-exposed materials, samples from those in ampule testing at various levels of drying. So, another test with the MURR sample to see what happens after we apply drying to it, could potentially be valuable. We are definitely interested in working more with the mini-canister; we think we're getting very useful data out of it. It's partially a matter of irradiator space and how to use that most effectively, because obviously getting this long series of data points does take quite a while.

>>PAUL TURINSKY: Can you take a sample that you basically irradiated and put it back in to get a higher dose to really confirm where that equilibrium point is?

>>ANNA D'ENTREMONT: Um so all right so… with the current mini-canister setup we have taken out canisters that had been in the irradiator when we needed other stuff to be tested and just kept the canister sealed. And, based on our previous results we do not expect, we expect that when we put that back in that we will be able to continue the irradiation and basically continue that data set in terms of dose. We are specifically not opening the canister when

we're doing that so that we don't disturb the gas environment. Is that what you're asking?

>>PAUL TURINSKY: Yeah, yeah, yeah okay. This would be really nice to pin down where this equilibrium setup and see if basically validate the model. Because that's, that will reveal, that has a great deal of sensitivity where equilibrium is set in so it will really tell you a lot about the validity of models.

>>ANNA D'ENTREMONT: Yeah that is one of the things we're hoping to get out of the mini-canister setup is be able to, not only be able to leave it in and reach that equilibrium but since we're sampling over time we can see where we are in the process before we open this thing. Whereas the ampules you have to pick a dose and then you are stuck with it once you crack it open.

>>PAUL TURINSKY: Yeah, okay. Thank you.

>>JEAN BAHR: Okay Dan Ogg has his hand up.

>>DAN OGG: Yes, thank you Anna, really good presentation great information. Two questions, one, do you have enough of the MURR material to do some of the MURR test with drying included or do you think you'll have to go to some of the other material like the Uruguay fuel or some of the other fuels?

>>ANNA D'ENTREMONT: I believe that all of our current MURR sample is used in this test. And so well Greg got into this a little bit but we're thinking that most of the hydrogen we’re seeing is coming off of the, at this point, a lot of it is coming off of the physisorbed water is what we are thinking. That’s why we saw in Greg's stuff, in Greg's slides that the pristine samples were given off similar rates of hydrogen generation at the beginning to those with a corrosion layer. If what we're, if what we're getting hydrogen from is primarily the water absorbed to it rather than oxyhydroxide itself, we think that retesting the same sample after doing a drying process will give us some useful data. So, one course of action is just to take the same MURR sample, apply our drying process and re-irradiate that to see how it behaves. Um --

>>DAN OGG: Okay.

>>ANNA D'ENTREMONT: And hang on I thought I had another thought there. [LAUGHTER] So yeah that I think that is the only, that is the only MURR, I think all of our MURR sample went into the canister. The other thing was Greg had been asked about the amount of hydrogen that could be contained in that oxyhydroxide, how much of it we were seeing in the atmosphere. I also do not have an answer for that off of the top of my head, but I know those calculations have been done and what we had concluded was we're seeing a very small fraction of the total hydrogen source that we expect to be in those, in those samples. Which is another reason we don't think we're breaking down much of the oxyhydroxide, and we could retest the same sample.

>>DAN OGG: Yeah. Okay thank you. My next question has to do with the dose rates, I see it looks like you are using dose rates that are somewhat lower than what Greg was using in his test. But my bigger question for both you and Greg is, are those dose rates bounding for the dose rates that may be experienced by real fuel that's being stored by the Department of Energy? Have you looked at that? Have you looked to see what dose rates are present out in the universe of aluminum-clad fuel and how do your dose rates compare to that?

>>ANNA D'ENTREMONT: Um unfortunately I don't have an answer for that. I know, I think I have seen numbers for some of this but I don't remember what they were. Our dose rate was just determined by, this is what our irradiator is currently putting out in our comparison between the INL and SRNL results and different irradiators that were involved seemed to indicate that it was primarily a function of dose rather than dose rate. So that's what we have been assuming so far.

>>JEAN BAHR: I see that Greg has his hand up, maybe he wanted to add something to clarify.

>>GREG HORNE: Anna is actually spot on, this was sort of addressing a question that was asked by I believe David um over mine is that we did unintentionally dose rate study but we, the thing to put in mind is yes there is probably numbers out there, I agree. And I don't know them off the top of my head now, if you had asked me four years ago probably had it when I wrote the proposal, the idea when we originally started I didn't mean with any sass, the idea

when we started this is that we have to, we have to get doses close to what these things are going to get in their lifetime. So, let's start with that. And then there was the differences in two irradiators as Anna has explained differences in dose rate. So, then we said look we need to do a round robin study just to make sure that, you know, if I'm saying we're getting oranges, Anna is not serving up apples or something like that. It turns out it's just a fruit salad at the end and it doesn't matter. We get there, we get there regardless of the dose rate and the answers are relatively the same within experimental error. So yes, we could do a much lower and a much higher dose rate to try and cap what would be expected for different fuels, different compositions, et cetera, et cetera. But I think the unintentional dose rate study that we did has kind of demonstrated we don't need to look at that. But if you feel like we need to, then that's something we could attempt to do. But I feel like it's been bounded. Sorry for jumping in Anna, I apologize.

>>ANNA D'ENTREMONT: No, thank you. I just wanted to say Bob Sindelar has jumped in on the chat to say the rough estimate is that the SRNL dose rate is close to a loaded canister of L basin SNF in a standard canister. So according to Bob's numbers the SRNL dose rate should be similar to what it would be getting.

>>DAN OGG: Okay, thank you.

>>JEAN BAHR: I see Dave Shoesmith, this will be the last question.

>>DAVID SHOESMITH: Yeah you stopped my video but according to the message but that's not the point, I just quickly wanted to from my own personal clarification, when I read through the reports the surrogates have different morphologies and structures and but the general theme here seems to be that it is the actual infantry of oxide that matters not necessarily the structure of the film when you concurring the surrogates to the real thing. Is that perception correct?

>>ANNA D'ENTREMONT: Um I'm not sure that we have a concrete answer to that. Because I don't think we have really a way to tune the morphology on our lab growing surrogates, so we

have really limited data in terms of whether that affects anything.

>>DAVID SHOESMITH: I think it is fairly clear that you are not going to be able to get in a few weeks or months, in a surrogate to get over 30, 40 years in variable conditions. I just wonder whether or not it's a specific parameter of the film that matters; it’s inventory rather than morphology that is more important. And therefore, even if the structure is not quite the same, it doesn't matter the surrogate is still valuable.

>>ANNA D'ENTREMONT: That is what we're, that is what we are currently assuming and hoping. And we have not seen any evidence to the contrary yet.

That is why we want to do a good rigorous comparison of the MURR to our surrogates, for example. Because they are very similar in thickness. So we would like to make sure they perform similarly.

>>DAVID SHOESMITH: Excellent answer, you would make a good lawyer. [LAUGHTER]

>>JEAN BAHR: Okay, thank you very much, Anna and questioners, and I think it is now time to move on to Alex Abboud who is going to be talking about the modeling of all of this. If we can get Alex up.

>>ALEX ABBOUD: Yep, I'm here.

>>JEAN BAHR: Okay. And we, are you controlling your slides Alex or are the hosts controlling your slides?

>>ALEX ABBOUD: I'll run mine.

>>JEAN BAHR: Okay. Very good. Looks like we're up and running.

>>ALEX ABBOUD: Okay so that's showing okay? Um so I'm Alexander Abboud a research scientist at INL, been there about five, almost six years now. My specialty is in computational fluid dynamics. So I'll be talking about the modeling and simulation results for aluminum-clad spent nuclear fuel specific to the DOE sealed standard canisters. There has been research done for the vented

systems and that's been published in reports, but I'm not going to be discussing that today.

So the, just to summarize what we will be talking about the kind of main goal of this was to improve upon just prior conservative assumptions that were made for DOE sealed standard canisters. This was in a DOE-REP-104. The specific topics I want to look into are the hydrogen content and total pressure for possible flammability and canister mechanical failure concerns, and again this is just for aluminum-clad SNF. I setup the model for three different fuels, the ATR (Advanced Test Reactor), the Missouri University Research Reactor (the MURR) and High Flux Isotope Reactor (HFIR), should bound most of the inventory managed by the system. So the improvement that I'm going to be making to this previous assumption is to use a 3-D computational fluid dynamics (CFD) model. This is run on INL supercomputer using star-CCM to fully resolve the temperature data. This uses a multiscale time stepping approach to arrive at a 50-year long time frame model. Then this is this CFD model is coupled with Cantera chemistry package to solve the gas and coupled surface phase chemistry.

So for reference as to what the DOE standard canister is, this is what the packaging scenario looks like for ATR fuel. So essentially there is a Type 1A basket that's loaded with 10 elements of ATR fuel. Three of these baskets are stacked on top of each other within a 15-foot high, 18 inch diameter stainless steel canister. That's then essentially back-filled with helium. For kind of speeding up the model I just use a symmetry across the center, really only modeling half of the canister.

For the chemistry model we use a gas phase chemistry, those developed by Wittman and Hansen at PNNL, that models 40 species with 110 chemical equations. This is then coupled with the surface chemistry of the corrosion layer to model the hydrogen and total pressure accurately. In terms of the solid chemistry right now I'm using updated G-values from Greg and his Task 2 work that are specific to helium. Right now there is no chemistry that kind of accounts for what Anna was just talking about as far as say an equilibrium state being reached. The other chemistry is fitting some of Ted's data from Task 1 making a conservative assumption that this mass velocity we’re seeing was due to essentially release of some water from the corrosion layer. Then also assuming just a general rate of corrosion of aluminum in water for some small contribution to hydrogen. The chemical model with these built in was then built to replicate the Task 1 and Task 2 experiments just to check that it's correctly predicting those results.

For the chemistry what we really wanted to take a look at was using a fully coupled CFD model, but what we found is when we started to dig into this chemical model, so this is the chemical pathway for just the oxygen molecule for all the possible things, it was just rather complex to resolve within a reasonable simulation time. What I switched over to doing was resolving the thermal field within the CFD model over that 50-year period, extracting the temperature data to come up with a five environment temperature distribution model that would essentially decay over time as the decay in the fuel decreases, and then this distribution is fed into Cantera for the full chemical model.

So for initializing this whole model we need to kind of determine well what range of parameters and conditions to utilize. For the ATR fuel we looked at our current wet storage inventory which has average decay rate of 18 watts, 12 watts standard deviation. So just kind of picked one standard deviation below and two above because of this large kind of tail of that inventory. With a decay heat with a cesium half-life and a dose rate based on that decay heat on the surface of the fuel assumed for the entire canister to be conservative.

For the external temperature we assume that these canisters would be in a site that's similar to our dry-storage site, so we use those temperatures which will vary from all the way down to 0, all the way up to 35 Celsius seasonally. And then we looked at the residual water content within the canister just from 1 to 100 relative humidity. And then we considered oxide film thickness with some of the work Phil had done I think that's on the lower end. Early assumptions were that this was 34, some of his work showed about a maximum of six microns and some of the work done by Savannah River shows somewhere in 5 to 15 microns depending on the fuel so just kind of pick this 5 to 15 as a good variation for the oxide thickness. And then we added some cases just assuming that the back-filling isn't quite complete just based on what the pressure is before the backfill.

So, this is showing the results for the ATR fuel for the sealed canister. So, on this left this is showing the lower, middle, and upper baskets that are stored within the canister where we see the hotter temperatures essentially in the middle and then going from top to bottom is the low to upper decay heat of the fuel. So, this ranges from 35 degrees to 82 degrees. So, we extract the temperature profile from these simulations to come up with this histogram of temperature environments that's fed into Cantera and extrapolate that over the 50-year thermal history. And then we have between those environments we take the average recirculation that's occurring within the canister as a mass flux between the environments within the Cantera model.

So, I’ll move on to the chemistry essentially for our nominal case. So, we consider that the oxide in the corrosion layer is boehmite, that's basically fully dried from nearly 1 to 1 water to alumina ratio within that matrix and then just for the, nominal case of 18 watts of decay heat and 10 micron thickness. So on the right here this is showing the full breakdown of the species present, the only species that we see in any significant quantities are the initialized water content, hydrogen and helium. Anything else is present in less than PPM quantities and specifically one of the ones here is oxygen is basically less than a ppb, so there is no oxygen present within, in the canister. By the end of the 50-year period the evolution of hydrogen within the canister increases it from initialized pressure, 1.2 total atmospheres up to about 1.36 and after the 50-year period the total hydrogen content increases to about 7%. Just for reference as far as the pressure that we're concerned with, the mechanical rating for the DOE sealed standard canister up at 500 psi or about 34 atmospheres and the model showing where down at 1.3, 1.4 so we're, an order of magnitude below any sort of limit for the any mechanical failure of the canister from pressure.

Next we'll just show some of the results of the variation that we considered in the parameters space. So when we look at the thickness, which is just showing blue here, we see very, very small changes with the hydrogen content. So this is the way that we built the corrosion layer into the model is that the G value is dependent upon the bulk of the sample rather than just the thickness of the oxide layer. There is a small dependency on pressure just because we allow for some physi- or chemisorbed water content to evolve from that thickness. When we looked at the hydrogen with a variation of the decay heat here is where we see largest differences within the model. We look at our maximum case where the loaded decay heat is 42 watts per element. We see the hydrogen increases to about 16% max and our pressure increases to about 1.5 atmospheres. We're looking at the low decay heat case. This is 6 watts per element for the ATR then the total hydrogen evolution is only 2.5% and the total pressure buildup is only 1.29 atmospheres. And this canister essentially by this design specifications…it starts out at 1.24 atmospheres so 5% change in the total at atmosphere… It's um rather negligible.

Then the next thing I wanted to talk about is well if we look at say going from this kind of nominal case to cases where we have less than ideal conditions so one of these would be residual 1% air in the canister essentially back filling with helium isn't entirely complete. So when we considered this case we don't see any changes to any significant changes to hydrogen formation or total pressure content within the canister. We see some NOx compounds present but they are relatively small. Most of the nitrogen and oxygen that's initialized within the canister ends up as nitric acid, for the nominal case. We see this buildup of nitric acid to about 1500 ppm. For the high decay heat case this could potentially build up to 3000 ppm. We’re not looking at the model from any sort of nitric acid corrosion perspective but based on some of these results I've seen in literature from dilute nitric acid concentrations these values are not high enough to cause significant corrosion on the aluminum or the stainless steel.

So the next kind of non-ideal case that we looked into is essentially that there is residual pseudo-boehmite as the corrosion layer. This is essentially a case where there is insufficient drying, there is a 2.5 to 1 ratio of water to alumina within the corrosion layer, so this is a fully saturated corrosion layer that undergone no drying. When we look at this case just from the general thermal dehydration of the corrosion layer from the decay heat of the fuel over the first couple of months of the model this increases the initial pressure up to about 2.2 to 2.3 atmospheres, about an atmosphere higher from the case where the fuel is considered fully dried. We don't really see any other species such as hydroxyl or peroxide present in large quantities when we consider this case. We also don't see any large increase in the hydrogen, in fact this is actually a slightly lower amount of hydrogen just because all this kind of residual water gets introduced into the system. So this comes out to just over 6, 6.5% after a 50-year period. We also see when we look at our pressure, this is still an order of magnitude lower than the max rated for the canister. And when we look at our species breakdown still see negligible amount of oxygen for this scenario.

So moving on to just the variation, so we see a larger spread in the total pressure that's predicted under this scenario. This is just because of that, all that residual water content that's released, and we see a spread within the pressure within the oxide thickness. In terms of the hydrogen content, we don't see any large changes under any of the scenarios we looked at compared to the other cases. The other thing that we see, within the water content, with higher water content there is a lot more temperature dependent reactions that occur. So, this leads to kind of a seasonal fluctuation within the model predictions as the external canister temperature is varied from that 0 to 35°C seasonally.

And the next scenario would be combining these two non-ideal cases where we got a fully saturated pseudo-boehmite corrosion layer, and a small amount of residual air. Under this case we still don't really see any changes in the major compounds that are present so it's really just the nitric acid that occurs in an appreciable quantity. So, for nominal case this is about a thousand ppm and then for a high decay heat case um this ranges up to about 2000 it is ppm, those are lower than initial residual air case just because of the additional water content that's been introduced.

So moving on that's all of the results for the consideration of ATR fuel. We wanted to look at a couple other fuels that are stored by DOE. So, the first of these is the MURR fuel which is stored in a significant quantities. This has a very similar storage configuration to the ATR, so it is expected to be packaged in the same Type 1A baskets except they are cut to a shorter length, so these are stored in 10 foot tall canisters again with three Type 1A baskets stacked on top of each other. This storage configuration and the canister has very similar surface area to volume ratio as a packaged canister of ATR fuel. And we kind of selected a sensitivity of the decay heat as 25 watts for the maximum and 13.4 for the nominal and 5 for the lower end.

So, this is showing the thermal CFD profile for the MURR fuels. So again, this is just showing the central lower and upper bounds for the decay heat as well going left to right just showing the bottom, middle and top baskets that are packaged within the canister. Max temperature we see is about 85 degrees and the low end is about 43 degrees, very slightly warmer than the ATR on the low side, but not a whole lot.

Just as before we come up with this 5-environments temperature model that is fed into Cantera, this is just showing the average temperature within the canister over a 50-year period and then this is coupled between the environments based on the recirculation within the canister. So just kind of dive into the chemistry here. So just for this nominal case we see a very similar trend to what we see within the chemical case for the ATR fuel except everything is slightly lower. So for this nominal case there is still no appreciable amount of oxygen. The pressure increases to about 1.34 total atmospheres within the canister and the hydrogen increases to about 6% total.

And then moving on just going to show the additional kind of non-ideal scenarios for the MURR packaged fuel. So first showing the high decay heat cases, so how this increases while it still shows increases when we go to higher decay heats this isn't as large as the increases seen with the ATR fuel, so here the maximum hydrogen content is just under 11%, the max pressure is just a little bit over 1.4 atmospheres and the max nitric acid only about 2300 ppm, still see the same trend where we don't really see any large dependencies on the oxide thickness.

So just to move on to the other fuel that we looked at was the HFIR fuel. So, we thought this would be an important fuel to look at as from the perspective of surface area to mass this particular fuel has a very, very high surface area. The DOE specifications for the HFIR fuel is to split the inner and the outer annulus into separate storage canisters so the HFIR fuel is stored in when a 24 inch diameter canister, three 6b baskets stacked together, and then the HFIR inner element is stored within 18 inch diameter canister with type 6a/6b, 6a baskets. Of note, these configurations for the storage have significantly lower surface area to free volume ratios than the ATR and the MURR packaging configurations, and you can kind of see this here as I look at the meshed configurations and the blue is just showing kind of the air so there is a significant quantity of air basically in the center of the canister and the outer area when these are packaged.

So, this is showing the thermal results for both the inner and outer HFIR fuel elements so because of that, those large gaps of air, the HFIR packed DOE canisters end up with about a 10 to 20 degree colder temperature field than that of the ATR fuel. So, this upper is showing the upper nominal and lower decay heat for the HFIR inner and then this bottom is showing the same for the HFIR outer element storage.

So moving on to the chemistry for the HFIR what we see is that the maximum across the cases for both the inner and the outer HFIR elements is very similar so when we're looking at the maximum for the upper decay heat this goes to about 12% in both cases, about 1.4 atmospheres of total pressure when the fuel is fully dried about 2.2 atmospheres of total pressure for looking at undried pseudo-boehmite case and then about 3500 ppm of nitric acid when we’re looking at a case where there is small amounts of residual air. Of note, still like the other cases we still don't see any appreciable amount of oxygen within any of these scenarios. And while the HFIR has a very large surface area in comparison to the other fuels, we still don't see any appreciable sensitivity within the modeling to the surface oxide thickness.

So just to summarize the results of these three fuels, when we're examining the worst-case scenario for the ATR, that scenario bounds the MURR, the HFIR outer and the HFIR inner storage scenarios, for say a nominal case. We actually see the HFIR storage configurations have a very slightly higher pressure and hydrogen content than the ATR. This is somewhat due to the assumptions that were made for the decay heat, the ATR inventory was just, ATR decay heat was based on a fuel inventory whereas the HFIR was just based on a tabulation of years after post-reactor removal. We also see a little bit higher nitric acid formation within the HFIR because there's that higher free volume, there is more air and lower temperatures which lead to nitric acid essentially forming a little bit higher concentrations in that scenario.

So just to conclude, for the scenarios that we see most of them exhibit significant hydrogen and while it is above a lower flammability limit, there is no scenario that has any case with any appreciable amount of oxygen. In fact, when we are looking at our low decay heat cases, the hydrogen content after 50 years is lower than the 4% flammability limit. So, this is 6 watts for ATR, 5 for MURR, 21 for the inner HFIR, 39 for HFIR outer. In reference that's about 25 years of after reactor removal and about ten years for the MURR and the HFIR elements. If we consider a case where there is one percent residual air, there is a range of possible values from 500 to 3000 ppm of nitric acid. However, this low and dilute concentration would be roughly a hundredth to three hundredth of a mil per year on stainless steel so it should be minimal corrosion. Also see our worse-case ATR fuel scenario bounds the other scenarios for the MURR and the HFIR fuel for the hydrogen and the pressure, so in reference to that 34 atmospheres for the max canister pressure we see with the ATR is a nominal case of 1.36 atmospheres with 7% mole fraction, mole percent of hydrogen. For the high decay heat, ATR case, this is increased to 1.51 atmospheres total pressure and 16% hydrogen, if we're looking at essentially a worst-case scenario where the fuel is completely undried. Then, this increases to about 2.6 atmospheres and 15% hydrogen. The other thing that we see from the modeling is that any predicted changes that the oxide thickness has on the outcome are rather small so the characterization of the oxide layer on an individual fuel element basis is probably unneeded as long as the fuel is intact.

Just over the next kind of year and forward some of the things we'll be looking at update results if we see updates from Greg's experiment showing differences in aluminum 6061 versus aluminum 1100 for the G-values for the hydrogen. Potentially extending the modeling timeframe beyond 50 years essentially to a final equilibrium state for the canister. And adjusting the model based on some of the results from Savannah River with long-term radiolysis testing so specifically some of this leveling off in production as well dried sample showing lower hydrogen evolution rates are two things we can look at building into the model. And then setting up the model for a direct validation of some of the instrumented test on a longer time frame.

For some of the kind of nitty-gritty details here is just a list of the published reports that are all externally available and then we have a journal article that's under review now.

So then this was funded by the Environmental Management Office and high performance computing center, INL. With that I'll take any questions.

>>JEAN BAHR: Okay thank you Alex, we got about ten minutes for questions. You emphasized the low oxygen concentrations and I think there was a question earlier about where does the oxygen go, and it would seem that with your chemical modeling you should be able to actually tell us where that oxygen is being sequestered.

>>ALEX ABBOUD: Yeah so, so in terms of the oxygen basically it's assumed that only hydrogen evolves from the corrosion layer and any oxygen that's in the corrosion layer basically remains there.

>>JEAN BAHR: So that's an assumption of your model, that's not a --

>>ALEX ABBOUD: Yeah.

>>JEAN BAHR: Something the chemistry shows, okay.

>>ALEX ABBOUD: Yeah, but it's an assumption that's made based on the experimental data since they aren't showing any oxygen evolution in anything that has been run on INL or Savannah River um there is not really, there doesn't seem to be a reason to build oxygen evolution into the model because of what they see experimentally.

>>JEAN BAHR: Okay. Paul Turinsky?

>>PAUL TURINSKY: I have some short questions and then a longer one at the end. Your CFD model is it a course media model that you are using?

>>ALEX ABBOUD: No, it's fully resolved so it's --

>>PAUL TURINSKY: So fully --

>>ALEX ABBOUD: Well so it fully resolves the fuel, I guess it doesn't fully resolve the fuel plate, so the one assumption it has a lumped solid for the fuel plates itself, so it doesn't resolve the aluminum-cladding from the fuel meat, but other than that everything else is fully resolved for the air gaps between the fuel elements.

>>PAUL TURINSKY: Okay, I see why you are involved in this on that. Uh, are you modeling the change in the gas composition and pressure with time in the CFD model?

>>ALEX ABBOUD: So I, not anymore. So I originally started out that way um but then once we kind of figured out where um the pressure increases were so low there didn't really seem to be much of a reason to do that. Especially since some of the stuff I hadn't talked about here but it is in one of the earlier reports, showed the sensitivities in the gas phase chemistry to the internal pressure of the canister. And that showed rather negligible results.

>>PAUL TURINSKY: Okay another short question. Have you just considered just using like a [COBRA] type model which for the commercial side they have a special [COBRA] model for basically dry-storage.

>>ALEX ABBOUD: Um yeah I have not that familiar with [COBRA].

>>PAUL TURINSKY: Okay it has been, to be pretty good. Then the really tough question you are going to have to answer because many, many people are going to count on these calculations to make safety related decisions is the verification and validation. Is there a verification and validation manual somewhere and how good do you feel about the current state of verification and validation of your models?

>>ALEX ABBOUD: Um in terms of that um I think I’ve at least verified some of it by, you know, comparing them all directly to some of the tasks to data for the long-term I think that's going to the validation is basically going to have to be done with some of those instrumented lid tests in the future. From say the long-term results in the verification/validation from a safety perspective I think that the model as constructed should actually bound the safety, if anything I bet that once we get results in, the model is probably going to be over predicting everything.

>>PAUL TURINSKY: Yeah, yeah, because under evaluation you can then develop basis uncertainty and you can do UQ analysis also which I didn't see any, any brackets on your results [LAUGHTER] to indicate --

>>ALEX ABBOUD: Uh, yeah, yep I've got a GCI test that's in one of, well yeah one of the reports. One of the sealed reports there is a GCI test about how the temperature changes with mass. That part of UQ has been done and then some of the variation with regards to pressure or external canister temperature were examined and have produced small changes. It's, yeah that stuff is just kind of been left out of how things are now just because everything has been shown to have such a small effect except for the decay heat.

>>PAUL TURINSKY: Okay, all right. Thank you.

>>JEAN BAHR: Tissa?

>>TISSA ILLANGSEKARE: Yeah, thank you for, so Paul so ask several questions I had in mind. But I need clarification question. So you mention that you sort of thought about doing a coupled model so you look at the chemistry and, you are right they are not easy to couple, so we understand is that you first saw the CFD model for the 50 years and generate thermal evolution and then you input the chemical model. Is that correct?

>>ALEX ABBOUD: Yeah.

>>TISSA ILLANGSEKARE: Okay so the question is that if it is to happen, what are the feedback mechanisms you are really neglecting in the process of decoupling the problem and then to follow-up with that is have you thought about doing some sort of sensitivity analysis even for a hypothetical case to

see whether the decoupling has, what the effect of decoupling on like a special more concerned if you are going to, more than 50 years some of these coupling processes may become important. Have you done any sensitivity analysis to look at these coupling and decoupling, what is the effect the final predictions.

>>ALEX ABBOUD: So I, I think the only effect that I would be missing out on directly would be the pressure increase. One of the early sensitivity tests that I had done was actually on the pressure just because of the back-filling procedure has variation in the canister initial pressure. So then that could impact temperature fields, effect equilibrium and all that within the canister. But the effect of pressure within the range of increase I predict with the hydrogen generation are small enough that the temperature effects are only about 2 to 3 degrees different. And that 2 to 3 degree difference was actually a second sensitivity test, it was actually plus or minus 5 um that showed hydrogen content varying by 10s of ppms with temperature. It's mostly again dependent upon the dose rate.

>>TISSA ILLANGSEKARE: So that you answer the question to Paul, but in the validation the only way you can validate this model is through the canister put together because you are, you can validate the model but coupling you can only validate through that data set that you expect with the full canister, is that correct?

>>ALEX ABBOUD: Um yeah, well won't have a full canister um so actually I haven't seen Evans presentation so I don't know if he is going to talk about the instrumented lid but I believe it is actually only like a third canister eventually. So it is just one row of elements, but it's should be similar enough that it should work for a validation case.

>>TISSA ILLANGSEKARE: Okay, thank you.

>>JEAN BAHR: Dave Shoesmith and this will be the last set of questions.

>>DAVID SHOESMITH: Hi Alex, congratulations on what is a very complicated model, but I'm sure you know with all models the question is if you don't know something um can you think of a realistic way of putting it in there?.. into

your model. And then the second [inaudible] which is essential, is have I made a conservative assumption? So a thing that's not known here is the fate of the oxygen. So what you have done is -- a mechanism by which the oxygen is removed by reacting reactively with oxide and aluminum. But also in your model what is, the metal is infinitely available, irrespective of what is on the service or in the bulk. It is not clear to me that's a conservative assumption, even though the reaction which is not been identified may be realistic. So I'm a little concerned that you haven't necessarily captured that in conservative assumption at this time. I'm not sure what I would suggest you could do about it, because I'm not sure but I think at some point there has to be looked at carefully. It is a critical feature here is you don't want the oxygen to be released as a gas in the presence of the hydrogen and therefore where it goes becomes a critical feature of viability. This is more of a comment than a question, so I'm not, I'm not familiar enough with the process to suggest make any meaningful suggestion about how you might…, you are relying on empirical observation. A real observation which is no oxygen forms in experiments but unreal, realistic assumption necessarily of how, where it is gone. I think it needs to be resolved.

>>JEAN BAHR: Thanks I think that was -- trying to get at as well.

>>ALEX ABBOUD: Yeah certainly I'm, I mean I think that would from a modeling perspective with the data that we have I don't think there is really a clear answer of where to go with that without any sort of experimental data to resolve that.

>>JEAN BAHR: Okay. Well with that I think we'll move on to our final speaker for the afternoon, which is Evans Kitcher from Idaho National Laboratory, going to be talking about wireless sensor development. And do we have -- there you are. Okay. And are you controlling your slides or --

>>EVANS KITCHER: Yes, ma'am.

>>JEAN BAHR: Okay, well go ahead.

>>EVANS KITCHER: Okay. Can you see my slides?

>>JEAN BAHR: Yes, we can. Thank you.

>>EVANS KITCHER: Okay, excellent. As Jean said my name is Evans Kitcher, I'm a spent fuel analyst here at Idaho National Lab. Today my presentation is on development of remote monitoring system for dry-storage um [LAUGHTER] we refer to it as the instrumented lid project, there has been other references to another Instrumented Lid Project, and I should clarify that these are different but the one that referred to prior is looking at instrumenting a mockup of the, of the DOE standard canister whereas my project, what I'm going to talk to you about now is regarding instrumenting an existing canister which is being stored here on site, in the 603 facility.

So just quick background I guess I can skip a few of the first slides, kind of goes back through what has been already talked about by Josh, in particular and then some of the other presenters today which are the issues that are facing extended dry-storage of aluminum-clad spend spent nuclear fuel and then where this remote canister monitoring system fits in to try to address some of those gaps. Specifically, some of the need for experimental data that can support some of the modeling that Alex is trying to develop and has talked about. We'll look at efforts in the past to implement something like this. Interestingly enough this has been tempted back in 2000s time frame so it looks like this idea of collecting real data from real facilities to help validate models has been a challenge for some time. I'll talk about our current activities, which is looking to develop the system we have identified three approaches, we have done some feasibility studies with Westinghouse and Idaho State University as collaborators. Then where we are now and what we plan to do in the coming year.

So as already been kind of discussed there is an obvious need for more complete understanding of this ASNF behavior provide a technical basis for continued storage of this material as Josh mentioned almost four or five hours ago. We have been storing this material in configurations that have been licensed for 20, maybe 40 years and now we're looking into the future and seeing that there may be a need for continued storage of this material. So we need a technical basis to either say that it is okay to continue to store it and manage it as we have been or if there are any changes necessary and also to inform future configurations either for transporting, conditioning, for example, the drying processes that have been discussed and then eventual disposal of this. So, in 2017 these five knowledge gaps were identified as has been mentioned already.

So, this when is the oxyhydroxide layer behavior and chemistry that radiolytic gas generation, the effect of these different combine phenomena on this radiolytic generation of hydrogen gas performance of the spent fuel and dry-storage systems and then the effect of high temperature drying. So, we believe that a remote canister monitoring system can help to address this issue. Specifically with respect to the radiolytic gas generation combined phenomenon test as we were just discussing at the back end of Alex's presentation, only real way to look at if you're missing out on, you know, the multi-physics that happens in real life or when you have to make simplifications in your models as to perform these kinds of tests, and hopefully if we are successful in it implementing this monitoring system we can help to speak to some of that. It provides us the opportunity to also evaluate what will be appropriate technologies for monitoring. The system that we're looking at is a vented system, so it is not sealed, and so that's what we're looking at, but we also need to look at what will be appropriate for potentially monitoring this fuel in a sealed system such as the DOE standard canister. As has been discussed these results can help for lab skill results and analytical and simulation modeling approaches and then also we could potentially identify additional dry-storage options here at INL. So I'll talk about that in a bit more detail but basically continued operation of ATR, we're going to need more storage options for this fuel as ATR continues to produce spent nuclear fuel.

The parameters of interest for the RCMS (remote canister monitoring system), temperature relative humidity, hydrogen gas concentration and then the radiation environment which we're basically going to use dose as an analog for that. So there is various types of aluminum-clad spent nuclear fuel here at INL. The majority of this by inventory is the Advanced Test Reactor spent nuclear fuel. Currently, this fuel is placed into baskets, transferred out of the ATR canal and dried and put into storage at CPP-603. You'll hear it referred to 603 or IFSF which is the irradiated fuel storage facility. This is where a majority of the ATR fuel is currently stored.

The facility was already, was originally built to store Ft. St. Vrain fuel, so the Ft. St. Vrain reactor is a high temperature gas reactor type, high temperature reactor using TRISO fuel, so a third of this capacity was filled up with Fort St. Vrain fuel before the shipments were stopped and the remaining spaces inside the canister array has been used to store other types of SNF mostly ATR. So, in this image here you can see a rack and a canister. This is a metal frame rack and the canister suspended from a flange at the top of the canister, so this allows airflow around canisters for cooling. The temperature and environment inside the fuel storage area is not controlled, so it basically oscillates with ambient conditions out here at the INL site. The temperature ranges are what you would expect to see in the desert, and then relative humidity and other conditions are also what you expect to see out of the desert.

Here is a layout of the facility with image looking at inside the fuel storage area. You can see there the array of canisters and the goal or objective of this project is to come up with an instrumentation system that can be deployed inside this fuel storage area on top of one of those lids to measure the conditions that the fuel is currently experiencing in that facility. So here you can see basically again a layout. This is the permanent containment structure, this is where fuel would be brought in, in a cask, lowered into this tunnel and transferred into fuel handling cave, repackaged using baskets into one of these canisters and put inside the fuel storage area.

So, the canisters that are used in 603 are primarily carbon steel or stainless steel canisters. They have 18 inch diameter which is the same as the DOE standard canister, at least one of the variants and normally 11 feet long. The DOE standard canister has two variants so this would be more like the short, small version of the DOE standard canister. Significant difference is that it is not sealed as is proposed for the canister for long-term storage and eventual disposition, and it’s held in place by a clamp. The lifting bale allows us to manipulate the canister. So the proposal is to instrument one of the carbon steel canisters; this is because they represent a majority of the canisters that are being used in the facility, and the idea is to instrument possibly a new loading as this is, you know, currently on going operation and it would be more feasible to instrument that canister.

So, as we have already heard a lot of these aluminum-clad fuels are made out of plate type fuel so some sort of aluminum-uranium matrix and these are in the form of plates, either straight plates or curved plates with aluminum I guess assembly and then this is highly-enriched uranium, about 1kg or 235 per assembly and for extended dry storage the fuel ends are cropped. We heard about some of these cropped pieces from MURR and other types of experimental reactors providing good samples where we can do some of this work but same thing for ATR once the fuel is declared spent it is cropped and placed inside a bucket and that's the bucket that's used to handle the fuel from then on.

Here we have an image of the ATR fuel element so you can see it is curved and then with the end fittings cropped. Inside 603 we have various loading configurations for the fuel and so as mentioned we're targeting the RCMS to be deployed with ATR canister and this is because again ATR is very representative as it is just a large portion of the inventory.

So, there is three types of bucketed buckets used inside these canisters for ATR specifically. This is the ATR4 bucket shown at the bottom left, ATR8 bucket shown in the middle, and then there’s the HFBR bucket or MURR bucket or other short bucket to hold other types of aluminum spent nuclear fuel inside the same canister. ATR buckets are 50 inches high, and both the 8 and 4 versions are in service. The HFBR bucket is a lot shorter and can hold six different elements, and sometimes these are loaded and sometimes the space at the top of the canister is just left open. So, you can see that in the center we have the ATR8 bucket. This is the original configuration which the fuel was stored. However, with capacity, considerations and the desire to store more fuel the ATR4 bucket was designed which fits four ATR elements inside the same space where two are previously spent. And so, this is the internal geometry that we are trying to instrument and monitor.

As mentioned, the canister has a loading where you can load a lower bucket and an upper bucket, so these are the two ATR buckets either in 8 bucket or 4 bucket can go in these locations and then at the top the shortest space has the potential to host the HFBR bucket and as mentioned that is only loaded as needed. Basically, to deploy a system that would be placed on the top of this lid and then possibly run instrumentation into this geometry to monitor the conditions inside this canister.

So just some previous efforts… So here in the image you can see a mockup of a previous system that was developed in the 2000s time frame, and it basically has a shielded compartment. The shielding is made out of proposed to be made out of tungsten, and that’s to provide the shielding while minimizing the weight and also the size as the real estate on top of the lid is limited. And so all of the electronics and detectors and other things that would be needed would have to be inside the shielded compartment and then it would need a way to wirelessly communicate the data that it is collecting out of the fuel storage area to a data acquisition system which would most likely be in the control room at the facility. And at that time, they also intended to measure the same things that we are intending to measure now, which again temperature, hydrogen concentration, radiation dose and the idea was to use radio-frequency transmission to get the data out of the fuel storage area. The shielding needs to survive nominal 500 hour per hour gamma radiation field and this is the, this is the dose that was measured above the fuel elements, sorry above the canisters inside the fuel storage area so anything deployed inside the canister would most likely be much higher gamma radiation field. The prototype was just left in the conceptual design phase leading to this mockup that we have here; it was never actually deployed or radiation tested. For reasons that I was not able to determine from the documentation that was available.

Our current efforts based off the system requirements that we were able to review and what we came up with we determined that there's three different approaches to arrive at this RCMS that we think that we need. The first and most straight forward approach is to develop technology update of the prototype that was previously developed, and this is basically to use the same measurement and data acquisition approaches and basically just update the components using newer equipment technologies. As you can imagine in 20 years there's, you know, faster, better, smaller, more lightweight, more reliable electronics that can be used for such a system. This allows us to acquire point data and so the old prototype basically used in air pump to take a sample of the air in the vacuum at the top of the, at the top of the canister and so this would allow us to basically take sample and that sample would have to be representative of the entire canister. As I mentioned 11 foot canister and so, you know, there is questions as to whether or not such a measurement would really be representative.

The other thing, the other approach or approach number two would be a wired solution, what we're calling the wired solution and this would be our first attempt at introducing sensors into the canister geometry. As I've shown obviously with the ATR8 bucket there is a lot more space in the geometry for something like this, but with the switch to ATR4 buckets instrumenting that geometry is going to be a lot more difficult. Threading of thermal couples into the geometry also represents a difficult remote operation for the operators of the facility to perform. The benefits of this approach would be that we could acquire spatial information, so potentially we could instrument the lower, upper and top canisters and so we would get three data points instead of one; and if it was successful maybe we could try to, you know, get some sort of axial data distribution to validate, you know, the models that for example Alex is showing you the temperature distribution, hydrogen distribution, et cetera.

The third approach, …this came to our I guess came on our radar from work that was being done by Westinghouse for wireless or passive sensing of in-reactor environments and so we thought okay if we can get a wireless sensor that could be resident inside the canister, this would answer a lot of the hesitations that we have with trying to thread thermocouples and other sorts of instrumentation into the canister. It could have potential benefits for maintenance and facility operations, and it would still have the benefit of allowing us to acquire spatial information inside the canister. So here is just a schematic that shows basically the concepts. So the first approach, which is just technology update is to have a shielded container at the top. Can you see my arrow, my cursor?

>>JEAN BAHR: Yes, we can see it.

>>EVANS KITCHER: Okay so the, the first approach, sorry, is to have this shielded compartment here where the electronics would lie, have a penetration that goes through the lid that allows us to collect point data from basically this cavity at the top of the canister. The second approach would be to have either a deployed sensor system that could communicate with the wire that is threaded into the canister geometry and then the third option is to just have passive sensors that could be placed either internal to the actual canister or maybe inside, or affixed to the buckets that hold the fuel and this would then communicate passively wirelessly to this instrumentation and this, this canister would then send that information out of the fuel cell into the control room.

So, so far for approaches 1 and 2 we performed a selection and procurement of appropriate sensors and key system components. We have done some separate testing of these individual sensors for performance and calibration. The testing has been done in representative environmental conditions with respect to temperature relative humidity and hydrogen concentration, we were unable to complete all of the irradiation testing that we had planned due to delays associated with COVID and working with ISU. And then we also managed to test transmission of the collective data to a data acquisition system wirelessly.

So with ISU, as I mentioned we just did a search for commercially available components, these components, an example of these are shown here. Basically what we found is that there is a whole myriad of options for each component we thought to use. Basically our options were many, our challenge was just to pick one, verify it had performance we want to integrate and put it into an integrated system that we could deploy. Here is an example of some of the components that we ended up identifying and selecting.

So in order to test these components we fabricated a test volume dimension test volume is shown in the schematic; it is not very big. It's actually quite small and really this was just to allow us to verify that the components had the performance that we expected and that before we went to integrate them into a system that could be used together. So the test volume has penetrations on the lid, the lid is clamped shut in the same way that the actual lid on the canisters are clamped. And it allows us to basically purge the system, introduce water or into account for relative humidity, have hydrogen and air or hydrogen and Argon mixtures that we can use to measure, and then we had a pressure relief valve for safety and we can also purge the system.

Here is a 3-D schematic of the test volume.

And this shows the test volume inside the lab at ISU. So once this test volume was fabricated we purchased various concentrations of hydrogen in Argon and used that to fill the test volume. Here you can see the test volume is inside a heating, heating apparatus by which we can control the temperature inside the canister. Through some of the inlets into the top canister, we were able to insert a thermocouple which we use to verify the temperature we are seeing from the controller. As I mentioned we were able to do individual parameter testing so we tested for temperature just on its own and then we did temperature for just hydrogen concentrations at various levels. I believe 100ppm, 500 ppm, 1, 2 and 3% of the hydrogen and Argon mixture. We also did relative humidity. We did not really have a good way of controlling exactly the relative humidity, so we introduced basically a some sort of wet air mixture and then just tried to measure what the relative humidity was in there. The sensor module was successfully programmed to collect this data and transmit it wirelessly in a CSV format. We were happy with this outcome.

As I mentioned the third approach was done in collaboration with Westinghouse. Westinghouse were developing this technology that would allow them to passively sense in-reactor environments. We were interested due to, you know, it being electronics that was designed to survive a high, high temperature, high radiation field. Which is um, well we don't have high temperature but we do have high radiation fields so we were interested in that. So we contracted with them to basically develop something that could be used for, for our needs. They developed a CAD model for all these components that they identified would be needed in the system and made sure it fit on the real estate on top of the canister as we described. So, as you can see here the model has the tungsten shield, they have two different enclosures, they have a pressure sensor, a hydrogen sensor, radiation sensor, battery bank and a circuit board for all the electronics.

We also commissioned them to perform some irradiation testing of the components that they identified. So, they irradiated various types of lithium-based batteries for… at the dose rates shown there and they took it to what we expected to be the lifetime of this prototype which was two years inside the cell and so they irradiated it to a total dose which was equivalent to two years exposure at that 500 per hour nominal dose rate we gave them.

They also performed irradiation tests of the in-canister hardware they were proposing to use. So here we have the in-canister hardware printed circuit board and then the data acquisition system and showed even in the presence of that irradiation field they were still able to transmit the data. The key feature of this technology that they are proposing to use for this is an inductance capacitance circuit which basically has a resonant frequency response that is proportional to whatever parameter it is you are trying to measure. If it is temperature then the circuit would passively resonate at some sort of frequency that is proportional to the temperature. And so this resonant frequency signal can be they’re proposing to transmit it in when two different ways either using RF signal or using an acoustic signal. In order for us to validate this theoretical approach could work, we ask them to do a mockup of this and so with the designs of the canister that we provided them they were able to do a full scale mockup and to have a microphone inside the lid that is designed to collect that acoustic signal, and then they had an acoustic generator at the bottom of the canister to simulate that signal that would be produced by the sensor. They filled up the canister geometry with metal, just scrap metal to represent the interference that you would expect from a loaded canister and then we gave them information on the penetrations that are in the bottom of the buckets as they felt these penetrations would be instrumental and actually being able to get a signal through the canister geometry. From their initial test and what we commission them to do they found that with some fine-tuning this approach could be viable.

So what, what did Westinghouse do? Basically they identified components, purchased those, fabricated mockup I showed you. Completed irradiation testing of in-canister components to show they can survive two year dose at 500R per hour, and completed temperature and humidity testing of some of the components. And verified transmission using radio frequency or acoustic signals was indeed viable.

So conclusion and path forward ..again just to reiterate we believe that if we're successful in developing this system it could provide data that would help us in our search to better understand aluminum-clad spent nuclear fuel in these different dry-storage configurations. Specifically, for us the current dry-storage configuration, not the future configurations that would be used for disposal such as the DOE standard canister. The feasibility studies looked at three different approaches, which was the revamp of the old prototype, a wired approach and a wireless approach. Our conclusions from these studies is that all of these are, you know, are feasible and so we have decided to proceed with the wireless approach in collaboration with Westinghouse as it offers some additional operations and maintenance advantages over the other two approaches.

So our current activities, we've just put in place another contract with Westinghouse to refine and develop this wireless approach …they will be making sure that they can, they've done, you know, single effects test and so now they are going to integrate those components into a single thing and then test it under combined environment conditions. So radiation, hydrogen concentration, temperature and relative humidity. And then on our side here at INL, we will engage with 603 facility, which is operated by EM and the EM contractor there to figure out what are the operations requirements that are going to be on this prototype. We also plan on doing some analysis to make sure that we can fabricate and build this thing to the requirements that would allow us to actually deploy it inside the cell.

And with that I will take any questions. I should also mention that there are few reports that have come out of this work. I forgot to add them here but I can provide that to the Board and also to the public. They have all been cleared for public release, and so there are some reports if anyone is further interested.

>>JEAN BAHR: Thanks, Evans, this seems like a really exciting and promising approach… get my video on there. Do you have an idea of the timeline for completion of actually getting, getting this done now that you've decided on the wireless approach and are moving forward?

>>EVANS KITCHER: So, what we have proposed is to go through a, as I mentioned an integrated effects test of a prototype and so we are going to engage with Westinghouse to have a prototype of this system fabricated this year. And by the end of the year we should have a working prototype we demonstrated in the lab and then also the requirements for fitting an actual prototype that can actually be deployed inside the facility that's been made to all the facility requirements and et cetera established. So with those two things we would be able to move into a bid phase where we could have that fabricated to NQA1 standards and all the other standards necessary for deployment of this thing inside a nuclear facility with the idea we could have something that we could actually deploy in FY23. That is our, that is our plan obviously what happened last year with COVID, those plans are pretty easily be thrown out the window.

>>JEAN BAHR: Sure. How easily do you think this approach, [inaudible] to spent fuel including commercial spent fuel for example.

>>EVANS KITCHER: So the fuel we are trying to instrument and the conditions we're instrumenting them in are very different to commercial spent fuel so the fuel is not as hot and hasn't seen as much burnup and the dose fields are different. In saying that, as I mentioned the technology at least for the Westinghouse one was being designed for in reactor instrumentation, so I believe they have variants that are applicable for high pressure, high temperature and under water operations. So I would say from a technology standpoint should be feasible to develop something applicable for commercial fuel but what we are developing might not directly transfer.

>>JEAN BAHR: Appreciate that. Tissa, has his hand up.

>>TISSA ILLANGSEKARE: Thank you, very exciting work. I'm not too familiar with this particular application but am familiar with the sensors in underground systems so what’s the motivation I assume for while sensing is probably accessibility, is that correct? So kind of you don't have access this system so assume that's the case. So my second question is the challenges in wireless sensing is as in the [inaudible] system, what with the whole issue of signal because if you put a container in the soil and we’re doing some work … looking at this issue, how do you get a signal out because of the radial signal, this is not an issue in your system, is that correct?, just signal on top of your container. Your controller, your --

>>EVANS KITCHER: Yeah so we do have two types of signal issues, so the first issue is the signal that's generated by the passive sensor, can we transmit that to the top of the canister where electronics as I mentioned from the work they have done; so far they think that it's feasible and we have commissioned to continue to search that. The other is getting the data actually out of the fuel storage area so the fuel storage area of course being a high rad area; this area is heavily shielded, and so that is another challenge that we have to look at to see if we can use RF signal to actually get the data out of the cell. I think from a technology standpoint it is feasible; however, there are some security issues with having a wireless network out at that facility and so there are other challenges that are not just technical that we have to overcome if we're going to deploy this, this technology.

>>TISSA ILLANGSEKARE: Yeah, the second, is this issue of power. So the sensors need power, when you have fully…wireless system one of the challenges we have in the subsurface, is how to power these systems. So the question is in your three designs, one of the designs you have container controllers on top of the container so you can basically, you can either do same wiring to get the power in but the question is that how do you in that third design, how do you power the sensors?

>>EVANS KITCHER: As I mention it is a passive system, so the idea that the circuit will oscillate at the resonant frequency that matches the frequency of the data it is collecting and so that's should be passively generated by

the circuit. However, if we have amplitude issues we will need to figure out a way to deploy power inside the canister. That would be a much greater challenge because I don't think they would allow us to put a lithium battery next to the fuel. So hopefully the amplitude of the signal is large enough, but if not then that, yes that would be a challenge that we would have to overcome.

>>TISSA ILLANGSEKARE: So long-term goal seems to be that you should have network of these, because each of the canisters will have its own sensing system that you can actually monitor a larger system. So that is advantage in my view having a system that can identify problem areas in the storage system itself.

>>EVANS KITCHER: Yes, so that is a clear future application of what we're proposing. Right now, our goals are not so grand. We just want to instrument a signal canister and collect data to help support modeling, modeling simulation that Alex is doing. But yes if this is successful you could imagine that we could instrument multiple of these canisters and monitor them for as long as it takes to build confidence that these storage are safe.

>>TISSA ILLANGSEKARE: Thank you for the good work, thank you very much.

>>JEAN BAHR: Okay Lee Peddicord.

>>LEE PEDDICORD: Yeah, thank you very much. I didn't have a specific question but I really wanted to follow-up on Jean's comments that I think what you're doing is really important and really exceptional and I can envision as we discussed lots of possibilities in the directions you can go with this when work beyond the aluminum-clad spent nuclear fuel so I want to commend you all, and I want to encourage you to keep thinking creatively and broadly with what you're doing because I think it can turn out to be so really important and have a wide range of applications. So, I just wanted to pass along that comment and I hope encouragement. Good luck to you.

>>EVANS KITCHER: Thank you Sir.

>>JEAN BAHR: Okay, Dan Ogg has a question.

>>DAN OGG: Hi, yes, Dan Ogg of the Board staff. Thanks, Evans, for the presentation, really good information. First I got a comment and then a question; first the comment is that there is very similar work to this being done through EPRI on commercial spent canisters. I don't know if you are aware of that. Josh Jarrell certainly I think has seen it in ESCP meetings and those kinds of things. If you haven't touched base with those guys you really should; they are looking at many of the very similar types of things with passive sensors that can either be interrogated with a signal and then return signal or using passive acoustic signals to transmit the data. And so again there’s some good work going on, might be great to have conversation with those folks and trade notes. So, my question though is focused on the application of your system, so you are starting with ATR vented canisters that are 603, and that's great I'm sure you'll get really good information from that. But in the long-term is there a thought that if this proves to be viable and useful and you are getting really good data would this then be applied to the DOE standard canister system? You or maybe Josh could address that.

>>EVANS KITCHER: Um should I, I'll defer to Josh maybe? I think right now our goal is to, you know, provide the verification and validation to support the canister and not need monitoring, you know, over an extended period of time. So, I think, we want to right now at least our goal is instrument one or two of these canisters, collect data that will help us to validate that. Yes, we can do this without needing constant online monitoring. But I guess maybe Josh has more to add.

>>JOSH JARRELL: Well actually I was just, echo almost exactly what Evan said. We definitely see other programs out there that have use of this technology whether employing standard canister or commercial spent fuel canister. You know, for this program the focus is on getting data to validate models because at the end of the day we want to load aluminum-clad fuel into a canister and want to do that safely, so that's the focus. But, you know, I again I wear multiple hats so we're always looking for opportunities to leverage the work in future directions whether that's in directly in a DOE standard canister or otherwise, the focus right now is where it is, we definitely think probably opportunities if successful.

>>DAN OGG: Right…this is great for informing you about aluminum-clad fuel and models that apply to aluminum-clad fuel …as you know DOE inventory is vast and very diverse. So if you start putting these other strange cats and dogs in the DOE standard canister, you know, it may be very useful to have some monitoring of those other fuel types which, for which your models may not apply.

>>JOSH JARRELL: One more thing to add is, the system that Dr. d’Entremont spoke about was specifically for an instrumented lid for DOE standard canister, using it for mini-canister right now. We have been very aware, I mean… the Board was very clear that instrumentation and monitoring as, as a piece of aging management practices in general was important. We definitely take that to heart.

>>DAN OGG: Thank you.

>>JEAN BAHR: Okay, thanks. Paul Turinsky has his hand up.

>>PAUL TURINSKY: Sort of a word of caution,… if this is going to be used for validation of models the fuel that's going in there really needs to be extremely well characterized so that the model not do bounding calculations but can do basically, you know, do realistic calculations to see how close the model is capable of actually predicting what's going on there. So it's not your lid part it's for the people who are going to be putting that fuel in, that they make sure that the fuel is very well characterized so that the inputs to the model can be very representative of what actually is occurring out there.

>>EVANS KITCHER: Agree.

>>JEAN BAHR: Not seeing any other hands up oh Andy Jung has a question. Andy? There you go. Great.

>>ANDY JUNG: Hi, thank you for giving a chance but I have two short questions. One is for the in terms of the materials allowed to use inside canister… I understand that any are carbon-containing material such as any organic material should be prevented based on the US NRC, the standard [inaudible] plan because I have experience for the developing wireless sensor, commercial product at the company before; and the second one is that I'm still wondering to confirm that no, you are expecting for you

based on the experiment so I'm maybe if you can accommodate a sensor for oxygen detection. I think would be very helpful to confirm your suggested mechanisms for the scavenging or the nitric acid gas generation to consume; they all generate oxygen. So, I think the first one is for the design aspect and second one is for the additional sensor if you can accommodate in your system.

>>EVANS KITCHER: Yeah, so the first question I will say that, that is one of our tasks for this upcoming year is to figure out exactly what the requirements are for these systems including what you are bringing up, which are material compatibility issues. I believe that for instrumentation that goes into the 603 facility there is going to be a different set of requirements for what we can do. If we are saying that this system is going to be temporary so we are going to deploy it, monitor it and then at the end we are going to remove it versus what would be going inside a DOE standard canister which you would imagine is for long-term maybe even permanent disposal. So that's, that's number one is that: yes, I completely agree there will be material compatibility issues that we need to deal with. However, the requirements for what we're proposing, which is, you know, instrument this thing, monitor for two years and then take… we have the option of removing the instrumentation versus maybe something that would be deployed in an extended hundred year maybe even a disposal scenario, those requirements would be different. So that's, that's the first thing.

The second thing, with respect to oxygen again this is two different scenarios. For the one that we're looking at we are in a vented system and so there is oxygen, there is nitrogen, there is all the components of air inside the system at 603. However, what my colleagues are looking at are inside the DOE canister which has been back-filled with helium, the disposal scenario in an absence of oxygen scenario. So, in theory, the oxygen sensor could be incorporated into what we're deploying but we're not looking at it since it's out of the scope of what we're looking at. That would probably fit more in what is developed for DOE standard canister.

>>JEAN BAHR: Okay, thank you so I think at this point we need to move on to the public comment period. So, thank you

again Evans for a very interesting talk and I'm going to bring Bret Leslie on, he is there now so, and he is going to read the public comments that we have received.

>>BRET LESLIE: Okay Jean, I'm assuming people can see both me and a slide and so before I get started I am Bret Leslie from the Board staff and before I begin with the public comments I would like to let those who are listening know that the meeting transcript will be available on our website by October 24th, and the transcript will include the following public comments. At the beginning of the meeting Jan Boudart from the Nuclear Energy Information Service NEIS.org had the following comment: she stated “is TD technical development? What is CPP-666?” That was the extent of her comment.

At the beginning of Josh's talk uh, Jan had another comment, again she is from NEIS.org, she stated “given the elephant’s foot is disintegrating, what precautions are being taken with scientific vitrification, how long will the new quote vitrification technology last?" A little while [Barbara Warren], with no affiliation had following comment she stated: “in Josh Jarrell's presentation he mentioned the use of surrogates but it was not clear. When and how surrogates were used for testing instead of the aluminum spent nuclear fuel. Clarification would be helpful.” End of her comment. A little bit later still in Josh's presentation, Donna Gilmore from safety.org had the following comment: “how can you monitor once fuel is in a sealed system whether or not it is vitrified? Other countries use thick wall cask meeting ASME3 codes which requires monitoring and maintenance. Have you evaluated that option? If not, why not? It appears given your limited research budget this could eliminate a lot.” During the break Connie Klein who had no affiliation had following comment. Stated quote “number one, is aluminum-cladding still used and why was it initially used? Does it have to do with enrichment levels of the fuel?” That was it. “Number two, can the drying and storage canisters and even the cast be unloaded if problems develop?” Number three, “Other countries use thick walled bolted lid casts that meet ASME N3 nuclear pressure vessel requirements/certification, specifically designed for spent nuclear DOE and commercial fuel storage and transport. This certification should be recommended.” Number 4, “Exactly what is being done proposed regarding vitrification and she provided two web links. First is https: www.asme.org/certification-accreditation/nuclear-component-certification and the second website was https:sanonofre safety.org/Swiss/, excuse me ... That's the end of her comment. During Greg Horne's presentation again Connie Klein, no affiliation had the following comment. She stated, “where is imported aluminum-clad spent coming from and why is it coming to the US?” Jan and that's the end of her comment. Jan Boudart of NEIS.org had the following comment she stated, “Greg Horne rates of H2 release? Dependent upon inventory? Please go into more explanation of this.” She had another comment shortly after again Jan Boudart in NEIS.org stated “please talk about coupons in this context.”

Moving on during Anna's presentation Jan Boudart in NEIS.org had the following comment “will Paul Turinsky please talk about the equilibrium point is it a radiation equi-point or what kind? And does he mean gamma radiation. This is in relation to Anna's presentation”. During Alex's presentation, Lee Ford from Snake River Alliance had the following comment “Thank you to the Nuclear Waste Technical Review Board for hosting this public meeting and for the opportunity to comment. I need to leave before the public comment time for another meeting, so I hope you can accept my input a little early. It was very interesting to hear efforts regarding research and possible dry-storage spent fuel at Savannah River Site, SRS, thank you for the presentations. From what I heard today it seems that SRS is firmly committed to reprocessing all the aluminum-clad and non aluminum-clad spent fuel at the site in the H Canyon and dump the resultant waste without uranium separation into the SRS high-level waste tanks. I'd like it simply ask that the SRS and EM officials actually genuinely consider dry cask storage of aluminum-clad spent fuel at SRS and heed the Nuclear Waste Technical Review Board comment on the matter. Thank you.”. During Evans presentation Jan Boudart from NEIS.org had the following comment, she stated “For Evans Kitchner, if you put monitors with wireless communication would the radiation inside the canister not interfere with, not only the monitors but also their ability to transmit info to the receiver at the top.” She had one more comment shortly after that again Jan Boudart “Dr. Kitchner talks about 500 somethings per hour as the standard dose. 500 what's per hour.” And Jean I need to check to see if any other comments came in. Indeed, one more came in. And final comment of the meeting is from Jan Boudart, which says “thanks a lot for having this meeting.” Jean that's the end of the public comments that I have.

>>JEAN BAHR: Okay well thank you to all the people who submitted comments. Thanks to all of the presenters, thanks to all of the Board members and staff who put this together and thanks to all the people out in in video land who have been paying attention to this as Bret noted we will be posting the video of this online in a few days and also there will eventually be a transcript of the meeting and thank you all for your participation. Have a good rest of your day.