UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

TRANSCRIPT

WINTER 2016 BOARD MEETING

Wednesday

February 17, 2016

KNOXVILLE MARRIOTT HOTEL
501 E. HILL AVENUE KNOXVILLE, TN 37915
MISSISSIPPI AND KENTUCKY BALLROOMS

NWTRB BOARD MEMBERS

Rodney C. Ewing, Ph.D., Chairman,
Jean M. Bahr, Ph.D.
Steven M. Becker, Ph.D.
Susan L. Brantley, Ph.D.
Allen G. Croff, Nuclear Engineer, M.B.A.
Efi Foufoula-Georgiou, Ph.D.
Gerald S. Frankel, Sc.D.
K. L. Peddicord, Ph.D., P.E.
Paul J. Turinsky, Ph.D.
Mary Lou Zoback,Ph.D.

NWTRB EXECUTIVE STAFF

Nigel Mote, Executive Director
Debra L. Dickson, Director of Administration

NWTRB SENIOR PROFESSIONAL STAFF

Robert E. Einziger
Bret W. Leslie
Daniel G. Ogg
Roberto T. Pabalan
Karyn D. Severson, Director, External Affairs

NWTRB ADMINISTRATION STAFF

Jayson S. Bright, Systems Administrator
Daniel Campbell, Technical Editor
Eva Moore, Travel and Meeting Planner

INDEX

ROD EWING: So good morning and welcome to the Nuclear Waste Technical Review Board's meeting on the "Performance of High Burnup Spent Nuclear Fuel During Storage and Transportation." I'm Rod Ewing, Chairman of the Board, and I'll introduce other board members in a moment. But first, I would like to describe the Nuclear Waste Technical Review Board, say a few words about why we are holding this meeting, and what we plan to accomplish.

As many of you know, the Board is an independent federal agency in the Executive Branch. I want to emphasize that the Board is not a part of the Department of Energy or any other federal agency, such as the Nuclear Regulatory Commission, the EPA, or whomever. The Board was created in the 1987 amendments to the Nuclear Waste Policy Act, with the objective of conducting ongoing evaluations of the scientific and technical validity of DOE activities related to implementing the Nuclear Waste Policy Act. The Board consists of 11 members who are appointed by the President, from a list of nominees submitted by the National Academy of Sciences. We are mandated by statute to report Board findings, conclusions, and recommendations to Congress and the Secretary of Energy.

As you will learn from today's presentations, we'll be discussing in detail the characteristics of high-burnup fuel, and we'll also learn that they're different from the characteristics of lower-burnup fuels. And different in ways that may affect safe storage and transportation, including transportation after extended storage when some degradation of the fuel may have occurred.

Because the Department of Energy will be responsible for transporting spent nuclear fuel, DOE and the National Laboratories are engaged in research and development to obtain the necessary data to meet the -- or satisfy the NRC regulations. The focus on today's meeting will be on learning about this work from the key personnel who are directly involved.

Because of the technical complexity of these issues, it may not be possible for us to address them all today. Consequently, the board may follow this meeting with a technical fact-finding meeting to obtain additional information from the Department of Energy, National Laboratory staff and their contractors on the status of the research and development activities. After completing the meetings and fact-finding sessions, we will report our findings conclusions, and recommendations to the Secretary of Energy and the Congress.

Considerable effort and planning have gone into arranging this meeting. I especially want to thank Gerry Frankel, Board member, who is the Board lead on this meeting, and Bob Einziger, the staff member who is the staff lead for this meeting. I also want to thank the Department of Energy and the scientists and engineers from National Laboratories for their contribution to this meeting, particularly their presentations.

Now let me introduce the Board members. I'd ask them to raise their hands as their names are mentioned. I'll begin with myself. I'm Rod Ewing. I'm Chairman of the Board. All of the Board members serve part time. We have full time real jobs. In my case, I'm a professor in nuclear security at the Center for International Security Cooperation, and a professor in geological sciences in the School of Earth, Energy, and Environmental Sciences at Stanford University.

Dr. Jean Bahr is a professor of hydrology in the Department of Geosciences at the University of Wisconsin, Madison. Dr. Steve Becker is a professor of Community and Environmental Health at the College of Health Sciences at Old Dominion University. Professor Susan Brantley is a distinguished professor of geosciences and Director of the Earth and Environmental Systems Institute at Penn State.

Allen Croff is a nuclear engineer and adjunct professor in the Department of Civil and Environmental Engineering at Vanderbilt. Professor Efi Foufoula-Georgiou is the distinguished McKnight University Professor of Civil Engineering, the Joseph T. and Rose S. Ling Chair in Environmental Engineering, and Director of the National Center for Earth Surface Dynamics at the University of Minnesota. Dr. Gerry Frankel is the DNV chair and professor of Materials Science and Engineering and Director of the Fontana Corrosion Center at Ohio State University.

Dr. Linda Nozick is a professor in the School of Civil and Environmental Engineering at Cornell University, but unfortunately she was unable to join us at this meeting. Professor Lee Peddicord is director of the Nuclear Power Institute and professor of Nuclear Engineering at Texas A&M. And Professor Paul Turinsky, professor of nuclear engineering at North Carolina State University. And Professor Mary Lou Zoback is a consulting professor in the Geophysics Department at Stanford University.

As I always warn at Board meetings, I want to make clear that as the Board members ask questions and make statements, please keep in mind that these are not the -- do not necessarily reflect the official Board position. Our Board positions can be found in our written documents; that is, our letters to Secretary of Energy and Congress, and our reports.

I would also like you to know that the Board -- that there's more information on the Board at the back. There's a one-page handout summarizing the Board's mission. Has a list of the Board members with a few words about backgrounds, and also at the back you'll find the Board's recent reports. You can visit the Board's website, www.nwtrb.gov, and there you'll find all our reports and correspondence, testimony, and meeting materials.

I should mention, related to the website, this meeting is being webcast, so depending where you're sitting, you may or may not find yourself as part of that live webcast. The webcast can also be found on our website. At the moment, that link is under the press listing on the second page. Later today we expect it to be more prominent on the first page of our website. So just bear with us and search for that link. At that link you'll also find uploaded the presentations by speakers, and you can look at those slides as the speaker addresses the audience.

During the meeting there will be two opportunities for the public to make comments, at the end of the morning and at the end of the day. If you would like to make a public comment, please sign up at the table just outside the doors so we'll know who wants to speak. Also, feel free to make written contributions or statements, and those statements will be part of the record. And, in fact, the entire meeting is transcribed, so that becomes part of the record, including the public comments.

When you're speaking or asking questions, please use the microphone so that we'll have a complete record for the transcript. Also, please identify yourself and your affiliation so that will become part of the record. And for speakers who are responding to questions, particularly considering that this is being webcast, please try to summarize the question so that everyone in the audience and those, watching the webcast, are able to hear the question.

Now let me -- those are the logistics. Let me outline, briefly, today's agenda. We'll have welcoming remarks by Johnny Moore, the Oak Ridge DOE site representative; Dr. Alan Icenhour, the Associate Laboratory Director at Oak Ridge National Laboratory; and Ned Larson from DOE Nevada.

After that, we'll begin with a presentation by Brady Hanson from Pacific Northwest National Laboratory. He'll give a description of high-burnup spent nuclear fuel and discuss gaps in knowledge. He'll be followed by Dr. David Tang of the NRC, who will explain the NRC regulations and expectations related to fuel behavior during storage and transportation under both normal and accident conditions.

The remainder of the meeting will focus on four main areas;. The analysis of the effect of hydride reorientation on transportation accidents, including an analysis of presentations made at an ASTM meeting on hydride reorientation that was held last year. ASTM stands for the American Society for Testing and Materials. We'll also get an update on the ductile-to-brittle transition testing at lower temperatures that's being conducted at Argonne National Laboratory.

The second topic will be the effects of vibration fatigue on spent nuclear fuel performance under the conditions of normal transportation, including work in progress to test irradiated fuel rod fatigue, simulated road testing, and shaker table testing. Third broad area is the DOE cask demonstration on the behavior of high-burnup fuel under normal conditions of storage, including discussion of the data expected from monitoring the cask and the initial characterization of the fuel rods.

And finally, we had planned a discussion of how the information from the drying studies being conducted at University of South Carolina, funded by DOE's Integrated Research Program, would support and augment the DOE cask demonstration. We have some last-minute changes there. The speaker may not be able to arrive or maybe will be able to call in. So we'll see how that goes as the day progresses.

So that's a quick summary of the day's agenda. I'd ask you, including myself, to mute your cell phones, and let's begin. And it's my pleasure to turn the podium over to Johnny Moore who will start the meeting. Thank you.

Good morning and welcome. Thank you, Rod. I appreciate the opportunity to be here to welcome the Board. I understand you were able to have a tour yesterday for those that were able to make it. I know many have had difficulties in getting here, so even that in itself is an endurance test. But what you do in your review and input to the department is very important, so we certainly think some of the technical questions and technical progress that gets discussed today, we do appreciate your input and feedback. So on behalf of the Department of Energy here in this East Tennessee -- and I'm at the Oak Ridge National Laboratory site office -- we would like, again, to welcome you. Thank you.

This is Alan Icenhour.

Good morning. As Dr. Ewing said, I'm Alan Icenhour. I'm the Associate Laboratory Director for Nuclear Science and Engineering at Oak Ridge National Laboratory. Let me add my welcome to Knoxville, for those of you who made it, and we hope that we'll have a few other people join us as they work through their various travel troubles. But we are very pleased to have the Board here for this very important meeting. And I just wanted to spend a few minutes and tell you a little bit about our work at Oak Ridge National Laboratory.

At the laboratory we conduct research and development programs for a range of nuclear science and technology, areas from fission to fusion, isotopes, nuclear security, as well as modeling and simulation. And in fission, I'll come back to that more in just a moment. In the area of fusion we focus on technology development and helping to understand and control the fusion plasma, and also in understanding the science of the plasma/material interface, and we support the U.S. contributions to the Heater Project.

Our efforts in isotopes, of course, go back to our very heritage, and we perform a lot of work with both radioactive and stable isotopes, applications that advance the science, such as the discovery of new elements like element 117; space exploration, we're working on the efforts to re-establish plutonium 238 production; for industry, Californium, 252; security applications, as well as medicine where we produce isotopes that are used for cancer therapy.

We're also engaged in nuclear security, where we perform research, development, and deployment of technologies that ensure the peaceful use of nuclear energy and nuclear materials. And this involves things such as radiation detector materials and systems, new security technologies, application of safeguard regimes with our international partners, and nuclear forensics, which brings to bear our facilities and analytical capabilities, as well as our extensive knowledge of the nuclear fuel cycle. And finally, our R&D efforts include modeling and simulation, which crosscuts many of these areas. A very visible effort that we have in this area, of course, is CASL, the Consortium for Advanced Simulation of Light Water Reactors.

So all of these R&D efforts are enabled by our nuclear infrastructure, which allow us to safely handle and study nuclear materials. Coming back to the area of fission research and development, our efforts span the entire nuclear fuel cycle, from enrichment to reactors, both current and advanced and the backend of the fuel cycle. Examples of our research include extending the life of the current fleet, providing the technical basis for license extension, a significant amount of work on advanced reactors, performing research and development related to molten salt technology, instrumentation and controls, the development of new structural materials, accident-tolerant fuels, and, importantly, helping to develop the licensing framework for advanced reactors.

So, finally, and very importantly, we have efforts related to spent nuclear fuel. We're engaged in DOE programs related to this issue, and you will hear about much of that today. And we perform research and development that provides the technical basis for the safe management of spent nuclear fuel. So today you'll hear about many of these types of activities in which both Oak Ridge National Laboratory and our partner laboratories are involved. These efforts are very important for dealing with spent nuclear fuel and thereby ensuring a sound future for nuclear energy deployment. I hope that you find these presentations and discussions to be very informative and helpful, and we look forward to interacting with you today. Thank you very much.

Now, next, Ned Larson from DOE.

Away we go. Can you hear me fine? It sounds like it.

Yeah.

First let me begin by giving regards from Andy Griffith who couldn't be here today. He takes this meeting very seriously. It's very important to him, but he had another project that was having significant issues that he had to take care of, so he asked to excuse himself and asked me if I would present for him.

What we'd like to do is begin with Andy and the Fuel Cycle Technology -- Office of Fuel Cycle Technology is responsible for looking at the spent nuclear fuel and the fuel in total, all the way from the beginning to the end, to the disposal in the repository. In order to do this, we believe that we needed a systems engineering approach because sometimes we have competing requirements, competing demands in the different phases of the fuel moving back and forth, and so we step back to look at it and try and define all of the system requirements, from the functions and requirements activities.

When we look at it, there's a lot of options that we can go through, for instance, today we go through the once-through technology. That is what we're doing today pretty much. We run it through, and then once it comes back out of the reactor we dispose of it. But there are other options also that we're looking at.

We're looking at are there things that we can do? Can we repossess of it? Not also reprocess, but can we reuse some of it or change pieces of the cycle to help reduce some of the other things as we go towards the end. As we do this, there's a lot of options in between, all the way down to, sure we could reprocess everything, of course, but there's some real problems with that also.

And so as we look through it and go through it, there's a lot of options that we have to take care of and to address as we look at the alternatives. And, again, the engineering approach is one of the most important things that we are considering. In the used fuel disposition campaign, which is where I work, there's some things that -- the ones that

I'm going to talk about today are the storage and transportation activities.

In the storage and activities the things we asking our cask systems to do, and our fuel systems to do, is, one, can we store them for a whole long longer than we originally planned? After we store them, can we retrieve them and deal with them and handle with them? Do they still handle okay? And after we do store them, can we transport them again, and can we transport and can they even be stored again? In order to do this, there's a lot of data and information that we still need and we still need to gather. And so those are the issues that we're struggling with.

One of the things that we asked -- that I was asked to talk about is how are we dealing with research and the considering uncertainty in the path forward in the repository. We still believe that the Blue Ribbon Commission recommendations are very good recommendations. The Department fully accepts those recommendations. Some of the recommendations in the Blue Ribbon Commission required changes of legislation. We recognize that and so did they in their report, in their final report. But they also said there are a number of things that we could do that did not require legislation.

And the things that we believe that we can move forward on to reduce uncertainty in the system are in the planning phase, and we have the Nuclear Fuel Storage and Transportation Planning Project. And then we have the Used Fuel Disposition Campaign, which deals with the research on the storage and transportation, as well as final disposal.

If we do our research correctly in this area, it will give us more options. It will give us better knowledge and understanding. If we can get our research down, we believe that it will give us more options in the future, and so right now we are still working within the bounds of what legally we can do and what we can't do, but at the same time, we are focused on the things that we can do. And we believe that by doing research correctly, by doing research up front, we will be okay. We will have options and defined options that we are not aware of today. And so you're going to hear about some of the research that is taking place.

Basically what it comes down to in our research for the three things that I talked about previously, we have to understand the strength of our materials, the cladding, the structural integrity of the canister, the welded cans, as well as the structural integrity of our casks. What is the strength that they have, especially the cladding? When we take it out of the reactor, it's high-burnup, we store it for long periods of time, how does it behave? What is the strength that we expect to anticipate coming out of that? And then the other is, what is the strength? What are the stresses that we will impose upon these materials once they come out? When we handle them, when we ship them, when we store them, when we do those things, what are the implied stresses that we're going to apply? And of course if the strength of the materials always exceeds the induced stresses from our handle handling activities, then we're okay.

And so you're going to hear presentations today. Some will be talking about material strength, some will be talking about the stress profiles that will be applied to the cap, to the materials as we go down the road. And so those are the two areas, as long as our strength of our materials exceeds that that we will induce from our activities.

When the project was created in 2010 we knew that research was going to be important to us and how we dealt with research and how we applied it. But we knew that we needed to prioritize. And because there's so many things that we need to take care of, so many things that we need to answer, but we need to do it in a priority basis, because we don't have unlimited resource, we don't have unlimited funds. And so in 2010 we started. In 2012 we published our first priority. I'm not going to wade through these. Brady Hanson is going to talk more about the specifics.

But in 2012 we published our first set of priorities, the things that we thought that we needed to be focusing on. In 2014 we updated it. These things don't change quickly. They don't change fast because it takes research to let us start checking things off, moving things in, moving things up in priority. It takes time. And so 2014 is our last priority. We readjusted it based on the data and information that we had coming in. We'll probably adjust it again in another year or so to make sure that we know and understand where we are. But we do have our priorities set. We have shared these with others. These are not -- these are pretty similar priorities to some of the other things. The Board did their report on the priorities, they're not far distant. And so we do set our priorities in order to do what we needed to do.

One of the things that is important to us is the High-Burnup Demonstration Project. This is a joint project with the Department of Energy and the Electric Power Research Institute, I guess EPRI. I'm so used to saying EPRI that I forget what it actually stands for. EPRI and the department had co-sponsored this project, and the issue is we need data and information. There's a lot of theories. Will the cask -- we're asking our cask to store thing much longer than what we had originally decided.

From a theoretical position we believe they'll be fine. We believe that they will store and handle these materials for long periods of time. When we do the numerical modeling and we do the other analyses, we feel pretty good about it, but we don't have data. And so we need data, and that's one of the things that we're going for, so on this project we are going to store and load a cask using typical processes and techniques that are currently used in industry. We will load them in the cask. It's a metal cask, fully metal cask. It's not the thin-walled canisters. We'll load it here so we can pull the lid off. If we have to, we can go in and measure. We can do things on the inside.

The things that we want to do is measure, of course, temperature. We hope to sample our gases inside to make sure we know and understand how it's behaving and how it's performing. There's a number of things we look at, active sampling, using sensors, things of that nature, and so we're mostly using -- we were not able to find sensors that we could put inside the cask at present. And so we will be sampling the gas as we go down the road. We work this with the shared our plan with the Nuclear Regulatory Commission, we received their comments. In fact, Dr. Einziger was one of them when he was with the NRC, worked with us on making sure that we addressed the sampling of gases, because we believed it would be giving us a lot of very good information.

In order to establish the baseline of what the rods would experience, we pulled 25 rods from the assemblies that we will be loading inside the cask, or very similar to those that we will be loading inside the cask. We will be testing those. Those were shipped. We've published our supplement analysis showing that we could ship those here to Oak Ridge, we could handle them here at Oak Ridge. Oak Ridge had all the facilities. Oak Ridge had everything that we needed to receive the cask and to start doing some of the testing here at the lab.

In January, the rods were shipped from the North Anna facility, where we pulled them, and were received here at Oak Ridge. I was not here for the tour yesterday, but I hope you saw the box where they were stored. I assume they've stopped dancing in the halls, because it was a big deal when they got here. I'll just say it was a big deal. It was good.

On the sister rods, there's 25 rods that we pulled. We've sampled -- one of the good things about the North Anna facility is they had a number of different types of cladding that was important to us, a number of different types of burnup. North Anna has been incredibly cooperative with us. We pulled the AREVA rods, several AREVA rods, Westinghouse rods. We've got four different types of cladding that we will be testing and measuring.

In the supplement analysis, again, just the overview, we will be doing nondestructive testing, as well as destructive testing for our rods. Most of the non-destructive testing will be done here at Oak Ridge. We will be cutting some of the rods up into what we call "rodlets," and we'll be shipping those to PNNL, as well as Argonne will be doing some work on those also. And Oak Ridge will be doing some destructive testing here locally.

As far as our activities with NEUP and the universities, we continue to believe that the universities are important partners on the project. Since 2011, you can see the amount of money that we have spent and allocated to the NEUP program. You can see the different research projects and storage and transportation on this list. We spent more than $27 million, is going to universities in order for us to complete some of the research.

How we do our research is we work with the national labs, typically that are engineer-oriented in nature, solving engineering problems so we could do certain things. We use the universities for more blue-sky type activities, what are some of the thing that are not well defined, what are some of the options out there? And the universities are very good at that. They're very good at being able to go out and brainstorm things and brainstorm solutions and come up with possible alternatives, ideas at the very forefront, the very cutting edge of technology is what we have used them for at this point.

We continue to use our universities well. Like I say, we've got some very good reports, and I'll just say we got some reports where they hope to put some things out. And they haven't worked out like we had hoped, but that's okay, that's the nature of research and development. It is tough to define at times. Let me just go back and explain.

As you see the number on the side, there are some numbers that are quite large, in the four million, three million range. These are what we call our "integrated research projects." This is when we define a fairly large scope of work and the universities then go out and form teams with multiple universities to solve them, picking up specials at different universities to come together as a team. My personal preference is for those, because they're very integrated in nature. They solve problems.

The universities work together, is my experience, and so those have worked well. The other ones are at about the $800,000 range, or less, 600 to 800K range. These are ones where we have a specific problem or a specific issue that we're looking to be solved, and we will award those and see at the university, see how far they can take that issue towards resolution. And so those are the two types of research that we applied, types of grants we give to the universities.

For the IRP, some of the two recent ones that we have done, recently we have awarding one, with South Carolina being the lead university. University of Florida also supporting them. This is the in the drying. When we dry casks we believe that we can dry them. Theoretically we believe that the water is coming out of it. We see no reason why it shouldn't, but we've never -- we haven't opened one up to make sure that the water's coming out. We just don't know. And so they are looking at some ways to verify that the water is actually out. Verifying that some of our theories are still correct and doing some of those things. They have some very innovative techniques that they're going to use to try and deal with that one.

Another one is sensors. We want to take more advantage of sensors and remote sensing in some of our casks. Penn State was awarded this IRP recently, teamed with Illinois and South Carolina. This is involving both the development of sensors, but also the placement of sensors, because some of the sensors that we need are in very limited play, they're very limited space, and we have a hard time placing the sensors in areas where the casks have already been loaded and they're sitting on the pad. So Penn State is leading this one up. We're very encouraged by some of the preliminary ideas that they have. We thought that they had a very strong proposal and so we're looking forward to seeing the results of that. And so these are some of the ways that we're using our universities to team with us and to work with us to solve some of the challenges that we have.

In summary, the technical direction, we're using experiments there, theory and modeling, to come down to do a demonstration, the things that we would like to do in the future are possible and doable. We believe we have the right partnerships established. We have the industry. We're dealing with the utilities, the cask manufacturers, the fuel supplier, even the rail and trucking companies, those who would actually be transporting our casks, we're dealing with industry in those areas. We have 11 national labs working with us right now, principal investigators have been identified in 11 labs that have specific expertise and knowledge and understanding of the problems that we are solving in our campaign. And then universities continue to be important to us. We have 18 universities working along with the use field disposition campaign.

I started to try and count the number of students and teachers and professors that we had, and I gave up, I'll just say. So you can do the math if you want, how much a grad student would, how much principal investigators and stuff. But it's a lot. I'll just have to put it that way. It would have to be a lot at $27 million. And like I say, we've had some good results come through so far. And so these are our priorities right now. Like I said, I you'll hear more specifics from the principal investigators who will be speaking to you a little bit later. But those are our priorities right now.

I guess you want me to take questions. We have a little bit of time.

Yes, please.

Any questions?

Ned, first, thank you for stepping in and doing such a nice job with the overview. We're very appreciative.

My pleasure.

So first, questions from the Board? I have one -- I mean, several. On the IRPs, how long do those last?

They typically last three to four years. We start at three years, generally speaking, and then they come back, and universities usually come back and ask if they can have another year, no cost extension, which is reasonable.

Right. Sure.

We're okay with that, so we grant them.

And you mentioned that you have quite a number of students involved in the program. Has this been going long enough to have some sense of how many of these students go into the

National Laboratories or to other universities and continue in this line of work, in research?

We started the real awards in '11.

Okay.

And we have seen several grad students already in some of our national labs, where they've come in as interns. I know that we've had interns in there. I believe we have hired a few, not many, because it just hasn't exploded like we had hoped it would, I'll just say. But we have had some that we've picked up that I'm aware of.

So this really -- an important aspect of this is developing the workforce for the future, and so I commend you on that.

We're hoping that is the case. I mean that's one of the advantages, in addition to getting our problem solved we're getting some very good experience at our university level already when they come out. We're getting great experience.

Thank you. Efi Foufoula, Board. So from the experiments, theory, and modeling, if you were to pick one of the three as the weakest link towards demonstration, which one would that be? And in terms of the money, the $27 million dollars, go, is there one that occupies more of that money space than the others?

Sure. I believe --

Here we go. I believe that the one that is our biggest challenge is experiments. Theory, we've got the theory. I mean the theory from engineering perspective and Earth sciences perspective, we've got great theories and knowledge and understanding. Modeling and simulation, we've got great computers and codes now. I mean we've got great codes. But the data to show that it's doing what we think it's doing and to verify the theory and the code is what's tough for us at present. And any time you add the word "nuclear" in front of everything it gets very expensive. And so as we go through, we try to do, like the universities, we typically have them do simulated materials. For them to deal with nuclear materials is very tough. Even for us to deal with nuclear materials in the laboratories is very hard. And so I believe it's the experimental. We need the data. We've got to have the data to verity the theory and the verify our numerical models. And so I believe that's the one where we are putting most of our money right now, actually, is in the experiments.

Ewing, Board, is as a follow up, could you give us some sense of our experimental capabilities, particularly in terms of characterizing a few as you do these demonstration experiments. Would you say that's in good shape, or is that another area that requires investment and attention?

Right now, like I say, the national labs are the one that is really have the capabilities for nuclear material experiments and capabilities. When we were looking at testing the 25 rods, both Idaho and Oak Ridge have outstanding capabilities to do all the stuff that we want to do to the 25 rods that we have pulled, both laboratories have all the capabilities that we need. They have the equipment. And like I said, and PNNL has the equipment. So when we go into the actual destructive testing of the nuclear materials, we are in pretty good shape there. We're in pretty good shape.

Thank you. Jean.

Jean Bahr, Board. You mentioned that for the high-burnup fuel test you had looked at possible internal monitoring and found that that wasn't feasible. Maybe you could elaborate on that a little bit.

Sure. One of the big things that we're worried about, that we're concerned about, is the temperature inside the cask, what will it be. And the reason -- and Dr. Hanson and Dr. Billone will be talking about why that is important to us. But under certain temperature conditions, pressure conditions, we have hydride reorientation, and the question is, will we have it? And it can reorient it in a very detrimental way. And they'll talk specifically about how that will affect our material strength if you're with me. But we will be looking at that to make sure that we understand what's happening inside that cask so that we can verify our theories and make sure that we know what's going on.

Jean Bahr, Board, again. You had mentioned that you were going to primarily rely on gas sampling because other kinds of internal monitoring hadn't panned out. And I know this was something that the Board had felt was important when we first heard about the high-burnup fuel test, that there be some attempt to put sensors inside the cask.

I actually probably misspoke a little bit. I love the sampling of the gases internally, because that's a lot of fun. I mean, pulling them. But the biggest issue for us is the temperature, actually. And we have seven lances with multiple probes going inside to know and understand what the temperature is; because the temperature and what happens to the hydride read, a big part of it is temperature related.

So you will have internal monitoring?

We will have wonderful -- we'll have more temperature data than you can imagine. What our agreement is is that when we start gathering that temperature data, we will put that on the Web. We will let anybody analyze it that wants to analyze it, NRC included. The Board, if you guys are suffering from insomnia, we'll share it with you guys too. And that's what we'll be doing is to make sure. But the temperature is the biggest thing inside the cask, you're right. But the gas is a lot of fun. I love the gas sampling.

Okay. Mary Lou.

Mary Lou Zoback, Board. Thank you. I wanted to follow up on the sensor thing. So the temperature data is going to be transmitted in real time?

It will go through a little bit of a QA process, but it will be pretty close to a real-time process, you're right.

Okay. Good. How is the gas sampling accomplished without opening the canister?

Well when we load the cask the reason we picked the metal cask is there's the drain port, where we apply the vacuum, the vent port, as we call it. We will be putting it under vacuum to suck -- to pull the water, the moisture out of it. But we are working right now with the Nuclear Regulatory Commission. We've submitted our license application. We want to go back to that same vent port and pull about two-liter sample that we can run gas samples on and test on. And so we can seal it back up again, bolt it shut again and let it sit longer. So it's a serious thing, don't get me wrong. It's not trivial. It sounds trivial just filling a two-liter bottle and stuff like this.

But, again, any time you deal with nuclear materials out on the -- right now we're looking to see if we can do out on the pad, the concrete pad that it will be stored at. We believe we'll be able to sample them there, but it's not a trivial process. Dominion is -- they've come up with some ideas, some processes and procedures to do that now.

As I understand it's a ten-year experiment; is that correct? So will you be sampling every three months, the gas?

No. Right now we're probably only going to sample about three times, probably in the beginning. Probably two or three years out, and then probably at the end of the ten-year period we hope to have this cask sit loaded on the pad for about ten years. After the end of ten years, we hope to open the lid up again, pull out 25 more rods, maybe more, 50 maybe, and then send them into the same laboratories to be tested again, and then compare how they were, how the ones that we pulled recently versus the ones who sat in the cask for ten years, how are they behaving? Do we see a degrading of the material strength or are they basically the same, untouched.

And then just to close the loop, I remember when this was first presented to us, I think maybe a year-and-a-half ago. They said there currently was no facility to actually open it up and remove the rods later. Is that still the case?

Right now we're still doing work on that. We have a couple of options that we're still doing analyses for. We've got a little bit of time to deal with that, but you're right, because of the length of time it takes to deal with these things, we need to be closing on it reasonably soon. But we're still doing studies on that phase of the work.

Okay. Thank you.

Gerry.

Frankel, Board, right here.

Oh, here.

Thank you for the overview. I wonder if we could bring up the slide with the prioritizations, the list of prioritizations. Yeah, so you didn't spend much time on it, and there are issues that you've downgraded because of various assumptions it seems. So initial license period, so, you know, aqueous corrosion of welding canister, there's not enough aqueous conditions. So what initial license -- are you talking about a repository for this, or what's the condition? What license period are you discussing?

This, we're talking about -- these priorities here are priorities based on storage and transportation. There's the repository. When I say we're doing research, there's two phases of the research. One is storage and transportation, the other is repository design and such. This is only related to storage and transportation. The repository people are --

So what do you mean, then, by the initial license period? For instance, you say you're talking about conditions during the initial license period. So how are you defining that?

I'm not real sure of your question.

So I'm looking here third from the bottom line, you say it's not a need because the aqueous conditions are not sufficient, not sufficient time for a breach during the initial license period. So you're assuming some period.

The initial licensing period, as we would see it, is just through the NRC getting the license to store it on the pads. Then as we go down the road they're going to have to renew their license if we don't -- they've only got a 20-year period, then if they don't dispose of it then, then they've got to renew it to extend it. Then they've got to extend it again, depending on the data that we come up with.

So you're only considering the time for the application of the license?

Here's what I would like to do, is Dr. Hanson will be talking about this in-depth.

Okay.

Down the road.

That's fine.

I'd like to defer to him. All I wanted to do is show that we have our priorities set, but I'll let him talk about the different priorities themselves.

That's fine.

The specifics, if that's okay.

Yeah, that's fine. Other questions?

Jean Bahr, Board. Again, on this table, I guess what prompted part of my question is there's an item monitoring dash internal and then there's a comment that says, "No longer a prerequisite for the HBU confirmatory demo." So I guess I'm still trying to understand what you mean by internal monitor and whether or not that's going to be done beyond -- does that refer to something beyond temperature monitoring, or has that been taken off the -- other monitoring taken off the table as something that's useful or necessary?

What I'd like to do is we're talking specifics of the table itself. So what I'd like to do that is defer to Brady, if that's okay, because he will get into that in detail. I just wanted to show that we had our priorities. But he will talk about the exact specific priorities that you're addressing.

All right. So we'll hold those questions for Brady. Other questions? Paul?

Tarinsky, Board. I hope this one wasn't asked while I was out. Beyond the integrated research program at Penn State to develop in situ instrumentation, what other work is going on at DOE?

Say that again.

There is a program that Penn State is heading up --

Yes.

-- to develop I'll call it "in situ insight --

To monitor the sensors, monitoring and sensors.

Yeah. Besides that research program, what other work in that area is going on?

Right now we're funding Summit National Labs. We're funding a little bit at some with Argonne, some with Idaho National Lab for specific purposes if you're with me.

Can you tell me what those specific purposes are?

Well the big issue that we're struggling with is knowing what's going on. What we would love to have happen, if all goes well. There's a whole lot of casks that have been loaded, to date. We would love to try and develop sensors and sensor technologies to see and know and understand how those are behaving internally without having, because they're welded canisters right now, and we're not going to be opening the wells. We just won't be doing that.

We would love to have technology so we could peek inside those casks and see how they're performing and see how they're behaving. Specifically, temperature-wise, we want to make sure that we know and understand how the thin-shelled canister itself is holding up, the stress corrosion cracking type stuff, and we'll talk about that a little bit later. To know and understand are we seeing pitting corrosion, even though we're using stainless steel, we still know, uncertain conditions, that can be problematic. And so we're trying to develop the sensors, remote sensors, technologies, delivery of the sensors to try and peek in to see what the others are doing. I mean, ideally we'd love to have that. But that's not going to be soon. I'll just say it is -- we're not -- that's a tough issue to solve. It's a hostile, hostile environment that we're putting our sensors in.

Other questions? Right.

Ned, thank you very much.

Thank you.

So, Brady. Okay. You already have questions waiting for you.

I'm not sure what that means, but, okay, so I'll do my best. So Dr. Einziger asked me to talk about what are some of the key differences between low- and high-burnup fuel? Why are we making such a big deal about high-burnup fuel? I there go into, as Ned said, what are some of these gaps that we as the use field disposition campaign have identified? I'll also go into the integrated approach that we're using, specifically for closing these gaps on high-burnup fuel, and I hope to provide some perspective about, well how much high-burnup fuel is out there, how high are we talking about, and hopefully show how the other talks following me fit into this integrated closure.

So the first thing is what is high-burnup fuel? By definition, the NRC has said it is anything with a burnup greater than or equal to 45 gigawatt days per metric ton. So what that means is it's either been in your core longer than low-burnup fuel, or for the time that it's been in there it's been subjected to higher power. So, as an example, if the assembly spends more time in the center portion of the core, as opposed to the outer regions, it can experience higher power. That means it has more fissions, it has more radionuclides, higher decay heat. All that falls out.

But it's important to remember that this definition that NRC came up with is based largely on changes to the cladding, a little bit for the fuel that happened. But I want to point out that none of these changes are dramatic step functions. It's a continuum, although I do want to stress that many mechanisms do accelerate the higher you go in burnup.

So what are some of the things that happen? One thing, you know, I know Rod is very familiar with from work that he did on fuel disillusion and changes is this high-burnup structure. So what happens to the fuel itself once you're -- your pellet average burnup is somewhere in the 35 to 40 gigawatt days to metric ton range, you begin to have the grains of the fuel subdivide so that it goes from the original approximately ten microns and forms these sub-micron sub-grains. It also is a very highly porous structure, see if the pointer works. I'm not seeing it. But if you look at the upper right picture in the middle you see in a MOX fuel, where you have substantially higher burnup in the PUO2 grains, how you have this highly porous structure and this smaller grain size surrounded by the regular grains of the UO2. And that's very similar to what happens in UO2-based fuels.

I do want to emphasize that that porosity is closed, so originally we thought that when you went through this alteration of grain sizes that you would release a lot of fission gas. That turns out to not be true. The fission gas is actually trapped in these closed pores. But because of changes that occur in the fuel, things such as changing the thermal conductivity as you go to higher burnup, you can effect and get higher temperatures in the center of the fuel, which actually does influence release of fission gas. So just one example showing in the bottom right there how fission gas does increase with burnup. But, again, it's not a big step-function change just because you magically get to 45. So I want to emphasize this continuum and not gross changes just because we have this, by definition, reaching high-burnup fuel.

One of the biggest degradation mechanisms that is of concern is oxidation of the cladding. So all zircaloy-based alloys, meaning the zirconium, reacts with the high-temperature water or steam in a reactor to form a zircaloy oxide on the outer surface of the cladding. It is very dependent on the type of alloy that you have. But it's important to stress that the NRC has a de facto limit of about a hundred microns oxide layer thickness, and this is based on in-core behavior. If you have too high of an oxide layer thickness you're thinning you cladding and, therefore, subjecting it to potential failure during accident scenarios such as a loss of coolant accident.

So this figure on the left is one that's been widely used. In fact, it comes from an EPRI document, but it was quoted and used in the Board's own 2010 evaluation report on high-burnup fuel. And a lot of people look at that and say, "Oh, we have a real problem. Look at how high you get oxide layer thickness as you increase burnup." But I want to point out that this only applies to low-tin zircaloy-4, and, actually, because of this issue of high enough oxidation, that it leads to potential concerns, the industry a number years ago, over a decade ago spent a lot of time, money, and effort in developing new alloys.

So Westinghouse worked on what they call ZIRLO or optimized ZIRLO, shown in the right figure, AREVA worked on what they call their M5 cladding. Both of those newer claddings contain approximately 1% niobium, so it really changes the corrosion properties and reduces the oxidation kinetics. So it's important to remember that, you know, not all alloys are the same, and we'll see that as Mike and others give their talks later on today.

I do want to point out, last week I asked a colleague in EPRI, and he checked what they call their fuel reliability database and confirmed to me that in 2014 less than 4% of all cladding currently being radiated in PWRs is zircaloy-4, either regular zircaloy-4 or low tin. So the industry as a whole has made this gross change to using newer alloys, and I want to point out that, you know, companies are still in the process of making newer alloys that reduce this potential for oxidation even further.

One of the reasons we care about oxidation is during this oxidation process, hydrogen is released. The cladding takes up a certain fraction of that hydrogen, something that's called the "hydrogen pickup factor." So when you combine the fact that different alloys have different pickup factors, different alloys oxidize at different rates, you have very dependent, alloy dependent, total hydrogen contents as a function of burnup. So very similar to the previous graph on the left for low-tin zircaloy-4, you can see the yellow data points, so that's actual measurements, whereas the blue are Monte Carlo model simulations. But you can see how, as a function of burnup, I do indeed increase the amount of hydrogen in my cladding.

However, on the right, you can see the M5 cladding, so the red line represents the zircaloy-4. The blue dots represent measured data on M5, and you can see that this AREVA cladding is very good at minimizing the amount of hydrogen taken up. I do not have a figure for ZIRLO, but it lies basically in between these two, and, again, Mike will go into detail as to why this is important and why it's important to test each of the different alloys as we go forward.

So, this is something that I felt, you know, very important to share with the board, with the public in general, because I have to admit, I was very surprised by this. For a number of years, industry documents, documents that I've written myself, talk about how we're on this move to higher burnup, and we're getting higher and higher all the time. So, last December, the US Energy Information Administration published, and it's available on the web, I do want to stress all of the figures that are taken from references, the references are shown as superscripts in the parenthesis, and those are provided at the end of the presentation. The EIA, Energy Information Administration, took the data from, what we call, the GC859 database, so under the general contract they sent out questionnaires to all of the utilities that had to answer all the characteristics, where's the fuel that you have ever had in your reactor and discharged, where is it, is it in dry storage in the pool? And, what we see in this table, is over the last decade, whereas, you know, we talk about really high-burnup, what we see is the averages are right around what this, by definition, cut-off for high-burnup is. So, yes we're making high-burnup fuel, but we're still making an awful lot of low-burnup fuel. We'll also see that over the decade, numbers that fluctuated a little, but they're not growing dramatically. The biggest, down in the lower right, we have an example from an analysis that folks at Oak Ridge National Lab did, looking at the Watts part Unit 1 Reactor, looking at all fuel discharged in the first ten cycles, number of assemblies versus burnup, and again, you can see, yes we have fuel that's above the 45 gigawatt day per metric ton definition for high-burnup, but it's not a lot higher, most of it is in the 45 to 50 range, and I'll say that over the years, most scientists have agreed that, you know, only being a little above does not change things dramatically.

The lower right we have an example of what Calvert Cliffs has actually loaded into their, what's called their 24P, meaning it holds 24 PWR assemblies. In their new homes, these are the horizontal storage modules, and again yes you can see there's fuel at 45 gigawatt day and higher, but not a whole lot that's much higher. And, it's important to realize the reason behind this, so NRC does have a limit, current limit of 62 gigawatt days per metric ton for the peak rod average burnup. That is, again, to limit changes that occur in the cladding, and are focused on what happens in core, specifically for potential accident scenarios like loss of coolant. But, the other things that limit it, is again, our systems in the US, the fuel manufacturing plants, transportation, et cetera, are currently licensed for a maximum five weight percent U-235 enrichment. That can change, but would require a fair amount of work. But, under that limit, and under the cycle blanks that the US reactors operate under, which most BWRs in the US are at 24 month cycles, the PWRs are split between 18 month and 24 month. Although I do want to point out, you know, we have the tendency to want to compare against, well you know, international data. An awful lot of the European countries still operate on a 12 month cycle. They don't have, you know, grossly different climates, like we do. They need their power during the winter time, they don't have air conditioning, they can shut down once a year. By doing so, you can actually push to higher burnup, so I want to quote a report that was done by EPRI. They said, with this 62 gigawatt day limit, with the five weight percent enrichment limit, it turns out that in a 24-month cycle, your batch average discharge can only be 46 gigawatt days per metric ton. On an 18-month cycle you can be up to 54 gigawatt days per metric ton for a batch cycle, where a batch is defined as all fuel that goes into my core at the same time, doesn't matter if some's in for one cycle, two, three, four, that batch is everything that goes in at once. It's actually why you'll see, you know, in Asia and Europe, why they get to higher burnups than we do, because at 12 cycles, because I'm replacing fuel more often, I can actually push the fuel that I don't replace harder, because I'm replacing the reactivity, if you will. So, again that's very important, and if we work backwards, and say I have this 62 peak rod average limit, what that says is, that assembly will have on average between 56 and 59 gigawatt day burnup, and that then says my batch is going to be on the order of 52. The point being, industry, you know, the nuclear industry is typically very conservative. They don't like pushing the limits too far. They don't want to have to justify to NRC why I went above a certain amount, and so they have a fair amount of margin.

All of that boils down to say why these numbers have not been increasing dramatically. We do expect them to go up gradually over the future, but you know again, it's not something huge. So, now hopefully I'll remember to address the questions that were asked. As Ned said, back in 2010 we started our gap analysis, a very detailed literature review looking at all the potential degradation mechanisms. We decided, based on how much data was out there, was there need for additional data to support either licensing, licensing renewals, or what I would phrase the DOE long term needs, because we're interested in eventual disposal as well. So, all of the gaps that we identified were then ranked from low to very high, based on this need for additional data. As Ned mentioned, we then prioritized those, first in 2012, based on, and priority is almost exclusively, not completely, but very heavily influenced by how soon do I need that data. Again, either to help me with a license renewal, or does it feed other, other gaps. For example, the first two gaps, thermal profiles, stress profiles, those are what we call cross-cutting, that information feeds a number of other gaps, so we said we need that data right away to help us understand and plan our experimental programs and our modeling programs. So, just real briefly, you know, thermal profiles are not too hard to figure out. Industry very often, and typically employs very conservative models in calculating temperatures. When the NRC says we don't want you to go above 400 degrees CP clad temperature, you're allowed to, but if you do, you have to produce data, you have to give us the justification. Industry says, well let's not do that, let's stay below that, so they employ conservative models to stay below it. We wanted to look and say, well what are our actual temperatures, removing all these conservatisms, and I'll go into that in more detail in a couple slides. Next one is the stress profile. By that we specifically mean external stresses, so as Ned showed on one of his slides, during extended storage we're doing modeling of what kind of stresses would be imposed to the cladding in a cast tip over, in a cast drop, or in a seismic event? As Paul McConnell will discuss this afternoon, he'll go over what are the external stresses we have during transportation, normal conditions and transportation. You know, you're absolutely right that some of the canister issues were deemed more important. We agree with the NRC that corrosion of canisters is very key, we need to be looking at that. So, just to answer a couple of questions, the monitoring external, what we meant by that was developing instrumentation or systems that are 100 percent outside of the cask, can I send the signal through, of any sort to determine temperature? Can I look at potential for fission gas release, because it might change the speed at which sound travels through the cask? We have a couple of labs looking at. Can I detect water inside a cask, purely by having something external? The monitoring internal, what we were looking at, as Ned says, is can we measure the temperatures? We were very interested in can we do real-time, very frequent gas measurements to look for any changes. The reason that I'll show in the next slide, and what Ned had as to why that changed, in 2013 we awarded the contracts to EPRI to do this high-burnup demonstration. -here's a lot of reasons as to why it had to be done then, why we're on this ten year program, because a lot of the questions that NRC has to the utilities, need to be answered before their license renewals are due, remembering that initial licenses were for 20 years, now it can be up to 40 years, if you can prove to NRC that things are good. So, in a nutshell, I think to answer your question, one of the reasons why internal monitoring dropped down later, is anything that goes inside that cask has to be part of the NRC license, and because we had to get our license application into the NRC in order to start this demo on time, you know, we said there's just too much R&D that still needs to be done to feed this demo. That doesn't mean we've given up, there's still, you know, look at can we do other things? But, it will be in subsequent tests, not in this initial demonstration.

Since we're here talking about cladding, see that, you know, hydride orientation was the top cladding one, all the way down at number seven we have other things, such as delayed hydride cracking, creep, annealing or radiation damage, oxidation that while important just not that important, because the consequences aren't as much, the need for the data isn't as much. So, again to what Ned showed, you revisited in 2014 based on what work’s been done by the program internationally, we looked at reports that NRC had put out, international colleagues, and so some things changed. Like I say, those things that we no longer needed to start the demonstration, dropped down in priority, and that automatically moved some things up. I think to answer your question, Dr.Frankel on the aqueous corrosion, we're still looking at just the storage period, but the difference between the atmospheric corrosion, that's the one where we're looking at this deliquescent-induced stress corrosion cracking, that one is very high on everybody's list. The aqueous one is more, I've got pooled water sitting around just causing general corrosion, those rates are not nearly as fast, you know, won't go through-wall in that 20 to 40 year license period, so that's why we made the adjustments we did.

I have to speed up here, so what are we doing to close the gaps? We have an integrated approach, I just want to point out Mike will talk about ring compression tests, the big thing I want to say there is, you know, we hear when he talks about ductile-to-brittle transition temperature. The example I like to use is, you know, the glass stirring rods that we all used in chemistry classes are brittle, does it mean they just break by sitting there? No, I can bend them, they don't break, I need to have a sufficient load in order to cause that to break, so that's what Bruce talks about when he does the CIRFT test, and again Paul McConnell will talk about these external stresses. I did want to take some time, because the others won't get into this, and talk about, you know, so what are we finding when we talk about realistic temperatures? So, just as an example, the figures on the right are calculations that have been performed by Harold Atkins and his team at Pacific Northwest National Lab, where they took the data provided by Dominion and the demonstration team, and removed conservatisms from the thermal models and said what do we calculate? I can tell you the industry team, using their conservative models came up with a P-Clad temperature of about 350 degrees C. You can see highlighted above, when we remove conservatisms, and just the thermal models alone, it drops you down to 315 degrees C. What's even more important is there are a lot of conservatisms built in to how you calculate the decay heat of each individual assembly. So, Oak Ridge National Laboratory went through, last year, using very detailed data provided by the utility, knowing the field designs, et cetera. And, when they recalculated the decayed heat, without all these conservatisms, it dropped the decay heat in that demo cask by 17 percent. So, PNNL is in the process now of redoing the thermal analyses with these, what we consider, realistic decay heats, and we expect the temperatures to drop even further, but I want to emphasize, if we apply that same methodology to everything that's been loaded, currently being loaded in the industry now, of what's moved conservatisms, et cetera, we fully expect and are confident that realistic temperatures are nowhere near this 400 degree limit, but much closer to what we see here. This is just a real quick example of Calvert Cliffs and what they reported in response to requests for additional information from the NRC. It's a very busy table, but what I want you to note is in column two, it shows what is the decay heat loading for the real cask, even though the license limit is 21.12 kilowatts per canister. You can see what it really was, and it's only 55 to 85 percent of the design basis. Still, when they use their conservative model, when they use the decay basis heat load, but the realistic time that it took to dry, we can see that, I believe that it's column five or six here, we're looking at temperatures in the 218 to 309 degree range, even though in their license application it says, "If we reach steady state during vacuum drawing, we get to 394." You go through, you can see when they back-fill with helium, again temperatures are on the 222 to 265 degree range when they transfer the cask. So, you put it in this canister, and a transfer cask to move it out to the pad, that's a period when you have very limited heat transfer capability, and you look at the temperatures. If they assume that the outside air is 103 degrees Fahrenheit, a nice hot day, and that is average for a day, not just what the peak in the day would be. Again, you're getting temperatures of 239 to 327, so I want to emphasize just the amount of conservatism that exists in the licensing cases, and I want to emphasize, we as the DOE program aren't recommending changing licensing methodologies or protocols, but realize when it comes to effects, such as what Mike and others will talk about, you need to keep this realistic thing in mind.

Just real quickly again, I'll go through hoop stresses, you know, it's a function of what we see here. Oak Ridge, using that same data from the Watts Bar Unit 1 reactor, did modeling of what are the hoop stresses going to be on standard rods, that's the regular rods we have. You can see peak hoop stress of 60 mega-pascals. So, when Mike gives his presentation, keep that in mind with what he's been testing in the past. I do want to be fair, the red dots are for something that's called IFBA, which stands for Integral Fuel Burnable Absorber Rods, this is a relatively new design that one vendor uses, where they put a zirconium boride on the outside of the pellet to act as a neutron poison over time. One of the things generated during that reaction is release of helium. You get a lot higher pressures, which is why the hoop stresses there are so much higher. But, even in this case, they used 400 degree C P-Clad temperature. If I'm not getting there, clearly my hoop stresses are going to be lower than that. So, just to summarize, we've identified these gaps, we're pursuing an integrated approach to closing them. We, me especially, was surprised that discharge burnups are not nearly as high as everything we've read over the last few years of where we thought it would be going. And, that has big implications, because again, once you reach 45, it's not like things magically change, it's a continual process. All of our future testing, or as Ned mentioned, the sister rods that we pulled to support the demonstration, we'll be looking at doing testing in these realistic ranges, realistic temperature, realistic hoop stresses, as opposed to the higher licensing limits that we focused on in the past. And, I'll go so far as to say, is as we put these pieces of the puzzle together, including what Paul McConnell will show on what kind of stresses do I apply to these things down the road, we have confidence that high-burnup fuel, actually all cladding, will continue to meet its safety function over time. So, again I do have references for you to look at, so thank you.

Thank you, Brady. Questions from the Board? Yes?

Thank you, Brady. Lee Peddicord from the Board. As you get to these better estimates of temperatures, a more realistic picture, what are some possible other implications that might arise from that, beyond what you talked about? The thing that comes to mind would maybe be, maybe be things like the drying process is residual water, lower temperatures. Might that suggest there could end up with more water depending on how you modeled all these processes?

Yes, thank you Lee. I agree some of the implications of these lower temperatures are indeed how it affects drying, so we actually have a meeting set up with Travis Knight, who's doing the IRP on drying the end of this month, where we'll be discussing that and trying to get him to change his test matrix to look at these lower ones. I know the group that PNL, Harold Atkinson’s Group, we're going to ask them to look at what that implication might be. The other one is, you know, back to the concern in a meeting the Board had regarding the stress corrosion cracking, obviously if fuel temperatures are significantly lower, external canister temperatures will be lower sooner, and so you know, we're already working that into our plans of, OK now as we test canister, we better look at things happening faster. So, absolutely.

And for me, just a follow on. So, might this lead to maybe some changes in procedures, as you are loading casks and so on, as you get, really a better feel of actual temperatures?

It could, like I say, within the DOE program, you know, we're doing this purely as R&D, we're not trying to influence NRC, or industry, one way or the other. We do have meetings with the NRC, and industry, open public meetings, where those issues are discussed.

Other questions, Gerry?

Frankel Board, just a quick clarification, last slide you showed these plots of hoop stress. Is that the hoop stress at temperature or at room temperature, what's...

What Oak Ridge modeled here again was Frapcon, so using that code to say, what is the creep down in pellet swelling to estimate what my void volume is? What's my fission gas release, as well as my initial field pressure, to give me pressures. But, they then estimated that the peak clad temperature during drying was 400 degrees C.

But, the stresses would be in the hoop stress, is during in storage conditions, is that right? So, I just wondered, so you have the internal pressure being ten percent of it, can we assume that's from this pellet swelling, the major part of the hoop stress then, is it?

So, I'll answer it this way, in a paper published last month by NRC, including Dr. Einziger, they did an analysis to look at the pellet swelling and helium release. The conclusion was, at least for the first 300 years, there was enough hoop stress from that, that I can cause creep, but not enough to cause creep failure.

I'm just looking at the data in the bottom left compared to those calculations. So that's, you know, at ten percent of the, of the stress is associated with internal pressure.

Well OK, so again, remember you know, what, to calculate the hoop stress, you take that internal pressure and it's a function then, you divide by cladding thickness and the cladding diameter. So, that's where it comes from. I'll let Mike address where it might come from, cause he's, he's better at that.

OK.

Efi?

Efi Foufoula, Board. So, so you repeat it twice that it's not the step function kind of behavior, but it is more transient. Yet if you go back to your plot, I was just thinking that, you know, if you, if you see that there is a factor of four in case of, say the fission gas for about 50 megabytes per day, and you put that together with the frequency plot of the bell map that you showed. And, increased volumes after the 45 to 50. All this compile in a way that my gross calculations here, if five percent exceed this probability in the burnup above 50, translates to a 20 percent of the probability in hydrogen, fission gas, and all of this, you see what I mean here.

I see what you're saying, you know, but I do want to point out, you know, so this plot here takes all of that into account, cause it's a function of burnup at the bottom, so it does account for, you know, the extra fission gas released, the extra creep, the extra pellet swelling, and even so you can see, the lower burnups, you know, hoop stress is 50 megapascal, only at the higher ones are we getting to 60. And again, I would contend that at lower temperatures we'll actually be below that. And, I want to emphasize that this is all come about in the last year that we've had these types of analysis done, you know, so we're actually pleasantly surprised, I guess you could say, as to we thought we had a real problem on our hands with hydride reorientation, but when we look at this new information, put all the pieces together, we still need to look at it, our plan is to do all the testing that we said we would, all the modeling that we said we would, to prove what is our new hypothesis of we don't think it matters too much.

All right, Sue?

Sue Brantley, Board. You mentioned a couple times that you were surprised to find out from the EPRI fuel reliability database that, you know, you said the use of high-burnup was less than you had expected. I think you mentioned that you called somebody last week to get that information, so what my question is about is, what's in that database, how much access do you have to it? Could you have just looked into that database yourself, and maybe don't have the facility because it takes a little bit of, you know, hands-on, or you know, just talk a little bit about that in terms of your access to that information, and what's in there, and that sort of thing.

Sure. OK, thank you. So, this GC-859 database is available through the Energy Information Administration. If you look on the website, and look up GC-859 on that, it gives you a contact information, you can get that. So, I now have that database, it's all Microsoft Access, and tons and tons of different sheets. For those of you who were at lunch yesterday, you saw John Scaglione from Oak Ridge present, they were able to get a copy earlier because they helped provide QA validation to that, and as they're going through and pulling out that information, that's what's helping to build their database. So, it actually is publicly available. Our next step is to work with Oak Ridge and others to spell out very specifically, OK what years were what burnups, what type of cladding is it so we can really focus our efforts on what the potential problems might be.

OK. I'm sorry, we're going to have to move on to keep this on schedule, but there are question periods throughout the morning, and I'll accumulate them at the end, so that questions can be asked. So, the next speaker is David Tang from the NRC.

Thank you, Rob, for including us in this public meeting to discuss NRC regulations on high-burnup fuel, storage, and transportation. See, we will look into regulations applicable to spent nuclear fuel structure evaluation. As a structural performance provides the basis for evaluating other operating features, such as shielding and the criticality safety functions for the spent nuclear fuel storage and transportation. Since the cladding integrity of high-burnup fuel is the major topic of this public meeting, after an overview of the regulatory tools we have, and the tools being developed and being updated, we're going to move into the conclusions part as the observations we have had, based on the industry and the user community applications. Ten KCFR Part 71, and Part 72 regulations cover the spent nuclear fuel as a content or equivalent. They do not differentiate the high-burnup fuel from the regular burnup fuel. The spent fuel must be properly described in the application or Safety Analysis Report, SAR. For instance, paragraph 72.236(a) requires specification of details of spent nuclear fuel to be stored, such as burnup, PWR of BWR, et cetera. Paragraph 71.33(b)3 requires package description to include the content of the package, in this case chemical and physical form of the content. That means spent nuclear fuel, in our case. On the spent nuclear fuels frontal performance criteria, Paragraph 72.236 (b), (c), and (d) requires establishing analyzed fuel geometry under normal operating and postulated accidents for criticality and shielding evaluations. In the transportation regulation, paragraph 71.55(d)2 states that during normal conditions of transport, geometric form of content is not substantially altered. In the same paragraph also states that after hypothetical accident conditions, the package must be sub-critical, assuming the fissile material is in the most reactive, credible considerations.

The Part 72 regulations also specify the spent nuclear fuels storage renew provisions for specific license. This paragraph 72.42(a) requires that the application to include the description, the discussion of renewal period not to exceed 40 years, and also brings up the subject of TLAA Time-Limited Aging Analysis, and also the other subject AMP, Aging Management Progress for the structures, systems, and components important to safety. For the storage renewal, in terms of certificate of compliance, COC, the language is there dealing with renewal period, and TLAA, and also AMPs. There are similar language there. As to the spent nuclear fuel structure loading conditions, they are provided as guidance in the storage assistance standard review plan, such as NUREG 1536. Primarily they are dealing with handling accident, side-drop and end-drop. They are described in the standard, they are described in the safety analysis report to be submitted, and also non-mechanistic tip-over accident to be evaluated. For the transportation part, the structural loading conditions are very prescriptive for performance-based regulation. For instance, normal conditions of transport, there are two regulations. First, Regulation 71.71(c)C5, and also the next one for the free-drop condition. For the hypothetical accident conditions, the 30-foot drop scenarios need to be addressed, 71.73(c)1. The cladding integrity of high-burnup fuel had drawn a lot of attention in the user community, in the following areas. First, fuel rod structural performance by analysis. It is usually done by computer analysis programs to define the configurations, such as, such as the fuel reconfiguration to be evaluated and considered by criticality and the shielding evaluation. However, in this case, the license in practice has been considering the de-fueled rod as assumption, which is bonding without interaction with the fuel pallet for a composite action consideration. In this case, the cladding temperature, mechanical properties of matures are of interest, such as Young's modulus, use trends, and elongation limits for various cladding materials.

So, Mike is going to talk about the ductile-to-brittle transition temperature this afternoon, and Bruce is going to talk about demonstration of fuel rod performance this afternoon. So, I'm not going to dwell on that; however, our observations are for the DBBT part, we believe that ovalization of de-fueled rod, the performance here is of secondary importance. In a sense it is related to local cladding fracture rather than materials rupture, the raw breakage. For the testing done at the Oak Ridge lab, it appears to suggest that for the circumferential hydride condition, they're as tested, the cladding or the fuel rod appeared to be very ductile. And, we understand tests have been done for some other kind of materials through DOE programs, and that also supports what has been observed for the, for the rod as in fuel rods performed under NRC sponsorship, with Oak Ridge lab. However, we are most interested in the phase two testing, which will involve re-orienting hydride. Our expectation there is, the performance perhaps is pretty similar to what the currently achieved results achieved for the cladding with the circumferential hydride. I'm going to skip these two slides. You can, Mike and Bruce are going to talk about this afternoon. Here are the regulatory basis we consider in licensing action review. On the top of this hierarchy is a Code of Federal Regulations, Title 10, Part 71, Part 72. Part 71 deals with transportation, Part 72 deals with storage. The next one is regulatory guide, we updated guides, regulatory guides on a regular basis, and the next is standard review plan. And, the NUREG, new CR, Contract Report, Intra-staff guidance, it has a faster turn-around, as far as review cycle is concerned, and the regulatory summary, RIS. And, of course, we rely a lot on the NRC, and DOE, and National Labs research results. I will skip this slide as well, because they are in good use. ISG-11, ISG-24, and a couple of these NUREGs dealing with the Oak Ridge test results, and consequence analysis. So, I'm going to focus on reporting the current status of these regulatory tools in progress being updated, and revised. NUREG 1927, Rev 1. The draft was out for public comment last year, July. It has been there for many months. The objective for this particular update, we introduce changes in the draft, such as Appendix B, Example M's, including high-burnup fuel, monitoring and assessment program, welding the stainless steel canisters. It's in good progress, and we are going to engage ACRS on final guidance, next month or in April. We expect to publish this final guidance this summer, this year. ISG interest staff guidance in progress, primarily ISG number two, Rev one, deals with retrievability. This guidance provide flexibility in compliance with 10 CFR 72.122(l), retrievability provision. It will provide guidance for system-based retrieval. The ACRS sub-committee meeting will meet next month, and we expect to publish that ISG summer this year as well. Regulatory issue summary in progress. Considerations in licensing high-burnup fuel in dry storage and transportation. It was noticed, quite some time ago, an idea is to provide technical details for high-burnup fuel licensing approaches. And, primarily we are incorporating the NRC and the DOE research results to supplement the licensing practices we have had. And, which is being harmonized with changes made with NUREG 1927, and the changes being made in ISG 2, Rev 1. This, this is drawing a lot of resource from us may eventually become a NUREG report. I'm going to skip these NRC/DOE research programs. You can read yourself. But basically, I would to use this opportunity to conclude our observations as such. First, structural evaluations have provided a reasonable assurance for the staff to find that the high-burnup fuel will perform adequately for storage and transportation, under normal and accident conditions. Other considerations, such as moderate exclusion, burn-off credit, and the consequent analysis will provide additional basis for finding that the high-burnup fuel can be safely stored and transported. The NRC and DOE research results serve as pillars in confirming the cladding structure integrity for storage and transportation. Finally, we believe challenges still remain. Effective use of NRC/DOE high-burnup fuel research results, and their timely incorporation into the staff guidance document. Thank you.

OK, thank you. Questions? Gerry?

Frankel, Board. So, you know I understand the NRC needs the fundamental and technical understanding of the phenomenon in order to, you know, assess the appropriate regulations. I'm just a little bit confused about this, you know, the relationship with NRC/DOE relationship, you know, in scientific work that you are closely involved with. I know this is difficult work, and you know, maybe the, you have the center at SWRI [South West Research Institute] that's dedicated to helping NRC, maybe they can't handle this, but how do you deal with the possible perception of conflict of interest, when you should be performing independent assessments of, you know, license application, when all along you've been working closely in developing the understanding?

Good question. I will answer this way. We independently look into the applications, you know, we use the DOE research results to confirm what our, say our licensing practice is, and the theory and the basis, we, we accept ourselves. And, of course, these bases are of very sound technical basis and grant. So that, did that answer the...

You don't see, you don't see that there's a conflict of interest, is there?

Uh no. I don't see this, at least for this particular subject we been doing with, I think the research results, seriously supplement what we have observed for quite some time. We have say licensed say spent nuclear fuel, low-burnup and high-burnup for years. For many different cladding materials, and we know that, say, what, say elastic performance that fuel clad is going to perform, and the potential, say brittle fracture properties for high-burnup fuel, but there are technical bases for us to make assessment and to make determinations.

Yes.

Turinski, Board. I'm going to ask you several questions for different states of fuel failure, and I'd like you to answer it in two senses, two ways. One is, is this still licensable, that state of fuel failure, and number two is what's the safety significance. We know licensing should assure safety, but safety sometimes is assured, even without licensing in there. So, the first state of, and assume the canister and the cask, their integrity is still there, so we're just talking about fuel degradation. So, if one had through-wall cracks, either during storage or transportation, OK? But, the fuel geometry was retained, there's no fuel particles that were disbursed into the canister, would that be licensable? Just cracks, through-wall cracks?

The answer is yes.

OK, so by implication it's safe, OK? Now, we have through-wall cracks, but the cracks have opened up large enough that there actually is some U02 particles that have come out of the fuel, so they're no longer in the fuel rod, they're somewhere in the canister or the cask. Would that be licensable?

That depends. There's a need for evaluation as to what this damage or conditions of fuel really amounts to. In our licensing basis, having very conservative, and we looked into fuel performance, and canister or cask performance, in the final bounding behavior perspective, so on a case-by-case basis, sure we're going to look into that. For instance, like if the fuel really is damaged, there are a number of ways that applicants propose for one. Secondly, if there is assumption, and like say rubblized fuel in certain fashion, but under certain transportation conditions that the applicants analyzed, the condition analyzed the so-called analyzed condition, the rubblized fuel, and all these assumptions, in the move forward to describe, to demonstrate that for shielding, criticality, and thermal function, the canister or cask can still be maintained, then the answer is yes.

Thank you.

Other questions? Mary Lou?

Mary Lou Zoback, Board. Just a brief follow-up. How would you know either of those conditions existed inside the canisters?

If I may, if I understand the question, the, the condition of the fuel, as described by the applicant, would be a part of the application, or safety analysis report. If they, if they assume that the fuel would be intact, or there's no fuel damage, and there ought to be some evidence to, evidence in the sense before the storage or transportation, there would be some inspection, or some other report to substantiate that particular description. So, that is the starting basis.

But, if the cracks were noted before they put it in, but they actually opened or released particles of fuel, you wouldn't have any idea that it occurred, after it went into the canister.

You're touching on a very good subject, what we call the regulatory gap. For instance, what you mean by transporting stored fuel, after 20 years or 40 years, you're transporting. The condition of the fuel ought to be sufficiently described so you know what the condition, or the content, the geometry, and the properties is such that during the transport, it's not the content, geometry, or what not substantially altered for one. Secondly, upon arrival at the storage, storage site, and for the storage purpose, the content or the content equivalent, ought to be properly described as part of the certificate for storage. So, that remains a challenge, but however, we see that perhaps some kind of a sampling, if there is such a need. If we do need, if we do know that toward the end of storage, the condition of the fuel, based on aging management, evaluation, and the time-limited aging analysis, there is a certainty, or there is some likelihood, that the conditions of the fuel, or the content of the package is very well defined, then this transportation can take place. Now, the next challenge is, whether the, the normal conditions of transport, a vibration or some other conditions will cause additional damage to the fuel, which is up to this point, so lab test results answer some evaluation will help us to establish the condition of the fuel for the next step of operation.

OK, thank you.

All right, thank you. I think we'll have to push on with the next speaker. But, I'll point out that we'll be calling all three speakers back, and time allowing, maybe a few more questions. So, thank you very much, and the next speaker is Mac Louthan?

Before I get started, I want to thank the Board. They question both my mental and physical endurances. They asking me to remember stuff that happened over a year ago, and then there were ten inches of snow at my house yesterday morning, and I had to shovel my driveway out in order to get here for this meeting, and so I need to thank the Board for that.

[laughter from the audience]

What, what I want to talk about, are discussions that we had in Jackson, Wyoming, a little over a year, almost two years ago now, and all these discussions are published in

SRNL/SIT, whatever that number up there is, and how do I work this, Scutter?

Forward, this would be forward, yeah. I, I am not real good on high technology things like buttons. The presentation, the presentations at Jackson focused on two things. The potential for hydride reorientation in spent nuclear fuels, and then could that reorientation cause the fuel to get brittle so that it would mess it up during the storage, transfer, and ultimate disposition of the fuel, and the work described is part of the program that DOE's had going on for a long time to assess the performance of irradiated fuel. What, what we want to talk about today really involves some simple-minded stuff. You put a Zircaloy-clad, or Zirconium-clad fuel in a reactor, you're exposing it to high temperature water, you're going to have that corrosion reaction occur. And we're going to have hydrogen enter through the surface of the cladding, and we're going to form an oxide film. We should also notice that dissolved hydrogen expands the lattice, it doesn’t quite fit in the interstitial sites, expands the lattice, and because it expands the lattice, it interacts with the lattice strains, and the hydrides precipitate when the solubility limit’s exceeded. At room temperature, almost no hydrogen is soluble. When you go to a higher temperature, hydrogen solubility increases, and one of the reasons some of the stuff Brady was talking about, where you were lowering the temperature, the actual temperatures are less than what we had anticipated during drying operations, is in many cases, we're not going to dissolve all the hydrides that is in the fuel, in the cladding, during drying. And, since we don't dissolve all the hydride, that might be a very, very good thing for us. This is an optical micrograph of a cross-section of a cladding, and you can see the oxide film on the surface, you can see the hydride rim just beneath the oxide film, you can see that they hydride content and the oxygen content changes as we go across the cladding. And, the question I would ask any of you is what's the hydrogen content of that clad? It depends on where you assess the cladding. And, when we talk about hydrogen content of 450 parts per million, it may be 450 parts per million averaged across that cladding, but it's not 450 parts per million throughout the cladding, and that's important for, for us to recognize, because there are a lot of variables that are going to influence how that hydride, how those hydrides are distributed in the cladding. First, the hydrogen content in distribution, you saw that in the cross-section that I just showed. We will show some slides and say the stresses on the cladding are very important, and these are both the macroscopic and the microscopic stresses.

One of the things we heard at the Jackson conference on several occasions is data that is developed on non-irradiated materials is not directly applicable to irradiated material. And, that's an important thing to have, and during the drying operations, we take a material out of a reactor, and we have this kind of a distribution, this is a high-burnup fuel, but have this kind of distribution of the hydrides, and then after we have dried it, the hydrides may reorient, and it depends on what that drying temperature is, because that's going to determine how much of the hydrides that we dissolve, all right? As we cool from the drying temperature, the existing hydride morphology is going to affect the way the hydride precipitate in subsequent operations. For example, when the first hydride precipitates, it punches out a dislocation loop, right? You heat that up and dissolve the hydrogen, you have dissolved the hydride, but that dislocation loop is still there, that becomes a favorable site for subsequent hydride precipitation, and so what was there before is going to influence what's going to come in subsequent operations. And, then we find, and you saw that in some of the slides that were presented earlier, where the hydrogen, burnup, the amount of oxide developed on a fuel, cladding during burnup, depending on the alloy. We have some alloys that don't oxidize very rapidly, we got some that oxidize fairly rapidly, and the alloy content is going to make a big difference in the way the material is going to perform. And, the texture of the cladding, the hydrides, at least the little tiny hydrides, all tend to form on the basal planes. And, so the texture of the cladding is going to make a big difference in how the hydrides precipitate. So, you got all these variables that you, that you've got to put in, if you're going to try to describe what's happening. And, let me show you the effect of cooling rate.

Now, this is a BWR cladding that has a Zircaloy, Zirconium liner, and this same cladding, we took cladding, heated it up to 300 degrees centigrade, after irradiation, put a stress of 70 MPA across the cladding surface to see if we could get some stress reorientation, and then we cooled it, we, I use the word we, and none of the data I'm showing did I develop, all right? It's stuff that was shown in the, in the Jackson meeting, and I'm stealing other people's data without giving them proper credit, but if you go to the publication of the Jackson conference, proper credit is given to all the slides that are in there. But, they cooled it, if you cool it 30 degrees centigrade per hour, you get one kind of distribution, and cool it .6 degrees centigrade per hour, you get a different distribution. And, then you look at that and you say, "Hey, look at this hydrogen depleted zone between the inner surface of the cladding, and the outer surface of the cladding, and could that hydrogen depleted zone have a big impact on the way the cladding's going to perform in subsequent operations?" And, then we say, this is something I had to throw in here because I figured some of you all were used to hearing about steels, where the Navy, and the Air Force, and the Army will cathodically charge, or work with some steel, and when they do that, they bake the hydrogen out. And, they say "Why on earth don't you bake the hydrogen out of the Zircaloy?" And, the reason you don't bake the hydrogen out of the Zircaloy, is hydride, hydrogen absorption is endothermic, and so the hydrogen would rather sit in the Zircaloy, than sit outside the Zircaloy as a gas, and so you heat the hydrogen up, heat the hydrogen-containing cladding up, you just dissolve the hydrogen. You cool it down, you re-precipitate the hydrogen. And, so what we want to see is the change in hydride orientation that might occur during cool storage, during drying operations, during dry storage, will that impact the mechanical behavior of the material by providing a path of weakness across the cladding surface. And, if we look, this is the kind of reorientation you can get. This is tensile bar, but here you can see the hydrides are almost all in this direction, you heat it up, put a stress on it, cool it back down and let the hydrogen re-precipitate, and now they're all, if this was cladding, I guess this is a piece of cladding, they radial hydride precipitation. And, what we're concerned about is that this path becomes a path of weakness across the cladding surface, the cladding can break very easily at low stresses, and particularly at stresses during handling and movement of the assemblies around. The variables that affect this would be the irradiation history, how much hydrogen you have, what's the hydride orientation, what's the post-irradiation thermal history, what's the cladding temperature, cause as you go up in temperature you're going to have less hydrides in there, come down in temperature, you're going to have more hydrides in the surface, and then what were the cladding stresses when the hydrides precipitated? You can see this in this sample. We've taken some cladding, and we've taken it, here is 90 parts per million cladding as irradiated, here's the ductility of that material as a function of temperature. We take that same material, basically the same material, it's a high-burnup M5, Mike may talk more about that later on, but you take that material, heat it up, put a stress on it, cool it, and you get the hydrides reorient, and instead of being ductile in this ring being tested, where you're taking a ring section of the cladding and just squeezing it, and measuring the ductility, instead of being real ductile, like it was when it was irradiated, or like it was when you heated it to 400 degrees and cooled it, you find that you get precipitation of the hydrides back into that radial hydride orientation, and the hydrides in the radial hydride orientation limit the ductility of the material, and causes it to undergo transformation from ductile behavior to brittle behavior.

I want to emphasize that in metallurgy at least, the ductile-to-brittle transition temperature has a special meaning, it's measured by special techniques, and we need to be careful with our use of the word ductile-to-brittle transition temperature. What we're talking about is the temperature where, in this particular test, the material went from ductile behavior to brittle behavior. And, you can see that this slide just points out one variable, and that was the stress of the cladding during hydride precipitation. Notice with 90 MPA stress, no brittle behavior, then we go to 110, we get brittle behavior, and go to 140 MPA we get brittle behavior at a higher temperature. Now, it's important to notice one other thing in this slide. All these samples were heated to 400 degrees C could dissolve almost all the hydrogen that was in the cladding. If this had been in a drying operation instead of going to 400 degrees C, we'd gone to 285 degrees C, we would not have, or we may not have, well in this case we would have, we would have dissolved all the hydrogen. But, in some of these, in some cycles, we wouldn't have dissolved all the hydrogen. The existence of those circumfrentially-oriented hydrides would make the tendency to form the radial-orientated hydrides much more difficult. And, so that's emphasizing the importance of knowing what your drying temperature is. If we're going to try and predict the behavior of a fuel cladding after service, we need to know at least all of these things. We need to know what the radiation exposure was, we need to know what the cladding alloy is. There's a tendency in a lot of the literature to talk about the behavior of cladding, and what we need to talk about is the behavior of specific cladding alloys, because there are alloy to alloy differences in this material. You need to know what the hydrogen content is, what it's distribution is, and what it's distribution used to be before you dissolved all the hydrogen. We need to know it's thermal history, in the reactor, in the pool, during drying, and during dry storage. Take that information with the service history, and we can calculate, and I'd rather use the word estimate, what the hydrogen, hydride morphology evolution is going to be. Are we going to go from circumfrentially-oriented hydrides to radial-oriented hydrides, or are we not. Once we know what the hydride morphology evolution is, we might be able to predict the mechanical response of the used fuel. We've done a lot of testing to try and predict the mechanical response of used fuel. One of the problems, we, a lot of the studies have been done on non-radiated materials, and the radiation affects the behavior, but working with non-radiated materials is very inexpensive compared to working with radiated materials, and so what can we learn from non-irradiated materials?

My feeling is, and the feeling at the Jackson meeting was we can get typical behaviors. We can see that there are critical stresses for hydride reorientation. We can see that there are roles of stress state, and I'll talk a little bit more about that in a few minutes, we can see that there are alloy-to-alloy variations, but we can't, from these non-irradiated tests, predict what's going to happen in irradiated material. All right? Then we use non-standard mechanical tests to develop the mechanical properties, ring tests, contention and compression, bin test, fatigue evaluations, and if we're going to model the behavior and predict what's really going to happen, we need to somehow or other, get these non-standard tests to meet with ASTM standard tests so that we can put 'em in the models appropriately. And, the results of any of these tests are dependent on what, how much hydrogen we have, where the hydrogen's located, and how the hydrides are oriented. And, to show you some of the things, this critical stress I was just talking about, this was a tensile specimen that's tapered, so that the stress along the axis as tensile specimen varies, and the stress at this area was 147 MPA, and down here it was 177 MPA, that's not a great big variation in stress, but look at the difference in the way the hydrides precipitated in that material. You can see they go from almost all along the axis of the tensile specimen, to they’re perpendicular to the axis of the tensile specimen. And, the behavior of the tensile specimen would be very different in the 147 MPA area that in would in the 177 MPA area. All right? Not only does the stress matter, the stress state matters. Look at the transition of that critical, or threshold stress from hydride reorientation when we go from a material that has a uni-axial tensile stress to a bi-axial tensile stress. Notice that it goes down. An awful lot of the data that we have involve materials where we have worried about behavior under uni-axial stresses, and in the cladding, we're at a bi-axial stress ratio of about .5, and look at the decrease in the stress necessary to cause reorientation when we go to a bi-axial stress, and we have not spent enough time in looking at that. Then we say we're going to characterize the hydride morphology. We've got to spend some time seeing what the hydrides look like in the material. Most of the hydride morphology characterizations are done by optical microscopy. We show a slide in the report from Jackson Hole meeting that has three specimens, prepared at three different sites, that have basically the same hydride content, and they look totally different, because the metallographic preparation was different in those samples. And, so we need to get some consistency. We need to get some consistency so that what is done in lab A, looks like what is done in lab B, it's done on the same material. And, then we need to get some consistency on how we're going to measure what the hydride orientation is, or the variable that's going to impact the fracture process. We could talk about the number of hydrides that are oriented perpendicular for the tensile stress, we could talk about the continuity of the hydrides, we could talk about the total hydrogen content, and all these characterization things can be compared. And, for example, here's radial hydride concentration compared to the continuity factor, and you can see that they're related, nicely related, but it's hard to take what one data set says and compare it to what another data set.

We need some consistency in the way we're going to do that sort of thing. And, and testing to look at the impacts of hydride morphology, we do straining to fracture, we can do that in a tensile test, we can do that in a ring test where we're doing ring compression, or ring tension, we can do that where we're taking a tube section and bending it a little bit, and we can look at the importance of strain rate and constraint. Anytime you're looking at brittle fracture, constraint makes a big difference in how the material is going to perform. And, we need to pay attention to the constraint that's on material and often we tend to minimize the amount of constraint we put on material when we test it.

We do different kinds of fatigue tests, and what we need, at least came out of the ASTM conference, was we needed some qualified tests to relate to results we get out of these kind of tests, to qualified ASTM tests, so that a modeler can use those to predict very well what's going to happen during storage and transport. And, to show you some of the difficulties that one might have, looking at brittle behavior, if you do a tensile test, the transition from ductile behavior to brittle behavior is going to depend on the hydride orientation. If you take that same kind of material and do a ring compression test, it's going to depend on the hydride orientation. But, you take that same kind of material, instead of squeezing or pulling on the sample, you try to bend it, it depends on the total hydride content, and not on the hydride orientation. And, the ductile-to-brittle transition temperatures that you measure in a ring compression test are different than those you measure in bin test on the same material, hydrided exactly the same way. And, then we need to pay attention to that memory effect that I talked about earlier. This was, is an opinion that is expressed in the write-up, but it's more strongly my opinion, and it's presented in the write-up.

This is a Louthan impact on this. I think we need some coordinated efforts to integrate the various test results. We tend to have testing done at one lab, analyzed at that lab, published by that lab, peer reviewed for technical report, all right? And, then we have similar kind of tests done at another lab, analyzed by that lab, peer reviewed for a technical report, and we don't tend to have integration of the work that we're trying to put together in assessment, and not just the mechanical test results, but all the behavior. We need to get more integration. And, here's determining the onset of brittle fracture, this is a picture Mike had in some of his work. It shows real brittle fracture of a material. The onset of that brittle fracture's temperature dependent. It's going to depend on the hydrogen content, the hydride orientation, and the alloy, and we saw at least in one report in the Jackson Hole meeting that you could have 100 degree K shift in the ductile-to-brittle transition temperature if you change the strain rate. And, so we need to pay attention to the strain rate, which may say that the ductile-to-brittle transition in a drop test is different that a ductile-to-brittle transition when you're just normally handling the spent fuel.

The purpose of all the evaluations that we're doing is trying to answer this question, will the used fuel cladding crack due to hydride reorientation? We say in the pool, that's very unlikely. During drying, the new drying temperatures retain hydrides may be beneficial in minimizing the hydride orientation. What we need to worry about are the drops in sudden loads, and we need to decide whether we're more concerned about a longitudinal split, or a guillotine break on the cladding, you know? And, I think we had a couple questions really worrying about that, could we license a material that had a crack in it? We certainly would have difficulty licensing a material that had a guillotine, guillotine break in it. And, then what happens when we transfer this material to dry storage? The conclusion that the Jackson Hole conference gave us was that the phenomena is fairly well understood. There are other things to be learned, but we're spending a lot of time with very little impact on the way we're going to handle spent fuels, by finding more fundamental data about the hydride reorientation process. The problem we have is transferring general understanding to specific situations, and this requires integration of multiple control and response variables. When you start doing these things, you come to the conclusion that standards will be necessary, and that you're going to have multinational interest in the standards. Experience suggests that we're going to have minimal problems, but when we suggest that we're going to have minimal problems, that is a gut feeling, we can't prove that we're going to have minimal problems, but we can have an awful lot of engineering assessment that says we're going to have minimal problems.

The recommendations that come out of the Jackson meeting were sort of these. We need to integrate our efforts. Get everyone on the same page by creating multiple site testing and analysis team. I had a poem that I was going to put in here, and my management would not let me put it in there. Right. So I’m going to quote it. But it’s not in there, all right? It says, “Each to his own work will refer, ignoring those who don’t concur.” And I think in an awful lot of technical things, we get in that problem.

We need to establish ductile-to-brittle transition temperatures that are relevant to fuel-handling situations.

We need to develop standards for characterization and testing of used fuel cladding and cladding alloys.

We need to recognize and attempt to understand the alloy-to-alloy differences.

We need to realize that there are no model alloys. We can’t take this alloy and say this is going to behavior of spent fuel cladding. We can’t take this assembly, or these two assemblies, or these five assemblies and say this is the way assemblies are going to behave during dry storage. We’ve got to be able to understand alloy-to-alloy and assembly-to assembly differences. And if we’re going to do this cheaply, we need to develop a protocol to use non-irradiated alloy testing to predict irradiated behavior. And so these were the things that came out of the conference, and I took a whole day of conference and did it in what should have been 15 minutes, and you told me to hurry. And so, you know, being as compliant as I am, I hurried, and my normal talk is a whole lot slower than I went here.

I’m sure.

And that caused me to have some problems.

Okay.

But I’ll be glad to answer any questions if somebody has any, but I know my presentation was so clear that there’s nobody that could possibly have any questions.

Because of the clarity of your presentation, let me suggest that we call all of the speakers forward to sit at the table and then we can ask you questions as well as others because there’s some connection. So Mac, if you’d join at the table, and then Brady and David, if you’d come up.

And then, for the first questions, if there are any related directly to Mac’s presentation. Okay. Paul?

(Inaudible) maybe directly. I’m Turinsky from the Board. We’ve heard mostly about experiments, and I was wondering what the state of modeling is. Can we do things like dynamic MD simulations and actually predict reorientation and understand how the stresses cause the reorientations?

I’m going to sound cynical, all right. The modelers are very well able to predict what has happened and they have observed. All right. They are –

In the order of observation -

They are not real able to integrate the alloy-to-allow differences, the effects of irradiation and things like that into their model and predict what will happen. But we’re getting there.

Okay. And the second question, again a modeling question, can we do fracture mechanics? Do we understand, fundamentally, if we have hydrides, predict fracture?

Yes. If we – the more mechanical test data we have regardless of how we obtain it, if we know the hydride orientation, the hydrogen content, and what the path across the material is, there are fracture mechanics techniques that can be used. I will mention a guy, and this is very selfish mentioning, but Poh-Sang Lam at Savannah River, very capable in doing that sort of thing. And he works with Jay An in a lot of fracture mechanics kind of work.

Jean?

Jean Bahr from the Board. Given the importance of the hydrogen concentration in the cladding to begin with, what do we know about what controls that? Is that a function of how long the fuel rods have been in the reactor and exposed to water? Is it a function of temperature? Is it a function of the alloy composition to begin with?

Almost all the hydrogen in a spent fuel cladding is picked up during reactor operations. Almost all of it. If we have corrosion in a pool, some hydrogen will be picked up. And we talk about hydrogen – the reason we developed – and Brady talked about some of these new alloys that have been developed. One of the big reasons to develop these new allows was to minimize the corrosion. A result of minimizing the corrosion was also minimizing the hydrogen uptake in the material. And so we understand quite a bit about that.

Just to follow up then, is the corrosion a function – is it a linear function of time in the reactor and is that why high-burnup fuels are much more susceptible to this or are there some threshold time periods? How does that rate of corrosion vary with time and temperature in the reactor?

It’s not a linear function of time. The surface film that is forming on the alloy is protective to a great extent. Therefore, to have continual corrosion you either have to have oxygen – water disassociating on the surface, oxygen diffusing through the oxide film, or metal ions diffusion through the oxide film and reacting with oxygen at the surface. I’m not sure which of those controls the oxidation reaction. But the reaction slows down – but as we go to more and more burnup and the film gets thicker and thicker, the temperature of the bottom part of the film gets higher, and so diffusion increases, and it’s a fairly complicated process but very well understood.

Thank you.

So if I may, to go along with that?

Yeah.

What you’ll find is you have quite an axial distribution of the oxide layer thickness. The water temperature tends to be cooler when it first enters the core, so down at the bottom of the rod you tend to have less oxide thickness and as you move up you get more. And so I personally have never liked burnup as the discriminating factor for many of the things we talk about because it’s not just burnup, it’s what we call the duty cycle. What’s the temperature that it’s exposed to, so as I said, countries that tend to push their fuels a lot harder than we do in the U.S. will have higher oxide rates and more hydrogen pickup. It’s axial variation. It’s circumferential variation around the rod because is this rod next to a control rod, next to what’s called a water rod? Is it near the outside of the assembly? Just an awful lot of variables that feed into it. But personally, I see temperature – and Mac pointed out a good one. The thicker that oxide layer gets, the higher the temperature will be so that’s what kind of makes it take off faster.

And another thing. One slide that was shown at the Jackson meeting that really amazed me was the slide that had the oxide film on the surface and cracks in the oxide film. All right. Now the cracks in the oxide film produce stress concentrations in the zircaloy beneath the crack. And we would find – they showed the picture – the hydrides oriented perpendicular – radially oriented beneath the cracks in that oxide film.

Can I ask a follow-up question?

Sure.

Bahr from the Board. So you’re describing a lot of spatial variation within a given rod and among rods that are of the same composition but maybe different places in the reactor, so that sounds like a real challenge then in characterizing the fuel before it goes into storage, which rods do you characterize, which parts of the rods do you characterize to even determine what the extent of hydride reorientation is and where cracking might be?

Very true. So one thing I want to point out, and I’ll talk about it this afternoon, we are, as the UFD program working on the test plan for testing these sister pins, and one of the chapters in that test plan discusses the very detailed effort that Steve Marschman at Idaho and John Scaglione (sp) at Oak Ridge went through to say out of these 32 assemblies, which one should we pull sister pins from? And of those assemblies, which pins do we want to give us that kind of variation so that we’re looking at as broad a range as possible.

Maybe Dr. Tang could also address the regulatory issues associated with this because you said the licensing depends on the descriptions of the rods as they’re going into storage.

Very good. We looked into the fuel rod performance in terms of macroscopic performance. Macroscopic in this case regular strands of materials approach at moment of shear in the bearing, all this (inaudible), and we understand that there is interaction between the fuel pallet and the cladding which will increase the flexual rigidity of the cross section tremendously in the sense that during buckling event or some bending event, bending, in particular. The fuel rod will not likely to lose its bending capability. That is really the basis for us to perform licensing review for many years and up to the demo program through this Oak Ridge testing which reinforce and confirm our understanding of the fuel rod behavior. So we feel pretty comfortable as to where we are as to the type of challenges we have and we understand the variation in cladding properties mostly deal with corrosion resistance and many other safety concerns. But, as far as the mechanical properties of materials is concerned, they are, perhaps, consistent as you could look into FRAPCON program and these mechanic properties. So our – I think we have a good understanding of the microscopic perspective as well. But I will also add that the microscopic kind of behavior, getting into, for instance, negative fracture, fracture mechanics, cracking, localized behavior, which may affect the fuel damage as far as release is concerned. But as far as global behavior as to the fuel geometry change, it seems to be pretty local and secondary from our perspective.

Okay. Paul? I'm sorry, Lee.

Lee Peddicord from the Board. Back to Dr. Louthan. In looking at a couple of the figures in your presentation, we don't have readily available, but the question is, can you get into a situation on the orientations where you have mixed morphologies and orientations? You showed one in the cooling rate where you actually had a situation with a high cooling rate of both circumferential and radial cracks in the one particular case. But the – drawing on Figure 14, is it correct that these are only binary states? You have one or the other? Do you have other – can you have other sorts of morphologies of the orientation of the hydrides other than these two that we always refer to?

You almost always have mixed morphologies. And if you look at that figure, and I don’t know what that figure number is, where I was showing the change in stress on the tensilespecimens), you see there are mixed morphologies there. It goes from a morphology – no, back – back one more slide. You can see it goes where almost all oriented circumferential or along the axis of that tensile specimen to almost all oriented perpendicular to the axis, but in between there are mixed morphologies. And the tendency to mixed morphology, and Mike will show, I think this afternoon, a lot of pictures that show mixed morphologies in the cycles he’s testing.

Okay. And how about the mixture is only between radial and circumferential, nothing in between?

Well, what we – Mike doesn’t like that. Now I’m speaking for him. He likes the continuity path. How much continuity do we have across the cycle? And he compares his data with the continuity across the surface. But I have seen in tensile specimens where you’ll take a specimen that has that mixed morphology you’re talking about, and we found that there were some pretty – this was in 1961, we found that you could take a tensile specimen and make half of it have circumferential hydrides and the other half have radial hydrides. And you could take that specimen and pull it, and the crack would run along the radial hydrides until it reached a circumferential, and then circumferential hydrides would run along those. We found that the hydride metal interface was much more susceptible to fracturing than anything else in the material.

And if I could add just one comment, and that’s I speak much more simplistically than Mike does, but very simplistically, if you look at, you know, what’s on the right here, the crack path, if you will, during a pinch mode is very easy to get. When you have the mixed morphology, combination of radial and circumferential, a lot of what Mike has found is if a radial crack intersects a circumferential one, it basically blunts that and you get different behavior that way. So be sure to ask him those tough questions this afternoon.

But that kind of analysis fits with the fracture mechanics approach that we were questioned about. The circumferential hydrides can act as a crack-blunting mechanism and reduce the stress intensity on the crack tip as it starts to the side.

Okay. Efi?

Efi Foufoula, Board. So putting together all these three factors, which is the morphology we’re talking about with its special variability, the stress state that could be bidirectional or whatever, and the memory effect, which basically tells us that things are not additive, rather they are more duplicative, and knowing that basically we’re talking about the extreme cases that would be kind of accident cases where five or ten percent of the all combinations, I wonder, could it be that a typology of these extreme worst-case scenarios could be formed? In that case, if you put all these factors together, some of them will be in the extreme (inaudible) more than others. So could this be more systematically classified towards the typology of these conditions for accident-prone cases? That is the weak pathways that you called.

That – that’s one of the difficulties I was alluding to if I didn’t state clearly, one of the difficulties we have. It’s not only the stress state. That slide that I showed with the effect of the stress state and the slide I showed with the tapered specimen are both done on non-irradiated materials. We don’t have similar data on irradiated materials. And it’s done for one alloy. And as Mike will show this afternoon, different alloys behave differently to the same stress. And as Brady just brought out, different areas of the tube behave differently, have different cladding, have different oxide thicknesses, different hydrogen content. It is a very difficult concept to grab hold of, and I think to predict real accurately. So what we have to do is look at worst-case phenomena and say we’re not likely to have a problem even if we have a worst-case phenomenon. That’s my feeling. I’d let the regulatory man talk about that.

Let me try to supplement this discussion this way. If we let alone the pinch load failure mechanism, which will create fracture type of behavior at (spacer grate locations in general, then primarily a fuel rod is subject to bending kind of behavior during accident with potentially some kind of circumferential stresses if there are still gas pressure inside the fuel tube. Now, if you look at the biaxial stress state that circumferential stresses are – circumferential stresses in the cladding is, at the most, about 20% or 25% of the total stress magnitude compared to the bending stress challenge. Bending stress, in this case, drop accident, tip over accident so on and so forth. So from macroscopic structure performance perspective, we believe that once you can address the bending performance of the fuel rod, you are almost there as far as structural performance adequacy is concerned. So this localized behavior, or some of these secondary stressing facts perhaps need to be visited in a different perspective and how much emphasis you really need to invest your resource there.

All right. We’re at the end of the time allotted for this morning’s session. I’d like to thank all three speakers and particularly the interaction between the three of you as we’ve asked questions.

So we’ll take a short break and begin sharply at 11:10. Okay. Thank you all.

If you would find your seats, we’ll start in just a moment.

All right. Please take your seats so we can begin. That music is the signal that it’s time to get started again.

The next speaker is Mike Billone, Argonne National Laboratory. And we’ll continue along the same lines in terms of the talk.

Excuse me. I’m getting wired up. Okay. Thank you.

It’s a pleasure to come back and talk to you again. I was here in November of 2013, and it was a very dynamic exchange of information, ideas and questions, and I’m looking forward to the same. I notice my previous colleagues have passed the buck where I’m supposed to answer all questions. I’ll do the best I can.

So, the basic outline is an introduction, which is going to be important to get us all on the same page of what we’re talking about. Let me try the laser pointer. Yeah, okay, it works. I’ll talk about previous results we generated for high-exposure cladding. It’s kind of a misnomer to say high-burnup cladding because has burnup, cladding has exposure to neutrons. But I may slip and call it high-burnup cladding. Both in the as-irradiated condition, as sort of your baseline, that would be coming out of reactor, sitting in the pool, what are the properties of the cladding. And then following cooling from 400 degrees C, which is not a licensing limit, it’s an NRC recommended limit, peak drying and storage temperature.

The new data, as Brady has emphasized, the cladding is never going to see 400 degrees C, it’s just really the worst case, or not. Do things improve significantly as you lower temperature of the cladding? So the new data for high-exposure cladding and the as-irradiated condition and following drying from 350 degrees C, peak drying and storing temperature, and if we continue the program, we’ll continue to go down in cladding temperature, particularly when we get to the sister rods.

I’ll tell you what tests are in progress and then give you some summary and perspectives. And I’ve chosen perspectives specifically based on questions I get at the end of every one of my presentations where there’s misconceptions of how to apply this stuff, and it’s important that we put this in the proper perspective rather than just presenting data.

All right. So as a way of introduction, the Argonne program, what we do is we generate data for ductility versus temperature following slow cooling from 400 degrees C or less and decreasing hoop stress. We determine ductile and brittle transition temperature for each set of peak drying and storage temperatures and stresses. And for those material scientists in the group, one in particular, really it’s the ductility transition temperature. We’re measuring ductility, looking at where the ductility goes to zero. So that’s what we’re calling the DBTT, which is different than how it’s used in other branches of material science.

In parallel with just running mechanical tests, we like to understand why the material behaves the way it does. So we characterize the extent of radial hydrides and correlate with the decrease in ductility and the transition temperature. In particular, Mac has mentioned as we look at the effective length of radial hydrides, it’s called a radial hydride continuity factor, probably not going to use that term very much. I’m just going to say the effect of length of radial hydrides across the cladding. From data you could extract, we haven’t done this yet, stress-strain relationships for the cladding materials and failure stresses and strains for the PWR cladding alloys that we test. And that’s what you would use as input to computer codes. So we have the data to do it. We haven’t done that yet. We’re still running scoping studies just to determine ductility.

It’s very important what our collaborations are because we’re not, you know, we don’t exist as an island within the DoE program. And we have very strong collaborations – sorry for the acronyms – EPRI has already been defined. But the Extended Storage Collaboration Program has a Fuels Subcommittee that I’m Chair of. And through that Fuels Subcommittee and work with Oak Ridge National Lab, we’re trying to establish what are the relevant ranges of stresses for normal fuel rods, standard fuel rods, and specially-designed fuel rods with burnable poisons inside that generate additional gas. That is a major effort there.

With the PNNL and Oak Ridge collaborations, the relative ranges of cladding temperatures are being determined. That’s very important to our testing program and the application of the data that we’re going to show you.

And then the question is what are the loads, relevant loads, on the cladding? Are we challenging the cladding at all during normal conditions of transport? And also I’m very involved with observing, anyway, the fueled-cladding response to bending fatigue loads that Oak Ridge is doing. And that’s experimental effort.

All right. Loads of fuel rod cladding during drying and storage, primarily the internal gas pressure loadings. I mean, assuming everything is sitting there quiet, and you don’t have seismic events taking place. During transport, it’s been mentioned that during normal conditions of transport you have to include vibration and shock. Primarily axial bending. The term “bending” was used before. It’s axial bending is the primary loading mode.

You do have – at the next presentation we’ll tell you about what happens at pellet-pellet interfaces where the fuel is not fully supporting the cladding and may, indeed, be providing some stress concentrations at the cladding. So I’ll defer all that to the next presentation.

Our work is geared towards any kind of source of hoop stress, which would be perpendicular to radial hydrides and cause the most damage and the lowest ductility values.

So, let’s look at a very crude schematic of fuel and cladding. You will notice the cracked fuel, the internal pressure, PCI – stands for Pellet Cladding Interaction Flaw. None of our samples that we had from fuel that was irradiated in commercial reactors had the PCI flaw, so that’s not part of our test matrix. It’s just not in our samples. That would cause a stress concentration.

Radial hydride is shown here just symbolically. That’s what we study.

So what do we have in our experiments? We have cladding without fuel in it, and I’ll explain that in a second. We don’t have the PCI Flaw just because it’s not present. And we have various conditions that produce radial hydrides. This is called the pinch-type loading which would occur at the grid spacer springs. Think this is a spring providing a resistance force. This is not literally a force, it’s an acceleration or displacement pushing the material on that.

So ring compression tests are tests that we run with applied loads at the top, support at the bottom.

The basic philosophy of modeling, since I’ve been in it for many, many, many years is that you try to learn as much as you can about the cladding, you characterize the cladding to your full confidence level, and then you use modeling to analyze this situation with the fuel in it, your uncertainties are new cladding bonding and how compressive is the fuel. Throw your uncertainties into the fuel. You run a few confirmatory tests to see if your modeling is any good. So there is a merit in studying defueled cladding because it is much less expensive to do so. You can run many, many more tests. You can define a range of interests for running these confirmatory tests. So I hope that’s clear that there is a value to doing that.

Our previous speaker talked about solubility, precipitation, let me do the same thing in two slides if I can. This material happens to be ZIRLO. It’s got 320 weight parts per million of hydrogen average across this cross section. If you take this oxide layer, which is the coolant side, the hydride rim, take that away, your middle two-thirds of the cladding have about 140 ppm on the average of hydrogen.

If I go to M5 with a lower hydrogen content, this example has about 76 weight part per million. And you can see up here that these are all – this is circumferential direction. Circumferential direction of the hydrides are primarily circumferential. You'll get hints of radial hydrides in this material at the low hydrogen content.

So let's, for an example, just take the material up to 400 degrees C and talk about what happens. I'll give you round numbers. We're looking at this picture and then a graph with precise numbers in the next slide.

This is the easier case with 76 weight part per million hydrogen. As I heat and I hit 300 degrees C, all of the hydrogen is in solution. So going from 300 to 400 doesn't change anything to do with the hydrides. They're all in solution. What's happening going from 300 to 400 degrees C is I may soften the material a little bit due to annealing. It may actually be beneficial. The internal gas pressure will be rising. But as far as hydrides, they're all in a precipitated state. And actually coming down cooling is something called a hysteresis or a temperature gap. You need a super saturation of hydrogen in order to precipitate hydrogen.

So coming down and cooling, if this all went to the solution at 300 degrees C, I would not get any precipitation until I got down to about 235 degrees C. It’s hard to precipitate hydrides. They have to push material out of the way to form.

So simple case, this went from this type of picture, and this is after cooling from 400 degrees C and 110 megapascals, the hydrides are primarily radial and long in this material. But as we’ll find out, they’re not as damaging as they look because of the low hydrogen content. And in particular what you can’t see is going into the board, how continuous are these hydrides going into the board? And the lower the hydrogen content, the less continuous they are. And they’re less damaging.

Let’s go to the top picture. If there’s 140 ppm within here, and your solubility limit is 200, approximately 200 weight parts per million, essentially all this hydrogen would be dissolved by the time you got to 400 degrees C, and you’d get an extra 60 weight bar per million from the hydride rim . You’d still be left with some of this rim.

If I now cool, the material does remember – memory effect – where the hydrides were that pushed the material out. And you’ll notice that after cooling you’ll still get some hydrides in that same location. But cooling under stress, you can have a competition between the hydrogen returning to its initial position and forming radial hydrides.

Let’s look at it with this curve, and then maybe we’ll be on the same page.

All right, I can’t see it. Oh there – it’s there.

Okay. If we take the simple case of the M5 with low hydrogen content, I am going to heat along this blue curve, yeah, these are two different investigators, Kammenzind et al. and Kearns, and their solubility curves, their dissolution curves are very similar. We’ll go along the blue curve, and at 300 degrees C you’re all in the solution. It doesn’t matter what happens if I go above. And then I have to come down to about 235 before I start precipitating. So I start precipitating hydrides right here, and as I cool

I continue to precipitate the rest of them. That’s the simple case because everything went into solution.

The more complicated case is I go up to 200 degrees – 400 degrees C with 350 weight part per million of hydrogen total. Two hundred goes into solution. Literally 210. I said I was using round numbers. And nothing happens during cooling until I cool about 65 degrees C and I start precipitating new hydrides. It’s a little more complicated than that, but let’s save that for the question-and-answer period. When you have an excess of hydrogen, the situation is more complicated than I’ve presented it.

So that’s the background to try to understand this material. Let me give you a perspective now, and let me give you the same perspective at the end. The perspective is DBTT is not a cladding material property in the sense that if under one set of conditions if we determine the DBTT is 100 degrees C, that is not meant to be applied to that material under all conditions all along the whole length of the fuel rod. It depends on the amount and orientation of the hydrides, and it depends on the orientation of the loads on the cladding. It’s not like Young’s modulus, it’s not like heat capacity.

It’s highly dependent on how you precondition the cladding and it’s very directional. The radial hydrides are in brittle material – can embrittle material – in response to the hoop stress loading, not axial loading or bending.

Effects of radial hydrides obviously depend on the extent of radial hydride precipitation. We used effective length as well as number density of hydrides. It may reduce hoop failure stresses and strains. It may complicate the structural analysis if you’re now dealing with a brittle material that you’re trying to analyze.

As far as transporting fuel assemblies at temperatures less than DBTT, it certainly does not invite failure of the cladding. That depends on what your loads are. At temperatures less than DBTT, the cladding is still load bearing. It may not have ductility, but it’s load bearing. Or it may have reduced ductility. And really what you need is an integrated effort of modeling and data to determine loads on fuel rods, calculate cladding stresses and strains, and compare calculated values to stress and strain failure limits. And we’re hoping that even with radial hydrides, your limits may be been up here, you may have reduced them down to here, and your loads may be down there.

So just because you see a radial hydride, don't over-react and think, Oh, my God, that means the thing is going to fail.

So, let's summarize previous results. I'll make recommendations for additional tests. But, again, this is cooling from 400 degrees C, which is more of a licensing issue and more of the fact that vendors go to NRC to apply for licenses with upper bound temperature limits. So any recommendations I'm going to make for additional testing here after I show you the data, I'm not making those recommendations to DoE, where we're interested in what are the actual temperatures, which are much lower than this, and actual stresses.

So let's – and the structure of this is one word and a lot of number slides. I'm going to skip them. They're for your reading pleasure. It's what I'm supposed to be saying when I show you one plot and one set of photographs. So that's for your reading pleasure and in case I misstate anything, go back and use that.

Let’s look at what we generated for M5, and you actually saw this in the previous presentation. And let’s start with the as-irradiated material, which we’re now going to squeeze and induce ovality and hoop bending stresses in the material.

And with the M5 material, the as-irradiated data points, which are these triangles. The material has relatively high ductility at room temperature there’s absolutely no cracking as we go through the full displacement in our test, which is 1.7 millimeters of displacement of a cladding ring.

If I go up to 90 megapascals and 400 degrees C peak simulated drying temperature and stress and then cool from that, we do see radial hydrides, but as I said, they have a benign effect on the ductility and you really can’t tell the difference between that sample and the as-irradiated sample, and that would be these two circles, that circle and that circle.

It’s not until you get up to about – use round numbers – 110 megapascals at 400 degrees C and you’ve cooled from that, and the material is clearly ductile at 90 degrees C, and it is brittle at room temperature. We don’t have the 60 degrees for that.

If I go into higher stress, and, again, both these cases I’m seeing all radial hydrides. I’m not seeing any circumferential hydrides after cooling. We’ll see that in the next picture.

Go to about 140 megapascals, it doesn’t make much of a difference. All the hydrogen is radial.

However, limitations of the data base are as follows. Let’s go from high to low. This was our highest hydrogen content at 94 weight part per million. This was lower at 72. And then this was only about 60. So as we decrease the stress we get improvement in material, but there was also a decrease in hydrogen. Coincidentally. Not planned.

So in using this database you have reasonable confidence – you don’t have a lot of data points – reasonable confidence that at 90 degrees C you’re going to be ductile. As to exactly where this transition is, you’re going to need more tests in that range to determine that.

So let’s just look at pictures of what we’re talking about. With 90 megapascals peak stress at 400 degrees C, and I have to remind you that by the time hydrides precipitate, my stress pressure is decreasing with temperature as I cool, in our experiments, which we hope simulate what a fuel rod would be. Really the 90 drops down to 65 and 210 degrees C where you would start to precipitate hydrides, and you get close to 40% - hydrides covering about 40% of the wall. But you will notice some circumferential and some radial at this low stress. There's also a low hydrogen content.

So let's move up to what happens at about 110 megapascals and a little more hydrogen in the sample. The 110 comes down to 82, at which hydrides would start to precipitate, so really, if you compare our results with anybody else's results, all those other results are generated at constant pressure during cooling. So you really should be comparing the lower number to anybody else's data.

The effective length of radial hydrides increased about 54% of the cladding wall thickness. And this is an example of one. And exaggerated you do see little tails of circumferential hydrides, but it's primary radial.

And then in the highest hydrogen content case, and the highest stress case, which is about 140 megapascals, they're all in the radial direction.

Yet the material is ductile at 90 degrees C. And there’s a transition temperature somewhere around 80 degrees C. And that’s due primarily to the fact that what you see in this picture, if I move into the bore a half a millimeter, take another slice, you’ll see a different picture, meaning those hydrides are not continuous in the axial direction. That’s beneficial as far as the mechanical performance.

Okay. So, what’s missing from that data set, and, again, this is not a recommendation to DoE. It’s NRC and the utility vendors. We really need data between 90 and 110 megapascals with hydrogen contents that are 75 to 95. We notice that ductility improved with decreases in hoop stress, but also the hydrogen was decreasing at the same time. So we need to determine DBTT versus hoop stress at higher hydrogen levels.

To get a robust data set, you really need to repeat tests. If you can narrow in on 70 to 80 degrees C as a critical regime, if you get – we only have three data points for this particular set of tests and one is ductile. Some scatter is expected, so you recommend two more rodlets cool from 400 degrees C to 100 megapascals. That would give you eight additional data points from those two rodlets.

But, what you really need for DoE is we need tests at less than 400 degrees C. It’s unlikely peak cladding temperature will ever reach 400 degrees C. The disadvantage is less annealing would occur at the lower temperatures. The matrix material might be harder, have less ductility. So you need to confirm previous results at temperatures less than 350 degrees C.

The number of tests will depend on the first set of results that you generate.

So that’s the situation with M5. The hydrides look scary, but they’re not as damaging.

Okay. We did a lot more testing with ZIRLO. Well, let’s go to Zirc-4. We’ll save ZIRLO to last. Again, this is the test matrix. These are the results in words. Let’s go to the graphs.

We found that Zircaloy-4 is much less susceptible to precipitating radial hydrides than the other two materials I talked about, the ZIRLO and the M5. And so even at very high stress, which is 145 megapascals, beyond what our imagination can possibly go to when we look at what standard rods are – and I should have mentioned that my upper bound estimate for standard fuel rods is about 80 megapascals of stress in those rods. And that’s an upper bound. So sort of for standard rods focus, and that’s Zirc-4 and M5, you’ve got to focus on less than 80 megapascals of stress.

But we ran our first set of tests for NRC at a very high stress, about 145 megapascals. And you don’t see the sharp transition. And the reason you don’t see it is your radial hydride length is only about 16% of the cladding wall for this case up here, for this whole set here, and we don’t have data at this point, but you would cross at about 60 degrees C. It’s clearly, well, low ductility at 90.

If I drop the stress level, and unfortunately the hydrogen content also dropped, I get better performance and then I get ductility all the way down to room temperature.

Everything was fine, and if you test the as-irradiated material with low hydrogen content, like 300 ppm of hydrogen, that sails right through our test and has no cracking at all. And that’s the full ductility that that can attain within the context of our test.

Then we ran into a problem in that the as-irradiated material, and we tested it at strain rates over a factor of 1,000, and so no effective strain rate.

We did a lot of tests at room temperature. One test at 90 degrees C. On the average, this material falls below our ductility criterion. And that’s primarily because they have high hydrogen content of 640 weight part per million. Locally the hydrogen is as high as 850 going around the circumference of the cladding.

We weren’t – I wasn’t satisfied with these results. They caused all kinds of problems with NRC. So we wanted to look more closely at what was going on because this is just circumferential hydrides. And as I’ll show in the next couple of pictures, we’ll just focus on the as-irradiated data.

Cracking along circumferential hydrides is not as serious as cracking along radial hydrides in the following sense.

All right, for this data set, it includes the old data plus the dark triangles, which are the new data, for three of these tests we stopped them as soon as we got a load drop between 25 and 30 degrees C – 30% - to find out how much cracking did we have. And when we did that, we could measure directly what the change in diameter is from what we started with. I’m calling that permanent strain. And the criterion for permanent strain is if you’re above one percent, you’re above the noise of the measuring techniques.

So what happened when we did that, reanalyzed all the data, the old data points, which are these open symbols, and the new data points, which are the solid ones, are all considered to be ductile if you will play this game with me. Because I might want to call it pseudo-ductility. What seems to happen when you have the radial hydrides is you get cracking through the hydride rim. You expect that. And you get cracking along – it’s better shown over here. You get cracking along circumferential hydrides ahead of the crack front. And then you get ductile, 45 degree angle cracking through the matrix of the material.

So what’s really happening is it’s not an instantaneous drop of 30%, it’s a stair-step. You crack along circumferential hydrides. You get ductile cracking through the material. So, in my opinion, the material has the ability to deform permanently, permanent strain, without going beyond 50% of the wall cracking. It’s load bearing. And it should not be considered as brittle. That is my personal opinion. And we got that based on load interrupt tests where we would stop the test, withdraw the load after a small quantity, then you get a good image of how much cracking occurred.

So this has got the percent wall cracking, and the load drop it was stopped at, and what the ductility is, just simply by measuring the diameter before the test, the diameter after the test, and normalizing it to the outer diameter.

So that’s new data.

ZIRLO is what we did the most testing on, as you can see by the large number of samples here. I’ll explain that in a minute.

You saw most of this curve last time. Let’s start with the as-irradiated material. Moderate to high ductility of that material, even though it’s got a thick hydride rim, what’s below the rim is ductile. And you can deform quite a bit before you get cracking.

If you go to 80 megapascals, which I’m saying for a standard fuel rod, I mean just fuel inside, and cladding, that’s as high as I think you would ever get – five minutes, okay – in terms of stress, that material behaves very well. Ninety megapascals is pretty good because you still have ductility at room temperature. And then there’s this big jump as you go from 90 megapascals to about 110 megapascals, and your DBTT, if you want to talk about it, is increasing by 100 degrees C.

So this area should be explored to find out where the transition is. What I do want to say before I go on with this, this is the same cladding material that’s used in what’s called the IFBA rods where you have an additional source of gas, which is helium, generated from the boron-10/neutron reaction. So stresses might very well be in the range of 80 to 110 megapascals. So I consider this whole range to be relevant to that special design of fuel rod.

Okay, I going to go quickly through this but, again, you can see why this material behaves so well following cooling from 80 megapascals. You just have short radial hydrides that are about ten percent of the cladding wall. If I increase to 90 megapascals, my hydrides are about 20% in length across the cladding wall. And if I increase to 110, approximately, they’re on the average 33% with a maximum of about 50%. And then, of course, if I go way beyond, I get much longer radial hydrides.

Okay. I’m going to skip that. I have very little time left, so new data from 350 degrees C peak drying and storage temperatures.

Expectations are very important because that’s what shows you you’re not as smart as you think you are.

If we drop to 350 degrees C from 400 degrees C, there’s 80 ppm of hydrogen – less hydrogen in solution available for precipitation. So we expected a benefit. Also there will be a decrease in internal pressure and hoop stress at the precipitation temperature, which is now going to be lower. Possible decrease of annealing of irradiation hardening. Prediction that we would get a benefit from dropping to 350.

M5 we expected no change because all the hydrogen was already dissolved at 300 degrees C.

All right. So we ran tests at 94 megapascals, and 93 megapascals with fairly high hydrogen content. The surprise was, as shown in this picture, this is the data that you’ve already seen. We expected the results to be like this. And the results are in red. We jumped 100 degrees – at least 100 degrees C. And actually we saw less ductility at above the DBTT, and with the red curve, than we did at a higher stress level at 400 degrees C.

So, one of two things. Either the hydrogen content being so high degraded the material to begin with, or we didn’t get the annealing at 350 degrees C that we got at 400. So we have a test in progress to try to sort that out. So things kind of got – maybe got worse at 350.

Just to show you the kind of hydrides we generated. They were long. About 50% of the cladding wall, averaged about 37 for this test. About 30% for that test. It just shows you what they look like.

So what’s in progress? We want to repeat that test I just showed you with lower hydrogen content more typical of what you would expect of ZIRLO after two to three cycles, 18-month cycles.

The M5 test we’ve already completed the cooling from 350 degrees C. We had a target hoop stress about 92. We had a target hydrogen content in this range. And what we achieved was about 87 megapascals, which is close to that. Hydrogen content was 80, which is good. And the ring compression tests are in progress. But I do want to show you the (inaudible) from that. This is sort of typically what we see, which is a radial hydride fraction across the wall of about 40%. More than half of the circumference looked like that. But you’ll notice one location where the radial hydride goes through 95% of the cladding wall. How damaging is it depends on how continuous this is in the radial direction. And that is a good place to stop that except how will this behave in the ring compression tests. It depends on exactly what I just said. Does that radial hydride go all the way through the sample?

Okay. Quickly on perspective. DBTT or Ductility Transition Temperature, more literally what I've been talking about. It's not a materials property like Young's modulus. It depends on orientation/length of hydrides and orientation of loads. You can transport at below DBTT. It does not imply failure. You just have to justify it. And the data may be used to determine hoop failure stresses and strains, which can go into modeling codes.

Last point is what are relevant hoop stresses during drying and storage? That's going to be a subject of the May meeting, and it's very detailed, so I'm just giving you my opinion. We're staying with standard rods which have lower gas pressure, just the helium you put in them plus the fission gas that's released. The IFBA rods, because Integral Fuel Burnable Absorber is a mouthful, have higher gas pressures because they have the helium fill, maybe a little bit less than that. They have the helium from the boron-10/neutron reaction and the fission gas.

It also depends on what your average gas temperature is during storage. And that will change depending on whether you have a horizontal cask with helium with no natural circulation. Most of the gas will be up towards the top, plenum region. You have higher gas temperatures with a vertical cask with helium in it because you'd have the natural circulation. And there is one cask design where you high heat load and you flow helium, that's convective helium, so the highest temperatures would be up near the plenum region.

So these show you the different kinds of cask design. This would have the lowest gas temperature. This would have the highest temperature.

So my estimates – and these are upper bounds. I'm confident they're upper bounds. For standard PWR rods, your upper bounds are between 60 and 80 megapascals, depending on which cask you're in. ZIRLO clad with IFBA rods that have annular blanket pellets that adds to your gas volume, 80 to 120 megapascals. And then if you don't make your blanket pellets annular, if they're solid, you could get up in this range, although 150 seems a little high.

So any of the data I showed you from ZIRLO between 90 and 110 megapascals I feel is relevant and that's to be determined in May.

Thank you very much for your attention.

Thank you. Questions? Gerry?

Frankel, Board. So great stuff. It’s interesting that sometimes more data just raises more questions.

Yes.

But I’m trying to understand the difference in the alloys. So, you know, it’s been presented that the new alloys corrode less so there’s less hydrogen.

Correct.

But that’s not everything, right? So if you had the same amount of hydrogen, and the same thermal treatments, the same stress for different alloys, they might precipitate hydrides differently.

They will. In particular the M5 that I showed you is recrystallized annealed and has more of a tendency to precipitate hydrogen in a radial direction.

Right. So is – so it’s not the alloy, you know, it’s not the hydrogen content, but is it the hydride orientation and distribution, is that everything? So if you had different allows with the same, if you could reproduce the same hydride distribution, would it behave the same? Or is there still a difference because of the alloys?

There’s a fundamental difference between both Zircaloy-2, which I didn’t talk about, which is used in the recrystallized annealed condition, and the other two alloys, Zircaloy-4 and ZIRLO, which are cold work stress relieved, and tend to precipitate more hydrogen in the circumferential direction.

I’m saying if you had the same distribution, okay.

Oh. Oh.

It’s different alloys with the same hydrides. Is it the hydrides that are controlling everything or are there other aspects of the alloy that are important in the behavior?

The hydrides control most of it. There is a difference in the matrix metal.

Right.

The ductility of the matrix metal.

It’s primarily the hydrides, then? So the whole thing is about knowing or controlling the hydride distribution, right, and understanding that?

Right.

Okay. In different alloys it’s happening differently because of different microstructures? And so –

Different microstructures.

What is – so it’s the grain size and texture? Or what is it –

The texture.

It’s the texture.

Mainly.

So if you had the same alloy with different textures, you would get different hydride distributions for the same hydrogen content and the same stress and the same thermal –

No offense, but that’s more of academic interest because the alloy that’s got the most tendency to precipitate radial hydrides is the M5 recrystallized annealed. It only picks up about 80 to 100 ppm. So I’m not in the laboratory forcing all these alloys to be at 350 –

Okay, so I’m an academic, you’re right. But, you know, I think the point being if you understand –

I understand.

If you understand fundamentally what’s happening –

Yes.

Then you can control it?

Yes.

Sometimes. Right? So, I mean, then you could maybe control the microstructure of the rods going in.

Yes. But the microstructure and everything about the cladding is optimized for in-reactor performance. Storage is an afterthought. You take what you get. So no fuel rod is optimized for storage. If it were you might put a bigger plenum, gas plenum, reduce the pressures more, and never even see a radial hydride.

Okay, so you don’t care about it.

I care. I’m just saying that the designers, if I can read into the minds of the designers, the venders, the fuel vendors who are designing their fuel rods to give optimal performance in reactor and to survive design basis accidents, that in terms of economics, that’s as far as I can see them thinking. And then as far as storage, arguing that conditions are benign and there is no problem. So, yeah, you could – if you took the whole fuel cycle into consideration, you might come up with slightly different designs.

Thank you.

Just a follow-up question. Ewing, Board. On this issue of texture, what surprises me is that in your presentation and others, there’s no transmission electron microscopy. That is, looking at the atomic scale at the nucleation and crystal growth of the hydrides.

Am I allowed to say we’re not funded to do that?

Right. But –

No. Actually even scanning electron microscopy would be interesting – as far as the fracture of surfaces are concerned.

Right. Right.

And there is more work you could do. My opinion is if the budget is limited, we want to do more scoping out of ranges in which this may be an issue. Ranges in which it’s not an issue. And then where are we with real fuel rods?

But, of course, finally your scoping work won’t necessarily cover the actual conditions. And there will be some projection into time and temperature space that maybe you hadn’t anticipated.

Correct. And we’re aware we’re relying on the fact that cooling at five degrees C for an hour is slow enough.

Right.

Given more funding we’d cool at a tenth of that and make sure it made no difference.

And I’ll end by saying that the TEM work, the temperatures that you’re dealing with are relatively low, so there are heating stages that could easily go to 500 degrees C.

Yes.

And you could watch the nucleation.

We have that capability at Argonne.

Yeah. I know.

Yes. Yes, you know.

All right. Sue, you had a question?

Brantley, Board. I think you said in the beginning that you don’t have any PCIs in your experimental cladding. Can you –

We don’t have fuel. I’m sorry, we don’t have the cracks.

So can you tell me more about what a PCI is and what that means in terms of your – what it would mean, perhaps, for your experiments. And then the second thing –

It’s been a –

The second thing is you don’t have fuel inside.

Right.

Would that impact your results, the fact that there’s no fuel inside?

You would have to push mush harder to get the same displacement. Your load would have to be significantly larger.

There’s no effect of the hydrogen diffusing across the cladding by having the UO2 inside it or whatever?

No. The Zircaloy has a very high affinity for the hydrogen, so you’re not going to be transferring hydrogen from Zircaloy-4 to the UO2 fuel.

So the fuel is just a mechanical – it would just change the mechanical –

In my opinion, yes.

Yeah. Okay, so then can you tell me what a PCI is and just –

It’s a crack – the idea of the cladding, it’s been observed in some low-burnup fuels, some older alloys. It’s caused by a combination of localized stress and chemical environment, like the presence of iodine or something else where you crack a little, and corrode a little, and crack a little, and corrode a little.

So it’s a crack between the pellet and the cladding?

It’s a crack in the cladding.

Oh, and is it radial or –

It’s radial.

Okay. So what does it have to do with the pellet, then, because you call it a PCI –

I’m saying that all the samples we tested did not have that. If that existed, that would be a source of stress concentration. It would be a source of precipitating more radial hydrides at the higher stress along there. And it may affect the ductility of the material when you squeeze it.

So, okay, so this is what I think I'm understanding. A PCI, right, is a defect that has been observed in certain fuel rods?

Yep.

And it just occurs sometimes, we don't know why. It's a radial crack across the cladding.

Just a small – a small crack. Not all the way across the cladding.

That starts at the pellet/cladding interface.

Yes. And somebody from Industry could better answer it. I didn't think that was a current major concern. But someone from Industry, from utilities, fuel vendors, could answer better, what is the probability of having that. Our cladding is all from commercial reactor fuel rods irradiated to burnups higher than the burnup level. And we have not observed that in any of the cladding that we have received. It has been observed earlier, particularly when starting out, ramp rates weren't controlled as well. And you cracked the cladding and you pushed it against the fuel during power changes, and you could crack the cladding, not all the way across, but you could initiate a crack.

And so all this work on radial hydrides, is anybody working on PCIs and the effect of PCIs on, you know, the properties of the cladding or the storage capability?

People have worked on what’s called delayed hydride cracking, which means that you have a crack, you have hydrogen migrating to the tip of the crack. Most of that work is done in non-irradiated material. And the findings seem to be, for that effect to be significant, to give you a crack all the way across the cladding you would need a load that’s large enough, and you need a crack that’s long enough. But the real question, now that you ask it, in my mind, so I go back and talk to the Industry people here during the break, is are PCIs – are those cracks, those stress-induced and corrosion-induced cracks, are they prevalent at all in modern cladding? And I have a feeling they’re not, but I’d rather hear it from an expert.

Well, so the fact that there’s so much work on radial hydrides tells me that DoE thinks that these are more important and more worrisome than PCIs. Can you convince me of that?

Um, let me rephrase the first part of your point. Two-and-a-half years ago when I addressed you, we thought gas pressures were much higher in high-burnup rods, and we thought radial hydride embrittlement was (audio break) a key issue to deal with. As we've moved on over the past two years, we've found out from Brady Hanson's (audio break) talk that fuel isn't being taken to as high a burnup as we thought. It's not taken to 62 gigawatt base per metric ton. During storage, cladding temperatures are much less than we thought they were based on vendor calculations. And now we're finding out the gas pressures are much (audio break of several seconds) additional source of helium. And if you're in a cask where your gas plenum is up here and most of the gas is here, and you're flowing helium, which is getting hotter and hotter, under those conditions, radial hydride embrittlement may – may be an issue. So I'm saying it's less of an issue now than it was two years ago. Not that materials – materials can develop them. It's just if your temperatures are too low – below 300 degrees C, for example – that you're not dissolving enough hydrogen. If your stresses, your hoop stresses due to gas pressure are low such that your stresses are 80 megapascals or less, you're not – you may see radial hydrides, like five percent of the wall, but they're not going to degrade the material. So that's what we're trying to converge to is not just do the research because the pictures looks sexy and they are interesting. We're really trying very hard to find out what is realistic. And what we keep finding is temperatures are lower than they were in our minds when we started this program. And the big shock was that gas pressures are much lower, that fission gas – for a standard rod, fission gas contributes very little compared to the helium you initially put into the rod to slow down the creep of the rod due to the system pressure of the coolant and improve the thermal connectivity. And that that seems to dominate. And what happens with burnup is your gas volume decreases. So your pressure does increase, but it doesn't increase anywhere near what we thought.

So –

So I'm kind of talking around your issue, but I don't think the PCI cracks are prevalent in modern cladding. And I'll try to confirm that before the day is over. And people that have studied that, they use a different buzzword. They do delayed hydro cracking, which is a combination of crack growth. Hydrogen migrating. That’s stress concentration. More growth. More stress concentration. But I’m not convinced that that’s a relevant mechanism for deteriorating the material.

So, Mike, we’re going to have to move on in to the public comment section.

Okay.

But you’ll be here all day. Is that correct?

Pardon?

You’ll be here during the day so –

Yes. Yes.

I’d invite others –

Especially if other people use my name in vain, so definitely.

Okay, great. Thank you very much.

Thank you.

Now we’re entering the public comment portion of the program, and we have three. The first is Ralph Hutchinson of the Oak Ridge Environmental Peace Alliance. If you’ll come to the microphone, please? And if you could keep your comments to five to seven minutes, something like that.

I’ll give it a shot.

Okay.

Thank you very much.

Thanks to the Board and the staff for being here. Welcome to Knoxville. We’re glad you’re here. My name is Ralph Hutchinson. I’m the Coordinator for the Oak Ridge Environmental Peace Alliance. We’re a grass roots group with about 2,000 members located here in East Tennessee. And for the past decade, most of our work has focused on nuclear weapons issues, so I’m not terribly well versed on the nuclear power, nuclear waste side of the ledger that you’re dealing with all the time. But since learning in November of the proposal to bring North Anna fuel to Oak Ridge and do research on it here I’ve been on a learning curve. And this morning I feel like I stepped on an elevator.

So thank you for coming and having this meeting in East Tennessee because it’s helpful to be educated by people who know what they’re talking about.

I remember in the early 1990s I read an article, I think it was in the Oak Ridge National Lab Review, and what I remember about this was in the margin of the article there was a pull quote from longtime lab director Alvin Weinberg. And he was reflecting on the first 50 years of nuclear power. And in his pull quote he said – this is not an exact quote, but the gist of it was, if I had known that 50 years later we would still have not have solved the question of what to do with spent fuel, I might have had a different view of nuclear power.

One of the benefits of the clarity-enhancing nature of hindsight is didactic – the past has things to teach us – and our lack of understanding of the nature and characteristics of high-burnup fuel, that might have been avoided had we thought more carefully about the lessons of the past, maybe through Alvin Weinberg’s lens.

As a lay person who is not focused on the issues of nuclear power or waste, I was perplexed to discover the decision was taken to modify the practice of fueling nuclear reactors to leave fuel rods in for longer periods to create a new, different kind of waste product, high-burnup fuel, and that this practice was allowed to be implemented across the nuclear power industry before the nature and characteristics of the fuel were fully understood. I think maybe a good way to think about this is the invisible horse problem. Because now here we are, standing on a cart piled with way more than a decade’s worth of high-burnup fuel, tons and tons and tons of it, and from atop our high vantage point we scan the horizon behind us but sadly we cannot see the horse that should be pulling this cart. That’s how far ahead of it we are. Decades after NRC gave the go-ahead for high-burnup fuel without knowing what the consequences would be, we’re still trying to resolve some very fundamental questions about the fuel. Questions about how to store it or transport it safely.

Is this a bad thing? Not for researchers. This is a career for some researchers. And not necessarily for power companies. It can wring a few more megawatts out of the fuel. Not even for bureaucrats whose careers will be history long before we have exhausted the hazards of high-burnup fuel.

On the other hand, those of us who live in communities through which high-burnup fuel is secretly transported, here in Knoxville, for instance, along Interstate 81 stretching back toward Charlottesville, pretty much in winter weather, and in which the experiments are conducted, next door in Oak Ridge, for instance, this fuel, which is twice as radioactive, dangerously unpredictable and unstable, poses significant threats. How significant the threats are we don’t know. And that’s a problem.

And it matters to those of us who are taxpayers now underwriting research that will serve the nuclear power industry. They get the profits, we get the costs and the risks.

And of course it’s not all that great of a thing for the environment either. To the air, the soil and the water, high-burnup fuel is a more deadly poison than low-burnup fuel, and that was bad enough. It has to be isolated from the environment for centuries in safe configurations in containers that we cannot yet contemplate, let alone design and build. What Dr. Weinberg recognized 25 years ago is even more true today. So in that context, I want to briefly mention two issues about high-burnup fuel decisions. And I will say at the outset, they’re nontechnical, but that doesn’t mean they are outside your purview if, in fulfilling your mandate, you want to be effective.

The first has to do with transparency. The arrival of high-burnup fuel in Oak Ridge sometime last month, I learned about last Sunday night from a report in the newspaper. This despite my request in November for information from the Nuclear Regulatory Commission. They never delivered what they promised me. And despite meeting with top officials at Oak Ridge National Lab, who I thought would have at least made a minimal effort to keep us informed. Instead, I read in the paper about a done deal.

Now I know the Review Board is not NRC, and you’re not ORNL or DoE, and I appreciate that you’re here having a public meeting in Knoxville. My point is that everyone who is wrestling with these issues surrounding nuclear waste should recognize that even if it’s painful in the short term, maximum transparency and thoroughgoing communication is a better policy than low-key, “let them find out about it in the papers” decisions. It’s true for the projects and the proposals that you’re reviewing as well. Communication with the public should be part of each one of the proposals.

And my second point is related, and it has to do with consent. Twenty-five high-burnup spent fuel rods have been brought into my community. Ten of them will stay here for almost a decade. And I learned about it in the newspaper, despite inquiries and requests for information. The rest of the fuel rods, and the waste generated during the research here will be foisted on some other community in Washington State or in Nevada. Oak Ridge National Lab was selected for this work, of course, only because Idaho refused to consent to having the sister rods delivered there. The places the waste products will go are not being consulted as these decisions are made on their behalf, and this is about as far away from a consent-based decision-making process as you can imagine short of a shotgun wedding.

So we’re told it’s just a little bit of material, relatively speaking, that’s the assurance I got from folks at the Lab.

And we won’t take anything without knowing in advance the path out. But as it turns out, more than a third of the material they’re taking isn’t going out, at least not for a decade. And just a little bit, when we’re talking about spent high-burnup nuclear fuel, is enough to do enormous damage, not to mention opening the door to what might follow. All of which is to say we who live here have a right to know and have a right to have a say what happens in our community.

You on the review panel, and across government, and academia, and industry, everyone responsible for making and signing off on these decisions has a responsibility to communicate and to earn our consent.

Thank you.

Okay. Thank you. The next speaker is Kevin Kamps of Beyond Nuclear.

Chairman Ewing and Board Members, thank you for this opportunity to make comments.

Please identify yourself, Kevin.

Yes, sorry. My name is Kevin Kamps, and I serve as Radioactive Waste Specialist at Beyond Nuclear. It’s always hard to follow Ralph Hutchison.

So I have a few questions and then some comments. And I’d just like to say thank you again to the Board members, Dr. Frankel and others, who have asked some tough questions today of the presenters. Very much appreciated.

So my first question is, it’s really directed at Mike Billone of Argonne, if I pronounced your last name correctly. Are you aware of the EPRI data that found a two-year-old Diablo Canyon canister had a temperature low enough for moisture to dissolve the magnesium chloride salts on the canister? This document has the data and references. EPRI has not produced the final report on Diablo Canyon, but the information on this paper references EPRI sources of information. This document was reviewed by Dr. Hira Ahluwalia and Dr. Digby MacDonald, Materials and Corrosion Engineers. And this document, Diablo Canyon Conditions for Stress, Corrosion, Cracking in Two Years by Donna Gilmore of San Onofre Safety is posted at her website. And I can provide that reference.

My second question is, are you aware that the Koeberg Nuclear Power Plant, in Germany, I believe, had a similar component, a wastewater tank, leak from through-wall cracks caused by the marine environment in only 17 years? The deepest crack was 0.61 inches, which is thicker than most U.S. canisters, just one-half inch. NRC Material Engineer Darrell Dunn presented this information at NRC technical meetings. San Onofre is located in a similar environment as Koeberg, with onshore winds, frequent fog, and high surf. And again I can provide references that are posted at San Onofre Safety and the NRC Atom System about these documents.

My third question is, due to high-burnup fuel hydride issues, under what conditions could an explosion occur if high-burnup fuel is exposed to air from a through-wall crack in a canister?

And my thanks to Donna Gilmore of San Onofre Safety for these questions. I'm sorry, Koeberg is in South Africa, not Germany. A German name, perhaps, or Dutch.

And a few comments as well. Just some context. As Ralph mentioned before, low-burnup irradiated nuclear fuel is dangerous enough. And high-burnup irradiated nuclear fuel is yet more dangerous. So the context that we meet in today is one of the chronic risks from these materials and the acute risks. So in terms of chronic risks. The EPA under court order was forced in 2004 to go back to the drawing board on the Yucca Mountain safety regulations. And four years later, in 2008, came back with a figure of one million years of hazard associated with irradiated nuclear fuel. And I would point out that certain radioactive poisons like Iodine-129, with a 15.7 million year halflife, far exceed even that one million years of hazard.

And along the lines of chronic risks, in DoE’s 2002 Yucca Mountain Final Environmental Impact Statement, they pointed out that waste kept on site at places like the Great Lakes or the seacoasts or the riversides could suffer catastrophic releases over time if simply abandoned and the dry casks allowed to leak their contents. Of course, DoE was making that comment in the context of trying to push the Yucca Mountain dump proposal.

I would just like to point out that on February 22, the D.C. Circuit Court of Appeals will be hearing the case of New York v. NRC, round two. In 2012, New York v. NRC resulted in NRC being forced back to the drawing board on its nuclear waste confidence policy, which they now call Continued Storage of Spent Nuclear Fuel, which many of us call the Nuke Waste Con Game.

And one of our environmental coalition’s expert witnesses, and we are a party to these lawsuits, Dr. Arjun Makhijani, of Institute for Energy and Environmental Research, referred to high-burnup irradiated nuclear fuel at the time, in 2012, 2013, as a data-free exercise. And as evident today, that data set is now being built, but one has to point out that it’s very late in the game. And we have to hope that it’s not too little, too late.

So on acute risks I just wanted to point out the circumstances that we face. Efforts in Congress to what they call enact the recommendations of President Obama’s Blue Ribbon Commission on America’s Nuclear Future, specifically the highest priority, centralized interim storage, as they call it, or consolidated interim storage, which would launch an unprecedented large-scale transportation program of high-level radioactive waste, irradiated nuclear fuel transportation, through countless

American communities. And some of the phrases that we use for the potential risks involved are Mobile Chernobyl, Fukishima Freeways, Floating Fukishimas, in terms of barred shipments on waterways, and Dirty Bombs on Wheels. And this, of course, applies not only to highway shipments, but rail shipments as well, which is the preferred transportation mode.

So one person who warned about even the impact of the transportation, and I saw vibrational stresses mentioned today, is Joe Campbell, an Elder of the Prairie Island Indian Community in Minnesota, whose community is but hundreds of yards away from the Prairie Island Nuclear Power Plant and its on-site high-level radioactive waste storage. And the Prairie Island community is another party in our coalition of states and environmental groups in this New York v. NRC2 lawsuit.

And I did want to point out that not only at Koeberg, South Africa, or the nuclear plants in California, these seacoast corrosive environments, but even places like the Great Lakes are a corrosive environment. So, for example, the Palisades Nuclear Power Plant managed to have failures with service water pumps twice in two years, in 2009 and 2011, due to stress corrosion cracking caused by a relative high chlorine compound concentration in the Great Lakes water of Lake Michigan. So that’s a concern for the dry cast storage on the Great Lakes, including at Palisades and many other places.

And, of course, rivers can also be corrosive environments. The Flint River drinking water catastrophe was due in large part to the fact that the Flint River water was 19 times more corrosive than the water from the Detroit drinking water system that was previously used, which was Lake Huron water, that had been treated with anti-corrosion chemicals. An example in the nuclear context that comes to mind is Limerick, Pennsylvania, near Philadelphia. The Schuylkill River, and whether due to natural or artificial ingredients in the Schuylkill River, and also the operations of the Limerick Nuclear Power Plant itself, the dry cask storage environment there is disconcertingly corrosive as the local watchdog group, Alliance for a Clean Environment, has pointed out for many years.

So I’ll just conclude by saying that we now have to worry about not only internal degradation, as I refer to it, the high-burnup irradiated fuel degradation being spoken about today. But we also have to worry about the outer degradation of these containers, the corrosive environments in which they are stored and very well could remain for decades or centuries into the future.

Thank you.

Okay. Thank you. The next speaker is Don Safer from the Tennessee Environmental Council.

While he’s coming to the podium, are there other speakers? Okay. Great. Cheers.

I’m Don Safer. I live in Nashville, Tennessee. I’m a Board member of the Tennessee Environmental Council. And I was Board Chairman for ten years, so up until the last couple of years.

Thank you all for the opportunity to address the Board, and thank you all for your hard work on this issue. It is extremely important, as you all know. And I appreciate it, and I appreciate the opportunity to address you all.

The nuclear industry is in decline. Even just today, TVA announced that they’re going to be considering putting the Bellefonte site up for sale. And that had been licensed for Units 1, 2, 3 and 4, two AP-1000s and two older reactors. And that’s just an indication of what’s going on worldwide. With the widespread, and really rapid implementation of renewable energy and energy efficiency, worldwide nuclear is in decline and it’s likely – I mean, it’s a given that this waste is going to last longer than nuclear reactors. Especially our current generation. They’re all licensed for 40 or 60 or maybe there’s talk of 80 years. I hope that’s not true for Brown’s Ferry, which is in its – after the 40-year cycle, and I think we’re all aware of the problems that we’ve had at Brown’s Ferry.

So I’m concerned that this work be done really well because this is the future for mankind, really. And there’s likely to be not as much money – if there’s not enough money now to do the studies, my gosh! Where’s the money going to come from when the nuclear industry is basically history? And where will the commitment be from the Congress to fund that? So I really encourage you all to get this right and to be very thorough and be attuned to your mission of being neutral. So many times I attend these types of meetings and there’s so much boosterism coming from people that are supposed to be giving a critical eye, and that really concerns me as a grandfather and as a citizen.

So one specific question that also comes from Donna Gilmore. And it concerns the thin cask Holtec containers that are being used along the ocean there in California. Those are incidentally being used at Watt’s Barr as well, but we don’t have the marine environment. But she asked, due to high-burnup fuel hydride issues, under what conditions could an explosion occur if high-burnup fuel is exposed to air from a through-wall crack in the canister? And, you know, the people at Watt’s Barr, the people at Sequoia, the people at Brown’s Ferry, never signed up, really, for the fuel to be stored long term. But that looks like what’s going to be happening, and so it’s important that these canisters be as secure as possible. Thank you.

Thank you very much. Any other public comments? All right.

Well I’d like to thank all of the speakers this morning and the speakers we’ve just heard from representing various public interest groups. We have a little bit of flexibility in our schedule this afternoon, and so we’ll start just ten minutes later, we’ll start promptly at 1:30, and continue with our program.

Lunch is available – I guess this is for everyone – there’s a buffet in the hotel restaurant.

Okay, thank you very much.

[LUNCH BREAK]

All right, if you'll find your seats we'll get started. All right, this afternoon we'll open with a presentation by Bruce Bevard of Oak Ridge National Laboratory. Bruce.

Thank you. I want to start by saying a lot of what we presented this morning gives the background for my presentation. At Oak Ridge National Lab we were original approached by the NRC in 2009 to find out what goes on with spent nuclear fuel, high-burnup spent nuclear fuel, if it's stressed, as if it was dropped, if it was vibrated, and so their interest was understanding the science of what would go on at the time. Because at in 2009, there was a thought that maybe the rods were very brittle so that if they dropped they would break, the fuel pellets might come out if that occurred. There might be other issues. And so they wanted to understand the science of what went on so that later on, when applicants brought in information, they would note how to process that information. So we worked with the Nuclear Regulatory Commission and the goal was to test up to nine high-burnup samples of H.B. Robinson fuel that had seen irradiation 62 to 65 gigawatt days.

So we have done that, and initially we started out to say, how are we going to do this testing. And the scientists at Oak Ridge came up with a test method that we call the "Cyclic integrated reverse bending fatigue tester" or "CIRFT," and that's how I'll refer to it from now on. And the CIRFT is not nice in that it allows us test high-burnup fuel in segments, and we use six-inch segments, but we do not have to manipulate the fuel at all. We don't have to put gauge sections in it or shim it or put a flaw in it. We test the fuel as is. What you'll notice is here's a fuel sample right here. We use this -- they developed this grip.

Let me step back. We have to do this testing, obviously, in a hot cell. To do it in a hot cell you have to do everything by manipulators. There's no hands-on type work. So to make this so you can get it in a manipulator and you can align your fuel sample so that it's straight, you can have the fuel so it doesn't bend and interface the drips, so we put a stress riser in the wrong place or put stress risers in it. We came up with a grip system and in there is an epoxy. We actually epoxy the fuel in. We set the fuel straight. Let the epoxy set, turn it over, make the other grip.

Now this can be placed with a manipulator in the machine, and we always have the exact same lineup. It also makes it so the fuel is not touching on the grip itself because that epoxy layer and that compliant layer allows the bending to take place without interference. We use three measurement devices here. They're called LVDTs, linear variable differential transformers, and they measure the bending of the rod. So you take a push-pull motion, a linear motion, and it will turn it into a curve, curvature, and we can measure that curvature. And by using those linear motors on the end, we can change the force that we used to test to see how this would respond given different force levels.

I was going to put this up here to talk about why is this important to everybody, but I think that people this morning went into great detail explaining about the different forces the fuel may see during transportation. So our goal was to test that, and as they said this morning, there are a number of different forces on the fuel during transportation, be it rail or train. But primarily it's in one dimension, okay. So we were doing the actual bending mode.

We did the NRC test, and when we finished with that it became apparent that the DOE is interested because in our NRC had tested one fuel type. They tested H. B. Robinson. DOE is interested to see, can we take this information and use it on other fuel types, other clad types, to see do we get the same type of information. And I'll discuss the information we got here in a second. But we were able to do that. At Oak Ridge we were fortunate there had been a previous fuel testing program, and there was some residual fuel left over that was in lengths that we could cut to six-inch lengths that we needed. So we were able to do the H. B. Robinson. Instead of doing the 9 tests, we've done 23 tests.

We had some Limerick fuel, and Limerick is a BWR, or boiling water reactor, and their clad type is zirconium-2, ZIRC-2 and it's zirconium-lined, and that's important because the zirconium liner wants to absorb the hydrogen more than the zirconium-2 does. So you would say it's kind of a hydrogen getter a little bit, so the hydrogen content in the clad lower. Also, the boiling water reactor fuel design is slightly different. It's bigger fuel pellets. The gap between the fuel pellet and the cladding is larger when they manufacture it. So the interest was to see does the boiling water reactor fuel that's high-burnup -- and this Limerick fuel was high-burnup -- give us the same response to vibration that the pressurized water reactor does.

We also had some M5 clad from North Anna had high-burnup, and because we had done testing on the mixed oxide plutonium disposition program for the Department of Energy several years ago, we had some MOX fuel left over, and that was not high-burnup fuel. That was 40 to 49 gigawatt day burnup. So one rod was technically high-burnup, the others were just under what the arbitrary rule of high-burnup. But it's in the right range. But we had that fuel there as fueled opportunity and we had a fair amount of it, so we've done testing on that, and we've done 15 tests on the Limerick and 19 tests on the M5. One of the things that I think that I'd like to say up front is we found that the fuels performed very robustly under various loading conditions and millions and millions of cycles. And I'll show that here in a second. And that's where the results are documented.

When we started this, nobody knew how the fuel was going to perform, and so that conclusion that the fuel actually performed very well, it turns out it was very ductile compared to what we thought it would be. It was an enlightening conclusion.

Let me show you a little bit about fuel. This is the H. B. Robinson fuel, and what we've done here is we've sectioned it axially, and you can see the pellets here. These pellets happen to be about .27 inches long, which is short for normal fuel pellets. Normally they're .4 to .6 inches long, so that's why you see this many pellets. What you'll see here, this is high-burnup fuel again, and you can still see the dish between the fuel pellets. That was interesting.

We had not -- people, when they do micrographs of the fuel, you section the fuel, you don't do it at the fuel pellet-pellet interface. So that showed that the fuel, the dish is still there. Although this looks like a gap, that's really epoxy. That is a very tight gap or tight bond right there, the fuel/clad interface, and that epoxy is laying over top of that interface because it couldn't go in there. But it shows that the fuel, even though it's highly fragmented, the fuel along the edges does not want to come out. There is a tight bond there.

Here's some of the results that we got when we started doing our tests. And if you look, you'll see the curvature, the amplitude over here, and this is the number of cycles here, and this is the H. B Robinson fuel. A couple things to notice here, these are pictures of actual fuel test results.

And in virtually every case, the fuel broke at the pellet-to-pellet interface.

Also, we got almost no residual fuel to come out. We put a pan under the fuel, because we only have a two-inch gauge section so it was easy to put a pan under the whole thing, and when it broke, well measured the amount of fuel that came out, and it was very, very small. In fact, some of it may have come out due to the grinding. The machine stops almost instantaneously, but not instantaneously once there is a break. The break is very sudden.

You don't see -- we measure the load on the system and the break is very sudden. You don't see a protracted growth pattern in the crack. You could see the lot numbers in a number of these. It just showed the fuel remained. Even though it is high-burnup, it has remained in have good condition. And you can see the dish there, so it really shows up much better in these pictures.

Things to see here also is the number of cycles it takes to failure. We ran a lot of this fuel. We started testing at high loads. These would be the higher load area here, and fuel would fail in the hundreds of thousands of cycles; okay, cycle being to one vibration, up to the millions of cycles, six, seven, eight. We ran one test to 11 million cycles, and it takes a long time to do that, so a number of cycles you see here didn't fail. We just said, okay, that's more cycles than we'll ever see. We now have reached kind of an asymptote, that at that load level, the fuel will not fail for a significant period of time.

We also tested -- let me go there. We also tested the Limerick fuel, the North Anna fuel, the Limerick fuel and the MOX fuel, and when I put those on the same curve, you see a little bit more scatter, and this is to be expected. One of the things we want to test, we want to look at, is what are the important aspects of fuel that would affect the longevity or the lifetime of the fuel? Pellet clad length, is that important? Is hydrogen content in the clad important? The geometry of the fuel makes a difference. The BWR fuel is -- the clad's slightly thicker, did those make a difference? And so you do have some data scatter in here. And you can see, if you look at the legend over here, you can see there's a lot of fuel that did not fail, and you can see the M5.

When you make this and you convert everything to stress or to strain and you take -- which takes into account the geometry of the fuel sample, the scatter is much less, which is what you would expect. And you see it falls along kind of a classic load. There's a number of cycles curve, an SN curve, that you classically get with any material. And that was really good to know, because one of the things we want to do is be able to model this. And by seeing that this does meet what we would expect, it allows more ease of modeling.

One thing we also found was, while we were doing the test, we're handling the fuel samples with manipulators. So think about holding everything with two fingers, only the two fingers are over there and you're over here, and so we accidentally dropped one, our operator did. When you do that, is that a legitimate sample to compare against -- you know, to put in there and run the test. And you've shocked it so maybe and maybe not. So just to make sure, we dropped it a second time, and now we know it's different. And then we ran it the same load that we had run a previous sample at, and right here is where the previous sample was, and this is where the second sample broke. Does that mean the shock decreased? It's one point. Does that mean the shock decreased the lifetime of that? It might.

What it indicates is there is some work to be done there. It indicates to us that maybe jolts or shocks make have an effect on the lifetime of the fuel, and how much we don't know. So, in fact, in this whole talk I don't draw conclusions as much as observations. We are working in a hot cell. The radiation dose to our equipment in the hot cell is very, very high. We have a mechanical machine in there that has wiring and plastics, and our fear is that this machine is going to burnout due to the radiation dose, so we have spent all our time and money trying to break as many samples as we can, and then we're going to go back and do detailed analysis, which includes more modeling than we've done.

So we do get observations. Are they hardcore conclusions? Not at this juncture. As we get more data and we do have time to analyze it, we'll come forth and say, look, we've thought this through, we've vetted it with our peers, and we think this is the truth as we see it. So right now everything is our opinion, but it's, I think, an informed opinion. So, anyway, what that tells us is that jolts or shocks to the system higher than normal loads may have a difference.

Another thing we saw -- let me go back to this one -- is this curve comes down and really goes down asymptotically. Is that really a one curve or is it really two curves? When you have a high load it may affect it one way. At lower loads you may not be affecting it hardly at all. So if this part of the curve is one piece of curve, that's useful information, because the reality is if your loads that you're experiencing on the rail are down here, there's some conclusions you can draw from that.

We didn't do hydrogen testing of the clad we have. What we do know is we got the clad a good amount of the clad from

Argonne who has done analysis of it, and they have looked at the oxides and the hydrides in it, and there's a correlation. And they told us with this oxide layer, the assumed hydrogen is X. So using that information, we went back and looked at the samples that we had and said some of them fall into higher hydrogen levels than others, and it appears that those with the higher hydrogen levels tend to react differently and fail differently than the clad that the lower hydrogen. So that leads us to the observation that hydrogen content may have an impact on the lifetime of the fuel.

Like I said, in the BWR samples we thought, well these may be very different than the PWR. They have a ZIRC liner that absorbs the hydrogen. They have a bigger pellet gap. We think the clad, the pellet bond is very important. We initially thought the pellet-to-pellet bond was important. We looked at that and we could see that one micrograph we had earlier, that the pellet-to-pellet bond looks to be pretty tight. And then after we looked at it and analyzed a little bit, we said, well that kind of mechanical pressing together, that's not chemically bonded and that bond isn't very strong, and that bond breaks very rapidly in our test in the first hundreds to thousand cycles. But the pellet-to-clad bond is very well tight.

In fact, when we defuel cladding to send to Argonne for Mike to do his tests, it's very challenging to get the fuel out of the clad. Normally we drill a hole in the clad first and then put it in nitric acid. And drilling it out is a very challenging task. You'll go through drill bits getting that out -- I'm sorry drill hole the pellet, not the clad; right. And we drill it right through the middle so that you don't affect the clad.

But what we really found was that the BWR fuel reacted very similarly to the PWR fuel. It's a little different on the curve. Okay, it's a little bit more ductile. But the clad-to-pellet bond is very tight right there, and we think that makes a big difference, and so that it will respond very similarly to the PWR fuel. We talked this morning at great length about hydride reorientation and what the potential effects of hydride reorientation effects are on the fuel. And Oak Ridge has been tasked to test fuel that has hydride reorientation effects. To do this, we have some H. B Robinson fuel that is the same fuel off the same rods that we have tested previously.

To make this work, to reorient the hydrides, you have to have to add the hoop stress back in. The only way to add the hoop stress back in is to repressurize the rod, and that's a very challenging thing. What you have to do is remove the oxide on the outside part of the sample. You have to clean up the inside, insert -- defuel about a half inch of either side, weld on an end cap, pressurize it. So we spent a lot of time and effort to do that, and that equipment has been tested out of cell, on pre-hydrided samples. We've also now moved the equipment in-cell and tested on pre-hydrided samples, and we tested it on real fuel, a four-inch sample, just to see if we could do this and we could get good results. In fact, we did.

When we were trying to figure out the goals here in the hydride reorientation testing is to make sure we have hydrides. We don't want to test it, go all this effort -- we only have four samples -- test it to maybe you'd say, prototypic conditions, do everything and say, gosh, as Mike was pointing out earlier, that hoop stress isn't high enough to make hydrides reoriented. So all you did was prove they don't reorient at that hoop stress. Well that's not what this test is all about. So we actually tested and went through a number of out-of-cell tests to say, what's the best way to make sure we have hydride reorientation? And in concert with the NRC, we gave them some options on how to do this, and the decision was made, let's do it like Argonne has done theirs. They got hydride reorientation.

So we picked this thermal cycle to use, where we actually do all the preparation, we heat the rod up to 400 degrees, we pressurize it to 3,500, 3600 PSI, and then we cool it at one degree Celsius per minute. And when we get down again, we'll heat it back up and send it through four cycles. That worked in our out-of-cell -- or our in-cell fuel test sample. We have now completed that on our first real sample last week.

Now that sample hasn't been tested in the CIRFT machine yet, and we will test it in the CIRFT machine and we will do the micrographs to see how much radial hydride formation we got, and we'll be able to present those results, hopefully at the May meeting. Our goal was to have it done for this meeting, but as was pointed out, the sister rods came in, and that kind of put -- that had our priority attention in the hot cells and moved this back just a little bit.

What you'll see here is that sample I was talking about of the fueled radial hydrided sample. This is preconditioning. And after we welded it, pressurized it, these are the radial hydrides that we got. And so we think that this was a heavily hydrided sample, and as Mike Billone pointed out, when it's that heavily hydrided you're not going to get as many radial hydrides as you may, because the circumferential will not all go in solution and they will prevent some of the radial hydrides from forming.

As I said, to help us understand what we're doing, we actually have a CIRFT machine that's out-of-cell. And in that CIRFT machine we can use stainless steel or pre-hydrided zirconium or pre-hydrided M5 with aluminum inserts to test in different conditions to help us understand what we think we may see when we do the real samples in-cell.

To help us understand what we're going to be out-of-cell, we also do finite element analysis, because in that we can say, at the pellet-to-pellet interface, what do we expect to see? At the pellet-to-clad interface, what do we expect to see? And what are the effects that may change what we're seeing, things such as pellet length, the amount of hydrides in the cladding, the pellet-clad interaction, the fuel rod condition prior to doing the testing that we're doing? So we have used fairly extensive finite element analysis to help us investigate what we're seeing and plan for our testing.

As I pointed out, when we prepare our hydride reoriented samples we have to heat it to 400 degrees. Does that make a difference? Does a annealing -- that's kind of annealing temperature, does that make a difference in the results? Would that stress relieve our sample so that we're not comparing necessarily apples to apples. I realize in a cask, that one -- those samples will get heated to some temperatures, as Brady was pointing out this morning. But the samples we've done in our hot cell, the ones we're going to be compared to, they weren't annealed. So we've done a limited amount of annealing test to see does this make a difference or does in situ pressurization maybe reduce the radiation and induce clad effects. So we do have that data so that we can incorporate that in our conclusions.

My initial observations from the CIRFT testing conclude the fuel does provide a lot of strength. And in our calculations, we think it's up to 40% strength to the fuel-clad system. And, remember, we're never transporting just clad when we move spent nuclear fuel. We are moving fuel/cladding system, and so it's important to understand the system effects. When the clad is fatigued, failure occurs primarily at the pellet-to-pellet interface almost uniformly. The pellets retain their shape at dish and chamfering.

And considering the complexity of the fuel, it was significant that the data was characterized by the SN curve with very, very minimal scatter, considering that each piece of rod is somewhat unique. It's not a homogeneous rod, as we pointed out this morning. But yet it does meet that -- it does fall on that curve reasonably well. And if you want to say there was an endurance limit, we tested to 10 to 11 million cycles at reasonably low loads, and the fuel performed very well, and above 10 million cycles, at about 7 Newton meters, it didn't fail at all.

The pellet-to-clad interaction, I think is, is very important. Understanding the pellet-to-clad bonding efficiency is something we need to do. Hydrogen concentration appears to have an effect. We need to understand that better. The spent nuclear fuel has significant stress concentrations and residual stress distributions that vary. Transient shock may have an impact that we need to look at a little bit further. And the addition of the pellet strength data the fracture toughness data will be essential to understanding the system performance. And what we're doing in the future is we're going to continue our FEA. We're going to continue doing the detailed data analysis that we haven't been able to do to date.

We talked a little bit about the sister rods coming in. There is a test plan that's being developed within DOE, the labs, and that has a reasonable amount of testing to look at the issues I just laid out, and because we have the full rods we'll be testing at the bottom and the middle and the top of selected rods to see is there a difference, or due to the conditions the rod sees during irradiation, and the sister rod destructive testing includes heat treating the rods some to see whether the thermal annealing effects make a difference, trying to find out what the uniform hoop stress, do testing on that to see whether that makes a difference. We will do the Met, the scanning electron microscope and the transmission electron microscope exams. We may look at isotopics. We will certainly do hydrogen analysis and figure out the different mechanical properties.

Okay, great, thank you very much. Questions? Gerry?

Yeah, Frankel, Board. Thanks, Bruce, and thank you for showing us everything yesterday, the Gen-1. I think it was really helpful for all of us to have seen the thing working, and helped us to understand what was happening.

You show data here. I'm a little surprised. I mean, you must have done a lot of experiments on what could be control samples.

We did.

Right? So you don't show the data.

No. I only had 30 minutes, but, boy, Jean wanted me to show lots of that.

So how do those SN curves compare to just fresh rods, you know or --

I don't have that here, but we did do that data. We have it for, like, stainless steel rods now. Now, remember, we're using aluminum inserts. We also, by the way, tested to see the difference in pellets, whether the pellets make a difference, a full log for example.

So you show these trend lines for hydrogen content and you didn't show the data, because I guess the data are ugly. You have trend lines --

No, actually, we just don't have enough of it. We have the few points.

Anyway, I think it would be interesting to have zero hydrogen; right? So you know, of course they're not irradiated.

I can provide that.

You should have -- so the question is, how much to the right are those data?

Off the top of my head I can't say. I'm not sure if we know. But I can provide you that information. We did do that.

It just seems that those samples you could then compare to a standard fatigue test. To try and understand what you're doing, you can use those kind of control samples. This is, you know, nonstandard.

And we did that, and we have that data.

Yeah.

So I could certainly provide that to you.

Okay. I called down -- we started with about 75 slides and you get about three seconds per slide, and they said, "No, we're not doing that."

Thank you for not doing that.

We're certainly not doing that.

So I've got another question about the test, though. So, you know, I saw data yesterday that the failure occurs in one cycle, okay, so you're going along and then, boom, it breaks.

That's right.

As opposed to other metals, and you would expect to see some fatigue crack growth and it degrades over time. I mean, I think you showed one. I'm a little confused by that; right? So one of the problems of SN curves is that you don't get, you know, the good engineering data, so, you know, Paul mentioned fracture mechanics approaches and you had said, you know, well where do you put a crack initiator or something, it's a little complicated.

Right.

But, you know, when you get cracked growth rates then you can -- and you can make engineering predictions based on fracture mechanics and some militant things like that. But I mean, the fact -- or the observation that the crack is, like, totally brittle, right, so that it just --

When it gives, it gives.

It opens -- right. And that doesn't make a lot of sense to me; right, because first of all, these are circumferential hydrides right, then so the crack is propagating across large parts of the zirconium that's not hydrided; right, so there's some weird crack morphology. So it's a very unusual fracture. So it's another thing that's unusual about this is the way that it fails. I mean that kind of concerns me, and maybe it's really representative. And I think maybe it's associated with the stress state that happens because of the fuel pellets that, you know, once a crack initiates, then, you know, you have some really increasing stress --

Stress riser right there.

In the, you know, the way that it bends when you push it the next time so that it comes apart.

That certainly could be.

But in any case --

We'll do it later. We'll do it later.

That would be fine. I just want to say that, you know, understanding the crack growth rate and what's controlling it, and this would be very helpful, I think.

Okay.

So I'd be interested to hear what Dr. Wong has to say.

So please identify yourself.

Jin Wong from Oak Ridge National laboratory. I want to point out high-burnup spent fuel is composite material not isotropic. See, the hydride ring forming the reinforcement part for that fuel. So Brady talked about crack propagation, when you touch those hydride rings you change duration. Okay. So we don't seecrack growth, we see delamination. But delaminationwill not affect your crack growth. Severe crack growth. So why —— Just like you play the rubber band, or rubber plastic, you go several string cells several times, or ten time, the string doesn't separate itself, but still has strength. But any of them give up, pop, whole thing pop. So my suspicion here, hydride ring on outer range is a reinforcement. But if delamination is sufficient enough they pop, it pops right through. That's my guess. Certainly we need to do a lot of fracture mechanical controls, and I agreed with you fully yesterday. So fracture mechanic is our next goal to really going to put a material response on a local level.

Another question, Gerry?

No, that's all.

Okay. Other questions? Efi.

Efi Foufoula, Board. I could not help but notice that in all of your fitted curves, in all the plots that you had, really had the same exponent, which was -.25, plus or minus. It was very, very little. This is, like, one-fourth, and my question is, have you tried to physical, you know, fracture mechanics or other arguments to see whether such a rate of the amplitude to another cycle to failure really can be explained physically?

No, we haven't.

Okay. Sue.

Sue Brantley, Board. So you showed, like, a million cycles to failure? What does that mean to me, like, in terms of this rod being driven down a road or on a train --

Every time you hit a bump --

I know. But how many cycles --

Oh, how many cycles?

Like, this number is meaningless; right? Like, you know, in route 80 you probably get a million cycles, in Pennsylvania anyway, in about an hour.

Or sooner.

Yeah.

Actually, I don't know the answer to that until Sandia is doing some work to -- and Paul McConnell will talk about that next. That's their area to say how much vibration you think you'll see and the load of vibration, and so, I mean, if you looked at mine, you said, okay, there's the asymptote it's 8.8 Newton meters. Is that a lot? Is that a little? How big is a jolt? Where are you on this curve? Paul will talk about that.

Okay. And then the second question -- I asked you this yesterday at the tour -- you take these six-inch pieces in a couple places on the rods, and then how are you going to convince yourself that, you know, maybe there's some defect that's spaced longer than that that you're just totally missing, you know? How are you going to make sure that that is a representative sample somehow?

Well that's one of the questions you'll always have is some rods will have defects in them that we don't know. So the sister rods, we plan to do testing; a number of tests on the bottom, a number of tests in the middle, a number of tests on the top. And you we're going to look at them and say, okay, we think we can do it under grid spacers, which is an area that we haven't done already that maybe that's an area of interest that could have a difference.

So I don't understand, what is it, you're going to do it under grid spacers, what does that mean?

When the fuel is made to help on the out -- if you look at it, to help hold them in position, they're built with spacers that hold them in position, and those are put at very engineered spaces by the manufacturer to keep vibration down and harmonics down. But they have springs on them, and they're actually holding the rods. So is there something -- because they are in physical contact with the rod for a period of time, do they induce a defect or a stress riser somewhere there?

Normally we don't test under those because that also, those grid spacers also absorb neutrons, so the power under them is slightly less. So we've always been interested in other areas. Well now we're interested in those areas, and we'll look at those.

All right, I think we need to move on to the next speaker. Thank you very much. And the next speaker is Paul McConnell from Sandia.

Okay, thank you. What we've been hearing most of the day are presentations that have to do with properties of the rods and the strength of the rods. And what I'm going to talk about is what kind of loads the rods are subjected to under normal conditions of transport, simulated normal conditions of transport. Sandia has done three sets of tests. We use the Sandia shaker, over the road, and a multi-axis shaker. In all the tests there was axle PWR assembly. You can see it in there, and all of the tests, the assembly was in a surrogate truck cask basket, exactly like the truck cask basket for the NAC LWT.

The zircaloy rods were instrumented with accelerometers and strain gauges. For the Sandia shaker, we took the data for truck testing, which is widely accepted to be conservative relative to rail normal conditions of transport, and we did vertical accelerations only, which are also widely accepted to be the greatest acceleration mode on an assembly during transport.

Then what we did, in the next year, we took that same basket assembly and put it on a truck and put it on a weight that was the same weight as a NAC LWT truck cask -- the only truck cask in the United States -- and we did road tests. Finally, we wanted to compare results from the Sandia vertical acceleration-only test. We went to Nevada, used the multi-axis seismic shaker, six degrees of freedom. We used NUREG for truck. In this set of tests we actually simulated rail, truck-truck, truck and rail.

And another difference in these tests is that in the first two series of tests we had a zircaloy rod, and we had a rope of lead in that rod. And there was a gap between the lead that simulated the UO2. In this test we used that same configuration, but we also filled one of the zircaloy-4 rods with lead pellets and molybdenum pellets to better simulate the fuel.

So I'm going to show you a video here. This is at the Nevada test site. And, Jason, can you give us a go here. This is simulating rail shock, not just vibration but the kind of shocks you would get in a rail test. You have to push that little --

[Inaudible].

So think of this as the rail car. Here's the basket. Can you see? There's a -- now the bar is up again. There's a little cutout up here, by the way, that I'm going to show you in a minute. We cut a piece out of the basket, and there's a Go-Pro camera right there so we could look in at the rods that were populated the 17-by-17 assembly. And I, hopefully, will show the video on that. Do you want to just try the next slide and see if that one will work?

Now in all of these series of tests we did multiple tests. We did multiple vibration tests where we subjected the shaker table to vibration cycles, which are low amplitude compared to shocks. Shocks for truck would be like potholes, railroad tracks, and for rail, of course, would be going over frogs or bumpy track or something like that. So we would do several truck tests, and on this shaker table we did several shock and vibration, both truck and rail. So there's multiple data. Okay, so here's what it would be like for an assembly on a rail car just going down the track and getting shocks, not vibrations. You would hardly see the thing move when you're doing vibrations.

Okay, so let's go to the next one. I think you have it, Jason. Okay, this is non-normal. This was an accident. There was a test. Okay. And can we repeat that one? Now I think you all saw this thing go. This is way beyond. But watch up here. And you saw that assembly. Down at this end, the basket dropped here, and the assembly jumped on that end.

During normal -- let's go to the next one now -- during normal transport, here is a truck racing down a road and head hitting a pothole. Here's the assembly. Now think of this, this truck is going down a dirt road, no relative motion between the rods, the assembly doesn't hit the top of the basket. It just sort of lays there and see clouds moving up here. It just sits there. It's a lump. Very little motion relative to the basket. It's just sitting there. This is a road condition that is beyond anything that an assembly would ever experience. It would be on interstate highways, nice paved roads at the utility.

Okay, so what did all this mean? All of the data that we ever collected, truck or rail, fell right here. This is a stress/strain curve for unirradiated low-burnup and high-burnup zircaloy-4 at elevated temperatures. And the interesting thing about this curve by the way is there's not much difference between an irradiated and non-irradiated zircaloy-4 in the stress/strain space. Slope is the elastic modulus. There's actually -- the curve continues over here because there's ductility, even unirradiated –- or irradiated. All of the data, dozens of tests, is down here.

So Bruce mentioned that -- we'll show this on the next one here. Some of the other conclusions were that in all of the tests, no matter where we put -- regardless of the test series, for a strain gauge that was at a similar location between the series of tests, we got essentially the same data, whether it was lead rope or lead pellets or molybdenum pellets. The difference between 310 and 119 is irrelevant. These are micro strains.

We looked at the difference between the data that we got when we had lead rope, in other words no pellets, like UO2, and zircaloy that contained pellets, in this case lead pellets or molly pellets. Again, data very, very similar. It may seem like there's a difference between 44 or 301. That's trivia, trivial difference. Pellet-pellet interface versus having a strain gauge over a pellet, not on the interface, very little difference. It didn't seem to matter. And by the way, we always had a gap in between the material inside of our zircaloy tube and the tube itself, and that's the worst-case situation, because a tube is not backed up by -- I think Bruce said there's something like a 40% strength increase when you have bonding between a pellet and/or versus if you have a gap. So these data should be conservative.

This is Oak Ridge data, some of the very data that Bruce just showed. And, yes, and they recorded the bending moment, so on and so forth, the strain, stress, number of cycles. Here's the conditions that we had, typical conditions we had. We had applied bending moment for simulated normal conditions of transport of this compared to this, what they were doing, because they wanted to fail their material. Our curvature fraction of this, all these parameters are related, which is why the stress-strain curves or the SN curves probably had the same parameter, because there's a relationship between all these values.

And here's a typical strain, and this a shock strain, not a vibration strain. So the question is, is how many cycles of failure might we expect from the data that we've collected for fatigue failure, and my conclusion is that the answer should be greater than 22.3 million, because Oak Ridge did fatigue tests and got no failure for much higher bending moment and much higher curvature, higher strain, higher stress than what we observed in our test.

Here's a fatigue curve that is similar to one that Bruce just showed. This is taken from a NUREG, by the way. And this is the curve that the NRC suggests that vendors use in doing a fatigue analysis. Interesting thing is the Oak Ridge data falls right on that curve. This is the lower bound curve, the black line, and the Oak Ridge data just sits perfectly along that line, but it should be higher because this is intentionally driven down in that NUREG.

Here is the stress amplitude. This is fatigue strength in terms of stress versus cycles to failure. Here's the stress amplitude typical of what we got in our Sandia test. Over here is the estimated number of vibration cycles for a 2,000 mile rail trip. It's between a million and a little less than two million. That's our estimate of how many vibration cycles you would get in a typical transport from a utility to some sort of storage facility or repository.

And using the stress amplitude for shock strain, which I said was higher than the amplitude for vibration, we're well below the failure line for fatigue. We estimated that the number of shock cycles for a rail test is about -- what is it -- 20,000 or 30,000 cycles. And so this red line would be appropriate for comparison with that number of cycles, again, well below the fatigue curve.

Now, just as an aside, fracture toughness has come up a couple of times, three times I think I heard that mentioned. In the very first report that we did on the Sandia shaker testing, there's a brief fracture mechanics assessment in there, and we found data -- measuring fracture toughness on a zircaloy tube is a real trick. Anybody knows about the size of specimens you need to use for fracture mechanics tests fracture toughness testing, but none the less, in an EPRI report there is data on the fracture toughness of zircaloy-4.

And using that data, which is a very low fracture toughness value, and using this kind of a strain, or a stress rather, based on a strain that we measured, I had estimated that you could easily tolerate a half wall flaw, half through-wall flaw in a zircaloy tube and still not get failure on a fracture mechanics basis, and probably could go up to, say, .7 through wall thickness flaw. I didn't present that here, because I mention that because it's come up a few times. And the fatigue curve is in NUREG 0800.

Okay, these tests tell us that the strain measured during simulated normal conditions of transport are very low. Because they are so low, fatigue is probably not a problem, and failure, just whether it be from shock causing a rapid failure or from fatigue during normal conditions of transport seems unlikely. Now however, none of these things look like this. This is a rail cask in the UK. There's a cask back here. The assembly is in a basket and the basket is inside of the cask, and the cask is on the rail car. It's not this, it's not this. This is the real world.

So we have this very unique opportunity that has come to us from a Spanish company in Spain called INSA, and they have offered DOE an actual real rail cask as a loaner for free, with a basket, all the hardware, so on and so forth. And if we had that we would be able to dot I, cross the T on all of this data to really, really confirm that these stresses and strains on the rods are very, very low.

And so here's what we would propose to do should we get this cask from INSA. It's a Spanish government company. They make the casks for the Spanish government -- or the Spanish industry. And the plan would be that we would start at INSA with our assembly, and they are going to give us free, another assembly, fully populated with zircaloy, a very similar assembly, so we will have two assemblies.

In Spain they're going to transport by heavy haul truck, a rail cask on a truck. And we have to do that in the United States, to a certain extent, to work on the shut-down site. Orphaned site studies shows that there are some facilities that will require some heavy haul. So we could do heavy-haul testing in Spain, then the cask with the assemblies in it, all instrumented, would be shipped across the ocean, collect data on an ocean. We have to do some barge shipments. Other countries, it could be an international program. Countries like Japan and Korea will be doing ships transport.

We get to the port, we run across the country in a train, collect data. We take it to the American Association of

Railroads Transportation Technology Center, where they have a facility. It's sort of like a Railroads National Laboratory, where they have all kinds of different track, odd track, bridges, different track, and we'd conduct a series of tests in Pueblo, Colorado, to really subject the cask basket assembly to sort of off-normal rail conditions. Then we'd go back, collect data, ship, collect data, back to INSA, take the assembly out, bring it back, and have a complete set of all transportation modes and lots of data.

So, again, it would provide data for all the transportation modes. It would add to the library that we've developed for rail and truck loadings. It would eliminate all the uncertainties that can be weighed against the testing that we've already done because of the necessity to simulate the configuration of the assembly in a cask. We think it could help support the future licensing of the demo cask, the demo cask, when it's done, and all that fuel has to be transported. And I think such a test would be the definitive word is there a problem with normal conditions of transport. So that's it, Rod.

Okay. Thank you. Questions? Bob.

Bob. Bob Einziger, the Board staff. I've got a couple questions and comments. As it was shown in the previous presentation, all the failures occur at the pellet-pellet interface. So obviously something's different there. Either it's the stress state or the support, or it's maybe the hydrogen is moving and it's changing the mechanical properties. So when you compare the behavior of these unirradiated rods and the yields and all with the high-burnup rods under normal condition, how do you know you're comparing it with the right mechanical properties?

Okay. So it's a good question. Here's my answer to that. Here, let's take -- I don't know if everybody can see it, but here's a rod, the black, another pellet to pellet. Here's the interface. When Oak Ridge is bending their tube pellet system, they are bending enough that that interface is broken between the two pellets, and it causes the edge of this pellet and the edge of that pellet to come up and contact the ID of the rod at the top. So you've got this point contact between -- of the pellet now on the inner surface of the rod. A lot of bending. When you do that, the rods will necessarily come up and contact the inner surface. That causes the stress riser that causes, 100% of the time in the Oak Ridge tests, failure at that interface.

In the normal conditions of transport we never have enough bending to cause that to happen. That's why in our test when we had a strain gauge here versus a strain gauge across two pellets we got the same answer, because there was not sufficient bending that the pellets are contacting the inside of the tube.

That's a reasonable approach. Is there any way to experimentally confirm that in the Oak Ridge tests?

Yeah, Oak Ridge could not put such a load on it and see where it might fail. But then they'd see -- but then they would have -- the problem there is that they would have to do so many fatigue cycles that they could never -- you know, none of us would be here, because that asymptote that goes out. So it would be very hard to do. The only way you can fail one of these rods is to apply a moment or curvature or strain that is beyond what happens in normal conditions of transport. They want to fail, but it just doesn't happen in the real world.

Okay. Another point, glad to see that you're going to be using an actual cask, because I'm not convinced yet that that mock cask that you used for the road transport is actually a good model of the road cask, especially the large 32 assembly or 37 assembly cask.

But another point, you know, the Japanese found that when they transported the cask -- both the casks, that there was sliding on the gasket that actually affected the containment capability of the cask, so that they actually had to have the ability to put a third lid on to maintain the containment when they were in storage. Is there any plans to monitor the containment of this cask as its going over Europe and transatlantic and over this country to see -- to get another data point on that information?

Well short answer would be no. And the only variation that we would have in this INSA cask from a cask that was actually being shipped with radioactive material in it is we'd have to use an instrumentation lid versus the real lid. We have to drill holes through a lid that they are going to design and give us for free, so we can get the cabling out for the instruments that are on the assembly, and so we will not be using the real lid. It won't be bolted down like a real lid, so we can't do that.

You done? Okay, Sue.

So you're teaching us that it looks like the likelihood of failure is really, really small, but risk is, you know, the likelihood of failure times like how bad is it if it fails. Can you just talk to me about what would actually happen -- I know it's extremely unlikely based on what you've shown us -- what would happen if the cladding broke, you know, in a truck accident or rail accident and the pellets came out? Like, what actually happens in that case?

Well there's others that could get into the radiological basis better, and things like criticality, but in my view, nothing would happen. For one thing, most of the fuel that will be -- assemblies that will slipped in the United States are going to be in welded canisters. So if that assembly broke up or if one rod broke, or even many of them broke, they'd still be in that canister, and they would still in a cask, all of which are designed to prevent release of radioactive material. The cask is designed to prevent radiation coming out, gamma rays coming out. So nothing would happen.

It would be a mess once it got to a central storage facility or the receiving station at a repository. But in terms of transportation, nothing would happen. And if anybody wants to dispute that, you know, please do, but I don't hear anybody. That's maybe a naïve answer in terms of, well it's -- there is some risk. It's only at the location -- the DOE location where the cask would be received, and then there would have to be some plan for that. It wouldn't be on public highways.

Bob.

Yeah, so, Bob Einziger, Board staff. This work was originally planned to look at how the effects of normal transportation. If you're going to have an accident where your questions addressed the situation's completely different. And then they have to analyze what's going to happen under the 30-foot fall and the submersion, and the results could be -- these results would not apply.

Correct. This is for normal conditions of transport. And there's also the hypothetical accidents, which is, you know, something really bad goes on, and we simulate that with a 30-foot drop onto an unyielding surface. For normal conditions of transport, you have to address normal vibration and normal shock. So Bob's right, if you had a hypothetical accident, a severe accident, there could be some damage. Now remember the one video I showed where the assembly just went like that, the strain on that was, say, roughly 2,000, to 3,000, versus the 200 or 300 that we got for normal shock. Maybe in May there may be a discussion of an analysis that PNNL has done for the one-foot drop under normal conditions of transport, and that's about the level of strain that PNNL has got for dropping the whole cask one foot. And so that video, which was I call "non-normal," that was maybe analogous to a one-foot drop. It was just a fortuitous thing, and we've used the data. We didn't plan that.

Okay. Paul [Turinsky, Board]. I'm wondering, you know, there's so many different cask and canister designs with different basket designs, different capacities. How different would you expect the responses to be given this wide variety of products that are out there.

Short answer is, in my opinion, not much. But that's one of the reasons that Pacific Northwest National Lab, Nick Klymyshyn, who is sitting right behind, right over here, has modeled all of the tests that we have done every step of the way. He has a very good model of the assembly with the rods in it, and he has essentially been able to validate and benchmark his model. So the hope is is that if you gave us another cask or basket or whatever, Nick would be able to model that new system, or whatever cask that DOE ultimately buys for their eventual transport campaign, and we could do all of this via modeling. And Nick will -- if we do the

INSA test, he will model that test every step of the way, then he'll collect the data that we've got, you know, after we analyze it and compare. But so far he's always been very, very close, always a little higher, but, you know, right there on the bottom of that stress/strain curve.

So is the plan going forward actually, independent of doing the experiment, to use this model and to look at the different casks, or require the cask manufacturers to do analysis like that?

Well they do analyses, of course, to get their COC, certificate of compliance. They don't do them quite as detailed, I think, as what we are doing, but I would think that, yes, that would be the plan, that that tool would be available. It would be available to vendors, certainly be available to the people within DOE that are designing the 2043 rail car? Are you familiar with that? That's the rail car that is required for a rail cask. It will be available to those folks, and when we do -- when DOE eventually buys casks, I'm sure we would do a modeling analysis on them.

Okay, last question. All right, thank you very much. We'll take a break now, but we'll start at 3:00 so that we have plenty of time for discussion at the end. All right, thanks very much.

If you could take your seats, please, we'll get started in just a moment. Thank you. All right, the next speaker is David Tang from the NRC, so, please.

Thank you. The two previous speakers talked a lot about their lab work, their recent lab work. It really helped me to make my presentation easier. I'm going to talk about the normal conditions of transport. This is the subject of this particular meeting. However, naturally it's going to extend to some other applications, such as hypothetical accident conditions, which come naturally toward the end of this discussion.

I'm not going to repeat too much about the lab work at Sandia and at Oak Ridge. I would just recognize some of these values and datas and measurements, this and that, how they are being used to confirm our licensing practice as to the input, as to the structural performance on these perspectives.

Toward the end, I'm going to discuss the licensing review implications, what we've come up with and how we take off on this space.

For instance, a few assembly will have a number of these components. They're very difficult to track as to the individual components, how they work together and what they really mean, and whether even we need to get into this detail to look into the component performance, and, after all, to demonstrate that fuel rod will perform satisfactorily. For instance, we have the top nozzles and bottom nozzles, spacer grades and a few other attributes we need to be looking to.

And here is a modeling -- a summary of the modeling attributes consideration for fuel rod analysis or fuel assembly analysis to define the -- analyze the configuration after accident or after transportation, formal condition of transport. And you see that, for instance, these, say, 144-

inch-long PWR fuel, there are seven spacer grade locations, spaced at about 24-inches apart. And notice that the fuel rods are really having free hanging onto these free spacers, because free spacers are attached to the these control rod guide tubes. Guide tubes.

Guide tubes. And so it's free standing. Free standing means that it's allowed to move up and down by itself. And for certain conditions, we evaluate it. It's very beneficial in the sense it's not going to have some hard drop and because there is, say, leaf spring kind of arrangements to prevent the rod to come up and down freely and introduce some friction and our job is to add some damping to that. But even for that, it's not the focus of our evaluation, because we are looking to some bounding configuration.

To you left your see these test results from Oak Ridge. So it's composite action, very hard to drill a hole intopellet, not to mention that they work together, and it has a rod that has strong rigidity, a rod with strong strength, but on other the hand, in our analysis, to make the analysis tractable, so we have these defueled rod, meaning that just the hollow tube there. In the model the hollow tube performance, and see what the bounding performance of the rod, if truly, in this case, can really experience during transportation, and the storage as well.

Now a little recap of these regulatory perspectives of course, talking about normal conditions transporting two components, one is vibration. Another deals withthe one-foot drop – operational one-foot drop, whether this will cause any kind of concerns. And naturally when we talk about 71.55(d)(2) you ought to know the content, the configuration, is not substantially altered. So these are the regulations we ought to be in compliance with.

Of course there is regulatory gap in the sense that after long storage -- and do you know the condition of the spent fuel’s content going into transportation mode? And upon arrival at the next station, what kind of condition do you have for the spent fuel in the storage? So those are the gaps in the regulations. Those gaps don't have these type of description or prescription, per se.

Now, talking about, say, some other aspects of the licensing basis, such as, say, this Reg 7.9, which prescribed that the combined stress ought to be looking to fatigue evaluation, but not necessary for spent nuclear fuel, or even high-burnup fuel for that matter. But the fatigue effects should be looked into. And, say, again, for resonance vibrations conditions ought to be considered, or should be considered as well.

In our transportation standard review plan 17, 16 and 17. And it has similar kind of language as those identified as the regulatory guide, which is of higher tier requirement. And, of course, notice that these regulations -- this is a standard review plan made reference to NUREG CR-0218 and other NUREG CR reports, which were generated by Sandia Lab many, many years ago, but still in good use.

In talking about vibration, really, the -- talking about vibration, really, there's no language as to what it really means to the reviewer or to the applicant, and it's non-prescriptive, it's pretty strange. It's very much performance-based regulationPart 71. And what does that really mean? That means that we are relying on the lab reports to give us some sense of where we should be as far as, say, design input, the vibration and amplitude or duration or number of vibration, so on, and so forth. And for instance, in this case, it leaves a lot of latitude for the users, or applicants, for the reviewer to review what is adequate, or of something additional needs to be addressed as well.

Of course, in this community, we talk about high-burnup fuel transport. A question may arise as to, say, some more vigorous definition for loading conditions. I understand if you go by S-2043, this American Association of Railroad, then perhaps the requirement, loading requirement may be not even as severe as what we have been dealing with, as I read some of these reports generated by one of the labs. So that, say, the reason here we were more definitive description of the fuel conditions may be associated with some other fuel behavior, such as effect of hydride presence and hydride reorientation. We didn't get into that soon.

So I mentioned that my job is easier, and Paul made a good presentation, and we follow, say, the lab report very closely. In the sense that I will use the next slide to describe what we understand what came out of these reports of these type of investigation, in the sense that, see, at worst, see, during this truck or railroad transport there could be some kind of shock. Shock is being represented by decaying sine, decaying sine waves, meaning that a mixture -- a number of harmonics, but a decayed harmonics in that sense. But it lasts only limited amount of time, five seconds, or even less. But this perhaps is the most severe condition that the lab devised for their, say, shaker table testing and some other investigations.

For instance, this in case that from NUREG CRO-218, and they were response spectrum with resonse that that it is a signature or some representation of some kind of a wave form, say five seconds, or whatever the duration. Meaning that if you drive that single degree of frequency system you are going to see this kind of response. So that is, in this case, is a mixture of, say, 60 or 50 decay sine waves. And once you have this mixture and plot the response back that will envelope what had been observed for that in that NUREG report.

Now the test configuration, I think Paul described it already, and I think a picture is worth many words. In a sense, the environment, vibration environment, the vibration realized by the conveyance and vibration being realized by the basket and being realized by the fuel assemblies. All of these need to be considered. And unfortunately they were accelerometers and tried, so you access the accelerometers being used to monitor the environment.

Now what does this measurement mean? So I just looked into their report and the pickup of all these large numbers, and you'll notice that for the rod expiration, up to even 22-some G, if the measure response is fielded at 1,000 hertz, which is very generous filter. So you have all kind of noise and spurious actions, this and that, and response. But if you have a fuel assembly, which is perhaps, say, 20 hertz, natural vibration, or even less, so this high-frequency input doesn't really mean nothing because the fuel assembly or fuel rod even won't be able to see them, because they are moving less rigorously, and so there is no resonance of build up.

So, ultimately, from our perspective, we're looking at the lab report and we made judgment and see what an important driver is to the evaluation concern. And you'll notice that if you field it at the hundred hertz signal, see, immediately you see only 6G but even hundred hertz is perhaps a little high, so your cut off frequency is moving down, down, down eventually. There are some things which I think is pretty common, say, understood. Perhaps the 2G or 3G, that kind of vibration will be associated with the shock. It's pretty reasonable already. I would leave that for the general public to address. However, and we noticed that these results are to be interpreted with some insight for as to how the basket fuel assembly, fuel rod responds.

Now here you have seen this kind of picture as to the reason why the lab test bending, and the fatigue test was, say, done by the Oak Ridge lab, and I'm not going to get too much into that. Basically the fuel assembly is subjected with some kind of random vibration, and occasionally some shock, and the response could be modulated and to have something a little higher response. But in the end, see, whether there is endurance limit or some kind of stress that the vibration is going to be recognized as a good design basis.

Now, see, for the, say, bending test, you heard -- what I heard this morning already with so many things happening, you ought to be very diligent to be current. But I think we follow the lab results pretty closely, and we have good benefit of using these results. And are very interested in seeing that for the phase-two testing, I understand that there will still be four samples, rodlet samples, and we'll be going through this static bending, take it off, and then the rest of them will go through these, say, cyclic bending testing. And we are very anxious, in fact, we said that would provide additional basis, a reasonable assurance, who has to make, say, findings as to the safe transport of spent nuclear fuel. So we look at it from that perspective. Any conflict of interest, we don't think so. We found that's a good use of resource.

Now these are the two parts of great interest -- I just want to spend a few minutes here -- in the sense the left one you see that there are four static testing just to push that bending moment R to the limit of this tester. So just introduce this much bending moment into that pure bending configuration from structural performance perspective, along the whole rod the same bending moment. That is what's going on or what's being tested.

So here is something to be realized. You'll notice that the moment, the bending moment introduced as high as 80 Newton meters. Later on we are going to talk about this. One Newton meters amounts to about 3G equivalent if you talk about side drop. And Paul just mentioned that the work out of this testing about they are set their set up was only .7G, .7G. about .7 Newton meter. .7 by 3 is about 2G, 2G.

So there's a very robust performance as far as high-burnup fuel being tested at the lab, this H. B Robinson fuel, and there are some other good things coming out of all these of similar pedigree. So we are expecting something similar to come out. And this right-hand side amounts to, see the fuel rod performance. Fuel rod means that -- see, the fuel record with the composite actually in place, the cladding and the pellet together, will increase the flexion rigidity, perhaps by a hundred percent. And for that matter, it takes more bending. It takes more beating. And you can see that, of course, it's not linear. The first segment, the second segment, the third segment, that was what the Oak Ridge was able to characterize and give us something pretty useful to make judgment.

Now you say what does this measure data for fuel rod really mean? If you use a FRAPCON, say, program to estimate what kind of, say, fuel cladding performance, and you notice that, see these slopes of these curves really represent the stiffness and the rigidity in that matter, and the composite cross action with fuel inside is perhaps twice as rigid or 50% or more rigid. So for that matter, it has a much more capacity than we just analyzed from, say, this hollow fuel tube or just the defueled configuration. So you had that kind of, say, test result to help you make judgment that, hmm, it is there and there's something we can take good advantage of.

Now this, you have seen this picture many times. The important thing here is, see, because of presence of fuel pellet, this composite action, the apparent stream, the way you can measure from this curvature through this LVDT displacement measurement will come out, for instance, say, .5%, and when you get into the endurance limit part, it's about .1%, but don't take it as what the cladding is experiencing, because this has some transformation as to how the test results are being interpreted. But I think we found this information very useful.

Now, you see some other fuel cladding types test through the DOE programs dealing with Limerick fuel and MOX fuel and the North Anna M5 fuel. They have similar trend.

In other words, the endurance limit is very impressive. Well we're going to see that what kind of, in the moment, introduced for this endurance limit. And you will notice that later as part of 15G -- what do I mean by 15G? 15G, and therefore normal transportation, you have only .4G or some very small number. So, meaning that the test has --

the test program was very robust. The response is even more impressive.

Now, this is a picture I borrowed from Joe. He made the same presentation. Well, look, what do we mean by hydride reorientation? Well you have these circumferential hydride, you have the radial hydride. They all point -- they all align into the board, meaning that the presence of these hydride will not disrupt the bending stress field. That is a very important part of this discussion context. Except, for instance, if they are some circumferential stresses associated with a few -- associated with the gas pressure, which may be not that big. We talked about this morning, about 20% the most that kind of a magnitude relative to the bending stress. And you can take advantage of that and make a story of that as far as acceptance criteria is concerned. There are many different failure theories in the structure mechanics and similar field of this calculation part.

So what these kind of, say, hydride presence give us some picture to see that their performance shouldn't be much different one from the other, as far as bending is concerned. And, incidentally, the bending is a primary the challenge to the fuel -- spent fuel included of course. Now, having said that it will look like, I think it's good to work out some numbers. So we use some classic three-moment equation for crunching a solution, and a work out for these particular Zirc-4 15 by 15 fuel, and with pristine properties, and seven spacer grades, and span, say, 24 inches each for that kind of fuel, and use that Lawrence Livermore report published many, many years ago, 20-plus years ago. It came out to say that through these calculations you can arrive at this at the bottom, 1G -- if you have one newton meter, that kind of input in this machine, one newton meter means ten newtons because the moment arm was only ten centimeters. So work out that, you're going to see that the testing was really very severe as far as input is concerned.

So I just used -- by the way, this first bullet, some corrections should be read fuel rod, lateral exploration endurance limit. There was a typo or just oversight on my part. Basically, you see, if we talk about the nominal dimensions without removing the oxidation layer, so then what is being tested at the lab Oak Ridge was 15G, you banded at 15G for endurance limit as is described. If you accommodate this 100 microns, that oxidation layer, and it means that it was -- the fuel rod was able to take above 12G. Okay? Just work out these details.

And interesting to note is that, see, the cladding -- the second bullet -- to reach the yielding of the cladding based on the data we observed you need about 84G. What we mean by -- no -- 84G, yeah, because the corresponding, say, moment input is 28 newton meters. All these numbers added together -- and you're going to see that as far as the failure, the fuel rod to fail, it was not defined because after that point the machine capability exhausted as far as displacement is concerned. So if we use this set of number and work backwards for this cyclic loading the fuel rod test at Oak Ridge, it was for endurance limit, 15G as minimal.

Now, see, for licensing part, we have seen one of these applications to say, well, they reviewed these old but still very useful NUREG report, and they came up with .45, 40% of G -- very minimal as compared to what we have tested, at least by a smaller by one order of magnitude for that matter. And we licensed the particular cask with that kind of, say, driving force for vibration consideration. And so that was for vibration part. How about for 1G, say, the free drop? The 1G free drop with impact limiter protection dropping is about 20G or 18G or 15G, depending on what kind of impact limiters you design to that. So, as far as we are concerned, the test results supplement what we have observed and we understood the subject. Very useful for that matter.

Now, having -- we have seen these phase one testing. We just mentioned that as far as expected behavior is concerned, phase two tests ought to be similar because the hydride being in the circumferential or radial direction would not make any difference as far as bending performance is concerned. And, incidentally, the Oak Ridge testing didn't have any, say, circumferential stress introduced, even though it was introduced to initiate the hydride reorientation. But when you tested it, there was no pressure inside. It was bending. So we are anxious to see the results.

So, in conclusion, we will say that the NCT vibration performance margin for the field fuel endurance limit, 15G much greater than .42G used at one time. And the cladding unlikely to fail undergoing NCT vibration will not fail under quasi-static one-foot drop. We're not talking about strain read effect, which is another subject we don't know whether it's really that severe or some other considerations need to be made. And we expect comparable phase one results to be again showing up for phase two results with reoriented hydride. And there are still challenges, and test result being considered for us to develop review guidance, which is slow-coming and some of this data -- it's real-time data, we need that. And short of that, we feel that it's not going to serve too much -- well, the real purpose because it moves fast and getting to that point. Of course, we talk about only one cladding materials after this point, as far as that part is concerned. The rest of them, how to address them, I think still need to be addressed, and so other programs. Well that concludes my presentation.

All right. Thank you. Questions? Lee?

Lee Peddicord from the Board. You had referred to a couple of different situations you looked at for both the 15 by 15 and 17 by 17 assemblies. From your perspectives, from the NRC, is there any distinction between the two or might one fuel assembly design somehow be slightly more limiting than another?

Very good question. In fact, we don't see much difference, except sometimes when the applicants or when the labs may have used some very conservative assumptions as to the boundary conditions or constraints, say, introduced to a fuel rod or fuel assembly to do analysis to see that it can experience such a high stress strength activity. We have seen this kind of report. But, you see, talking about -- you see, like PWR fuel, see it suspended there through space of grades in -- see, the worst comes to worst, perhaps one system will have, say, let's just call it a 20 Hz, like in a natural frequency, another is 30 Hz or another 15 Hz, it can be analyzed. In the end, you calculate number of cycles along this 2,000, say, rods, which came out more than a million cycles. So it's still, say, a few million cycles. And we have seen that at least for one cladding type. It was 15G versus some very reasonably, I think, more, say, side lateral vibration challenge.

Okay. If I may, this was actually back on what you presented this morning, you were talking about that ISG in progress on retrievability.

Uh-huh.

And you made reference to "provides guidance for system-based retrieval." What is system-based retrieval?

Good. See, the original ISG deals with assembly-based. So there was some need, some real concern why should be assembly-based retrieval. That was at one time, ten years ago. But I think there has been discussion on the street, the retrieval, you can use perhaps, on the basis of canister, your retrieval of canister and disposition of the canister, whatever way you see that. So that is what we mean by other systems, other alternatives for system retrieval.

Thank you.

Yeah.

Other questions? Gerry?

Frankel, Board. So this morning you talked about the hypothetical accident conditions that is covered by one of the regulations, 71.73(c)1, as a 30-foot free drop. So what of the data being collected help you to address that situation?

See, in fact, we have been doing this or the applicant has been doing this with quarter-scale or third-scale -- the model -- drop testing, dropped at the height of 30-foot and through the submitted to the loss in the work out, say, the data reduction. For instance, for the scale model drop testing, you observed, for therigid body deceleration, that was what we talked about, we used some kind of pipes or some heavy stuff to rips in the cask body, protected with impact limiters. And you drop it and you measure the decelerations. For instance, like, you measure, say, 200G for that scale model. By the scaling law, if it's quarter scale model, divide it by four. So, actually, as applicable to the prototypical configuration, it's 50G. So that kind of, say, testing protocols, the series and basis are very well-established and we have been using that for a long time. And, more recently, we moved even beyond that on getting to, say, analytical prediction with some kind of an explicit dynamic analysis model.

So are you pre-testing high-burnup fuel at 50G?

The answer is there's no need to do testing per se, because what we did with these scale model drop testing was to establish applicable, say, loading compartment. So there's one-to-one correspondence to say, "Well, within this cask -- or within this transportation package -- it doesn't matter, it's standard grade fuel, whatever -- the applicable deceleration or acceleration will be such and such." Then the internals, including the containment boundaries, will be analyzed for that particular challenge. Shall we say, in general, for instance, up to this point, it's about 50G for the large span fuel transportation cask for side drop. In end drop, anywhere from 50G to 60G, you drop it, this is the peak deceleration. If you talk about tip-over, what, because the size of the cask is about 40G. So all these are very well-documented, established as being used in our -- by the applicants to make a case in their application.

So what will happen to high-burnup fuel if you – at 50 G -- I can't hear you.

Now let me try to answer that way. Now, that's a good question, because their tests show that the fuel was good for that kind of a 28 newton meter, which translates to 75G or 80G. So for that particular fuel, just bended, because you have these fuels supported at space of grade, in turn it's supported by the fuel basket. But drop it and 50G side drop, and there could be some amplification there, which could translate that to 70G or 60G. But the amplification factor is easily calculated. Now, even if you have 80G, 75G internal basket, fuel included, the test results show, at least for H. B. Robinson, it was good for that. By the way, it was good for many, many cycles, not just the one shot. The only thing we haven't discussed further was strength read, in fact, which perhaps was not that important either. We're not getting into that because, up to this point, the licensing protocols and the way we look at the subject is such.

Could I follow up on both of the previous questioners, because I think I've lost the train of thought? So, in the retrievability question, it's not the spent fuel assembly but it's the canister that matters in terms of being able to recover or retrieve?

We didn't rule out, say, assembly-based retrievability. But the other same retrieval, say, configurations is acceptable if you can demonstrate it. So that would mean by some other --

I guess from Gerry's question and Paul's comments, I'm led to wonder why we care about cladding when we don't care about what's in the canister. Or have I missed the train of thought?

You have a very good question and valid concern. That is really -- I will leave this to other people to interpret because do you really want to ship something without knowing the content, what the content really is, whether the content is 100% intact or whatever at the end of the storage, or partially damaged, damaged to what extent? So what are you going to write into your certificate of compliance as you describe the content for transportation; what do you mean by that? So there are these kind of --

But to know or have that kind of information, doesn't it require retrieving the assembly and examining it?

That, in general, is the common understanding. But, on the other hand, see, we have these aging management program, we have these time TLAA, which will enable us to use some baseline configurations to make, say, conservative estimate for the end of the storage, or beginning of the transport, what that content geometry or content physical form really looks like, and there is some estimate there, which is not uncommon. By the way, if we really don't believe what it has happened, it can always do some kind of sampling test on one or two, but we are not getting to the area to say that you shall do this, say, investigation or testing before you can transport, no, we don't have that regulation. By the way, that is what I call regulatory gap. And we don't -- at this point, it's still being discussed, but whether there's a real need there and how to implement that, that is really the challenge.

Okay. Thank you. Other questions? Bob?

Bob Einziger, Board staff. David, isn't it premature to change from an assembly-based retrievability to a canister-based retrievability before you have established whether you're going to have to repackage the fuel for future stages in the backend of the fuel cycle?

See, Bob, I think you were part of that discussion, and we know that there are two different views there. And both of them have some advantages there. Yes, we understand that, see, draft one for these ISG [indiscernible] just offers alternative way to do it. And, by the way, I think the general public has provided comments as to that draft ISG, and we are working to get it published sometime. I don't know how we disposition these comments, but others, the canister-based retrieval is part of that. We are going to accept it, but what that really means to licensees or some other, say, considerations up to this point, I don't know how to answer that question.

All right. Thank you. Other questions? Bret?

Bret Leslie, staff. I would kind of reiterate kind of what Bob had said, is while you're focusing your ISG on storage and transportation, DOE is not really the licensee. They're going to have to accept whatever you certify. And if they have to repackage, what is their gap from taking something that was certified to be canister-retrieved and repackaging it into a waste disposal canister where they need to take out the assemblies?

Yeah, the question was good. And, in a sense, you see, the most, at this point, we look at is interim and consolidate the storage facility, at this point, which is being considered for this pilot program. And there was no content as to -- there was no discussion as to repackaging that part. Whether it would fall into some other, say, permanent repository initiatives in the future or whoever, that is something we don't know. I'm not trying to get into that. But we understand that, see, at one time the canister was much smaller than these 32, say, PWR or 40 PWR, and many other configurations. We knew that, but at this point we're leaving that to future discussions.

Other questions? All right, thank you very much. You've almost made regulations interesting. Thank you. All right, let's welcome Brady back.

Thank you, Rod. So, so far today, what we've been hearing about is what we would call our separate effects or our small-scale tests. Now I get to talk about something that is really fun. This is the big thing. This is the whole enchilada, if you will. It's the High-Burnup Spent Fuel Data Project. We've gone through a number of iterations on the name, so you might have heard us talk about the High-Burnup Cask Demonstration Project; we got rid of that name because we said, well, wait a minute, we're not really demonstrating the cask, what we're really looking at is the fuel. Gone through a few other names. This is the official one for now. Who knows when it will change again? But, for shore, we still do call it the Demo Project.

So why are we doing this? The real reason is to collect data to validate and confirm the technical basis for extended storage of high-burnup spent fuel. I'll phrase it this way, there's a NRC Interim Staff Guidance Number 24 that talks about what you need to do if you are doing a demonstration project, what kind of requirements you need to meet to help the NRC staff be able to accept the applicant's application to them. And this goes back to the 80's.

So the picture you see here are a number of casks. They're still sitting out at Idaho at I believe it's called the INTEC facility. And the way this started is back in the early 80's, when some of the utilities pools were filling up and they said, "We've got to do something," and that something was leading to dry storage, the Department was tasked with assisting and doing a number of contracts with industry to put together these demonstration projects. So one of the first ones is with that green cask that you see there, that's called the CASTOR V/21. The original demo was to load this with spent fuel, 21 assemblies as the name implies, and to do thermal tests on it. How does this work? What are the temperatures going to be like? Is this even feasible?

So, again, in the early 80's, this cask was loaded. And it was loaded hot, even though this was low-burnup fuel. Also, I think every assembly is above one kilowatt per assembly. Eight of them were up as high as 1.8 kilowatts. They were only two-year cooled. I don't know of anybody who does that nowadays. So they were screaming hot thermally, even though they were low-burnup. The lid of that, very similar to what you've heard discussed already, had penetrations in it with thermocouple lances to measure the temperatures. And in that demonstration of that cask they looked at helium backfill, nitrogen under vacuum, they had it in a horizontal configuration, vertical, to figure out what are the temperatures and to obtain data against which to validate the models of the time.

I just want to point out that in that cask, you know, just for full disclosure, the peak clad temperature was estimated to be as high as 424 degrees C. So for all these years, when we talked about really high temperature, it had to do with -- well, we observed it, but, again, one of the reasons is the very short-cooled, very high-heat load assemblies that were in there, and, to be perfectly honest, the old casks weren't designed to facilitate heat transfer nearly as well as modern systems are.

So I will phrase it this way, the Nuclear Energy Institute, knowing what the needs of the industry were, knowing what requests from NRC were, they had, very high on their list, we want to do this demonstration on high-burnup fuel. They worked with Department of Energy to come up with a plan. And as we worked through this plan, we had a few major prerequisites, if you will. Number one was we wanted to make sure we had as many different cladding types in this demonstration as possible. Again, because the focus is fuel, we didn't want just one or two cladding; as many as possible, because I'm hoping you have seen today just how important alloy to alloy differences are.

Because the plan was, once it's loaded and stored, eventually to open it again, possibly reseal it back up and let it keep cooking for years on end so we could look at it, one of the other requirements was to use a bolted lid cask just for ease of opening as opposed to cutting open a welded canister. So, with that in mind, the Department of Energy went out with a call to solicit proposals. The winning group came forward and said, "Hey, we have what's called a TN32" -- TN is an AREVA Transnuclear -- 32B -- "B," in this case, does not stand for "BWR," it's just a configuration of this cask, which means it's designed for single failure-proof trunnions. So it's the only difference between a regular TN32.

The winning team, the utility had four different high-burnup cladding types that we were very thrilled to be able to get our hands on. And they said, "Yes, we'll design a lid and we'll put seven thermocouple lances in there." Each lance has nine thermocouples located axially. So we're going to get a very good -- axially within this cask. I'll point out that AREVA is the one in charge of designing and building these thermocouple lances, and they are basically the same design that they used for reactor instrumentations. So they know these things work in very harsh environments, and we have very high confidence that we'll be able to collect data for a long time.

We wanted to dry the fuel using a typical drying process. There's really two ways that are allowed nowadays, either a vacuum drying or using what's called forced helium dehydration. In this case, we'll end up doing a vacuum dry. Once the cask is loaded, we need to be able to store it at the utility's ISFSI, take measurements and gas samples over time.

And the bottom bullet -- I think you asked Ned that this morning -- is what are we going to do when it's time to open? The Department takes that very seriously and is working very hard to identify a number of options. And if I may throw in that we understand that we have to have this consent-based option for whoever is willing to take it to have the state and everyone else agree to it.

So we had two different teams apply. And DOE awarded the contract to EPRI in 2013. Keith Waldrop, sitting back here, is the project manager for that. Ned Larson is the federal lead for this program. So, basically, the winning team of EPRI, Dominion, AREVA, Westinghouse, and NAC, I'll phrase it they're in charge of the licensing, the logistics, the operational aspects, but they do have the support from the six national laboratories. We've had numerous meetings with them throughout the process to have them help us understand what's going on. We give our two cents of what's needed from an R&D standpoint. And it's been a very good relationship in this.

If you have questions, I'll let Ned explain later, but for reasons of federal procurement and what it all means, it was decided that the first procurement would only be for a five-year period, so covering 2013 to 2018. So the bullets in green are what we've accomplished to-date. The team has the cask. We went through a number of iterations on planning what fuel would go in there and where they would be located within the cask. Develop a design and licensing basis document, otherwise known as a DLBD -- that is a lot of the technical explanations as to how are these changes still allowing you to meet the regulations of the NRC.

Dominion then submitted the license amendment request. They pulled the sister rods. And, as you heard and some of you saw on the tour yesterday, those sister rods were shipped here to Oak Ridge National Laboratory and arrived last month. So, right now, they're working on modification of the cask lids so we can put these thermocouple lances in. A license amendment is into the NRC and we're going through that process. The plan is to load the fuel in 2017 and begin the monitoring, and then store it.

The second contract that will be awarded will be to continue to monitor this cask and eventually ship it to a hot cell facility. As Ned said, they are looking at a number of options to see where that might go. But for a while we had talked about should we/should we not open it wet. And we decided it we really do want to open it dry, although my personal opinion is now that we realize the temperatures are a lot lower, our biggest fear of opening wet was what if I open it and I've dried it once, so I've changed and reoriented the hydrides, and now I pull some rods out in a pool and I have to re-dry it, am I going to make things even worse? That may or may not be the case, but with lower temperatures might not be as big of a deal. But the plan is to open it dry after approximately ten years. When we open it again, we'll pull rods to do comparison against the sister rods that you saw in the hot cell yesterday, and, of course, do reporting all along the way.

So I want to point out that when they install these thermocouples, it will be done when the cask is still full of water. So we will be getting temperature data from the moment they start to drain the water out, all through the vacuum drying process, through the refilling with helium. And then actually it's going to sit in this prep bay or decon bay, whichever you want to call it, for two to three weeks. That's key to us to allow temperatures to equilibrate. Meanwhile, we're recording those the whole time. We also have the utility. We're in discussions with them about how do we obtain temperatures of the ambient air, temperatures of the external of the cask and all that, all while minimizing dose to workers. So we're looking at various means to do this. But, again, to really help firm up not only the models that we already have and believe are validated, but there are a number of other organizations, including the NRC, developing their own codes. And this data will be available, as Ned said, to anyone to be able to validate said codes.

But while we're in that building for two to three weeks, one of the advantages, as Ned said, we can pull gas samples through the vent port. And it's a lot easier to do it in the building because that building is set up that if you had a radiologic release, you know, if, for example, we had release of xenon or krypton or fission gases from one of the rods, and as we're pulling that, if you had a leak, because you're already going through HEPA filtration and such, you don't have an off-site release. So, over that two to three weeks, we hope to take at least one, maybe two samples, but a lot is going to depend on what we get when we analyze it. If we see a problem, we're going to work with the utility to try to pull more samples. If it's boring, we might only have to take one.

So what are we analyzing for? Again, looking for the fission gas. We're looking for hydrogen, which would be an indication of did we have water that has undergone radiolysis. Can you see oxygen? Did we have the backfill the way we wanted? Did we have any kind of leak coming in? And also looking at moisture. And I think it goes to -- I think it was Lee that asked this morning, hey, at lower temperatures, am I drying as much? So looking at that data to see is there moisture is key.

But I want to throw out, this is something that hasn't been done before, and so I want to emphasize that just because you detect moisture does not mean that things are bad. As good of a vacuum as you pull, every process that you do, there will be a little bit of water remaining. NRC did a number of studies to look at what that potential might be. So just because you detect it doesn't mean things are wrong. And I want to make that clear.

So, again, the industry team has led the licensing effort with this TN-32B cask. It's fabricated; one of the things they had to do, though, is that cask was not originally licensed for high-burnup fuels, so obviously that's one of the things that's in the amendment. The labs worked very closely with the industry team in developing this design licensing basis document to cover the items you see here. And one of the advantages of this team is Dominion has what's called a site-specific license, so we are not licensing TN-32B casks all over the world or all over the U.S. to be used for high-burnup fuel. This is a one-time-only shot through their site-specific license.

A couple pictures. The cask is currently at Columbiana Hi Tech where it was brought into the building there, it was opened for inspection so they could look in and say, "Yep, it looks pretty good." They're measuring tolerances, making sure everything is fully up to specs. If they need to repaint, et cetera, it will be done. And that's also where the lid penetrations will be made.

So our milestones, again, everything in green is complete. So we completed that design licensing basis document, pulled the AREVA M5 rods over a year ago, pulled the Westinghouse rods about six months later. The license amendment request did go into the NRC; it was docketed by the NRC in late September of last year. NRC has already issued the first and hopefully only round of what's called "request for additional information." So Keith and his team are in the process now of answering those, and they give those responses back to the NRC. NRC then continues to review this. And what we hope is to have the license awarded somewhere in the January of 2017 timeframe. Of course, the sister rods have been shipped already. In February of next year, a year from now, we expect the cask to show up at North Anna, ready to go. They will do dry runs to make sure everything goes smoothly. And then in July of '17 we will load this with the 32 high-burnup assemblies, complete with thermocouples, and taking the gas samples. And then by August, it will get moved out onto the pad where it will sit for approximately ten years.

So just to cover some of the fuel, I know that diagram is a little hard to read but that is the map of the actual TN-32 cask. So each one of those squares is one of the basket holes, if you will. So we've identified, first, the assembly identifier on the top line, followed by what kind of cladding is it, what is the burnup, what was the initial enrichment, how many cycles was it irradiated for, how many years cooling is there, and then what is the decay heat. The bottom left number before the slash is decay heat at the time of loading. The second number is what we expect the decay heat to be about ten years from now, and that becomes very important to be able to do your calculations to say are my temperatures within regulations that I can actually transport this.

One thing I did want to point out is so the four assemblies in the center, there's one of each of the four cladding types -- we did that on purpose to drive them to the highest temperature we could get. As we went through iterations, we weren't happy with how high the temperature was, so we ended up replacing the eight assemblies surrounding that -- so these two, these two, these two, those two -- with assemblies that are relatively short-cooled. They're only five years out at core at the time this will be loaded next year. That's something that the utility does not normally do. Normally they let them go longer in cooling, but we did this again just to drive temperatures up.

In the sister pin selection, as Ned mentioned this morning, we pulled nine AREVA M5 rods. We have 12 of the ZIRLO rods -- one of the reasons behind that is, one, as Mike said, ZIRLO kind of one of the more interesting ones, so we wanted more of that material to test. As Bruce Bevard said, we haven't done the CIRFT testing on ZIRLO yet, so we wanted more of those rods. But we also have some of the older Zirc 4 and low-tin Zirc 4. They were received here and we're working on the test plans for this. Right now, the plan is to do two separate volumes, where volume one is a higher level, giving background, boilerplate information and "why are we doing this" type thing. The plan is to have that draft out by the end of this month, and we're looking for comments from industry, from NRC, from the Bboard, if you're interested and available in reading and commenting, and from the public as well. The second volume is the more detailed, outlining exactly which tests we want to do, and that will be later in the year.

Ned showed this diagram from the supplemental analysis that was performed. Again, just to outline that the 25 pins come to Oak Ridge. The plan is to send material to three other laboratories -- or two other laboratories to participate in the testing. This will change slightly as we go through the detailed test plan. We're looking at accelerating the time sum. We don't want to wait until 2025. We've come up with enough tests and what we call phase one and phase two that this needs to go a little bit faster. This is the system that those who were able to tour the hot cell yesterday saw kind of in pieces that Bruce talked about. But one of the reasons why Oak Ridge was chosen to receive this was not only do they have the hot cell capability to, but this ADEPT system is really a very nice system for doing the nondestructive examination of the cladding.

Our plan is to do this on all 25 pins, everything from detailed visual scan to gamma scan, eddy current exams, and such, just to -- and one of the big ones is profilometry, not only rod length but rod diameters at various locations. The biggest reason for that is we're going to compare that baseline data with rods that we extract ten years from now to be able to say did we have any rod growth, did we have hydrides that moved from the hotter regions of the fuel to the colder regions. So doing this will provide a lot of very important baseline characteristics.

So, amongst that, like I say, we'll be doing this nondestructive exam to come up with what we call the T0 -- Time 0 -- that means as received. What you saw sitting in the hot cell, we will look at. That is Time 0. We'll then, after all the nondestructive is done, puncture the rods to measure the internal pressure, do the composition of that gas to determine how much fission gas had been released, determine the free volume, because, again, that is very important to look at, what is that rod internal pressure. Those rods either intact before puncture, or afterwards where we'll, as Mike explained in his presentation, pressurize the segments to a known pressure, heat them to a known temperature, and do these what I'll call simulated drying to come up with what we call our T1 post-drying characteristics. And, again, those will be compared against rods ten years from now to say is what we did in the laboratory correct, can we validate that?

Some of the testing that were done -- that we will do, it's everything that you've heard today. We want to do baseline and post-drying tests to see what the temperature and strain rate. You've heard that today that in some instances strain rate really can have an effect on performance. Do the CIRFT test. Here at Oak Ridge, we want Mike to continue doing a ring compression test. And then we want to obtain material properties following ASTM methodologies.

So this -- I think I had talked with Susan yesterday -- when you manufacture the cladding, they go through the same types of rigor to prove to the buyer that this cladding is good, to identify how many weak points or flaws might be in there. So you do a number of tube tensile compression and burst tests. You plot them up on what's called the Weibull plot so that you can come up with your statistics. We're going to do the exact same thing on the irradiated fuel. And the biggest thing is -- I think Bruce said it right -- what we learn by doing the first round of testing will influence where we go from there. I believe, yep, that is everything.

Okay. Thank you, Brady. Questions? Bob?

Uh-oh.

We may break up your questions by recognizing others, and go back and forth.

That's fine.

Okay.

Bob Einziger, the staff Board -- Board staff. Let's see. Let's start out with what modeling or other efforts are in progress to use the results of the demonstration program to support the relicensing or expected future performance of fuels with other cladding types, higher burnups, other maximum drying temperatures, higher internal rod stresses, or BWR fuel?

Excellent question, Bob. So, I'll phrase it this way, first, within the experimental program, what we as the lab team are working through is when I say that the T1 data, the post-drying, I want to emphasize we're not looking at doing only the temperatures that we get from this demo cask. In July of 2017, we'll get the data from the thermocouples, we'll be able to validate our models with that data. Some of the fuel will be tested to that temperature and those pressure hoop stresses. Others of the sister pins we fully intend to use the tools that John Scaglione discussed, this UNF Standards Database that pulls things out of the GC-859. We're already in the process of going through and trying to identify what casks have higher temperatures, how high are they, working with industry to find out the hoop stresses. So we will use sister rods to look at those other extremes. In other words, we want to make sure that what we test covers the full range of what's out in the fleet currently.

In terms of models, we've worked very closely with Nick Klymyshyn of PNL. Before we did any test matrix in that, the first question was, "Nick, tell us exactly what you need to feed into your models," that, again, he is doing both for what I'll call extended storage and looking at -- make sure I say this right, Nick -- cask tip-over, cask drop, and design basis seismic events, as well as looking at normal conditions of transport. So I think we've got the modeling covered. It will be validated all along the way with data that we get.

I'm not sure that that's quite enough, because, I mean, there are lots of short-term tests. And the purpose of the high-burnup fuel demonstration was to show that the short-term test can be used to validate what's in the long term. And you're only going to have the one long-term test. And so how are you going to know that these short-term models that you're developing in the sister rod tests are actually going to be valid for the long term?

That's the whole reason why the cask gets opened in ten years, but it will also add part of what's being kicked around right now is is it possible to take some of the sister rods and actually heat them full-rod for extended periods, whether it's the full ten years or not is debatable, but to do a longer term test in the hot cell to help validate these shorter term ones.

Okay, I heard someplace, I think from somebody, that the maximum temperature in the cask is now going to be around 315 degrees C.

That's what I presented this morning, yes.

Mike has shown that when you decrease the maximum temperature from 400 degrees C to 350 degrees C that you've actually increased the ductile-to-brittle transition temperature. Might not you consider having Mike do similar studies at the 315 degree temperature to see if things even get worse?

Yes, the plan is to have him do those tests this fiscal year, as funding allows. And the plan on the sister tests will be -- excuse me, on the sister rods will be to do tests at those lower temperatures. And, you know, you're absolutely right in that we might not have the hydride issue, but at lower temperatures are we not annealing out radiation damage and having less ductility that way? So we also have tests that Oak Ridge is doing to focus solely on -- and Mike, I believe you said you're doing a test this year on looking solely at annealing effects, to see if that is causing this quote unquote "worse behavior" even at a lower temperature.

A number of times I've expressed the concern that if there's a rod failure releasing krypton that there's going to be segregation of the heavier gases to the bottom of the cask and that you're gas sampling from the top of the cask -- produced a report -- a theoretical report that says that shouldn't be the case because of the convective currents inside. Is there any plans to try to justify -- or not justify -- to confirm that that's the actual truth, because it would be a shame to take gas samples and go along and say, "No, there's no failures or anything," and then open the thing up in ten years and find there's failed rods because the theory that you've applied didn't work?

Yeah, I'm going to let --

[Inaudible from the audience].

Okay. So there is one test where we can try that on. It's a joint test between NRC and DOE that I think you're familiar with, Bob. And I think David presented a little bit on it. So it's looking at the thermal hydraulics in a BWR assembly. This is a surrogate assembly, not with fuel in it but with heater rods that Sandia is going to run, thermocouples all over. In theory, in that vessel that they have, we can inject xenon or krypton and look to see what happens. I don't think we have that definitely in the plan, but we will take that strongly under advisement.

One more question. As I know it, the license amendment request did not include the request for gas sampling out on the pad. When is the license amendment request going to be put into actually do that, and is the system, as it's configured now, suitable for taking those samples?

We believe it is suitable. My understanding -- somebody tell me if I'm wrong -- is --

September.

Oh, September is when you're putting that in? Oh, okay, so it's much sooner than I thought. So, according to the project manager, Dominion will submit that amendment in September. And, again, so the audience understands, as I said, sampling the gas inside the building is one thing; when you move it out on the pad, if, heaven forbid, you know, as we're connecting something, we have an uncontrolled leak, you now have the potential for an off-site release, which no utility wants, nobody wants to put up with. So we're doing everything we can to design a system that will prevent that, going so far as discussing putting a I believe they just call it the "paint shack" because every now and then you have to paint these casks, putting this paint shack over the cask, having HEPA filtration on it so that when we're pulling these samples on the pad, which, again, I think it was your question, I think, Doug, about how often are we doing this, probably once or twice out on the pad, just because it is an evolution that has risk to it, although it's extremely minimal. But we are moving forward with it.

One last question.

Okay.

Did I say that already? It was thought when the demonstration was going to be done for PWR fuel that that would bound the behavior of BWR fuel, because the BWR cladding is thicker, the internal stresses are less. But as one of your DOE reports indicates, the evolution of the design of the BWR fuel is going to thinner cladding and higher internal pressures. When will the DOE be in a position to confirm that the BWR performance should, in fact, be bounded by this demonstration and that a second demonstration using BWR fuel won't be necessary?

I can't answer the "when," but I can tell you we are in active discussions as the laboratory team to look at two things, the BWR fuel, as you said, because the newer ten by ten BWR assemblies are exactly as you said, thinner cladding, higher pressure, and, as Mike alluded to, just because of the microstructure of Zircaloy-2, it facilitates radial hydrides, even without going through the drying. So we recognize that as a gap that we're discussing. The other one that we're talking about is getting our hands on some of the IFBA rods, since those are substantially higher pressure. If I had to prioritize right now, I'm thinking that IFBA would come first, but we seriously are looking at how to get enough BWR either rods or even a couple assemblies to perform a demonstration that meets the requirements of ISG-24.

[Inaudible from the audience] but I don't have them right now.

All right. Additional questions?

Lee Peddicord from the Board. Going back to your map on slide 11, which is the layout of the way you're going to be loading the F cask. So a couple of questions come to mind. Of your 25 sister rods, which of those assemblies did those come from, and was there any more than -- was it only one from each assembly or --

Okay, excellent question, Lee. So the two rods from the Zirc-4 and the two rods from the low-tin Zirc-4 did not come from these assemblies because those designs are old enough that they weren't designed to be taken apart. So they came from sister assemblies that were already taken apart for previous programs many years ago. I believe -- Keith, correct me if I'm wrong -- but I think the two in purple that are the M5, those are the two assemblies from which the nine AREVA M5 sister rods were taken, the ZIRLO rods, actually none of them came from assemblies going into the cask but rather from sister assemblies, ones that were irradiated in identical positions throughout the core. The biggest reason for that is when you're pulling rods, it's actually a requirement that when you remove a rod, you have to insert a dummy rod back in so you don't have an empty space in there. And just with the risk associated with that, we decided we didn't want to -- we wanted to minimize any potential damage to what was going into the cask. So we took those from sister assemblies.

So, looking in the map here, it's kind of striking the range of times that they've been out of the reactor, from the shortest ones at five years to the longest one, 22 years.

Correct.

And it looks like that's the only low-tin Zirc-4 assembly is the one there, number 20.

Yes. So when we worked with Dominion to identify what high-burnup fuel do you have, they didn't have that much of the low-tin Zirc-4 or the Zirc-4. In fact, the regular Zirc-4 was part of a EPRI high-burnup demonstration something or other project -- I don't remember the full name -- all those many years ago, which is why -- actually, I think the Zirc-4 is 28-year cooled, if I remember.

30.

30?

Yeah.

Yeah, so, yeah, like I say, that was part of a program many years ago to really drive fuel up. You know, kind of going back to my discussion this morning, if you look at those eight assemblies, like I say, those are the shortest cooled, as you mentioned, five-year. Dominion normally wouldn't put something that short-cooled in there, but, to me, that gives you the best guess of what are their recent offloads looking like. And this was the high end of their recent offloads, which is why we put them into this system.

And finally, one last question, as you're pulling rods, if you have crud disposition, do you lose any of that as you pull rods out of assemblies? So your 25 sister rods, might they be a bit atypical of what you're going to have here in the cask?

So, I know -- I think Steve wants to address that, but we have full video of them actually pulling the rods. I sat in Steve's office and watched them. You can see the scratch mark, if you will, from where you're pulling up and the rod is hitting those springs within the spacer grid. But, other than that, those little scratch marks, we have not seen any gross spallation.

It's not flaking off [inaudible].

Correct.

Thank you.

All right. Sue?

Sue Brantley, Board. Can we go to the second slide? I've just been puzzling over DOE speak here, and I just thought maybe you could talk to me about it. The first bullet, I guess this is your objective, "Collect data to validate and confirm the technical basis for extended storage of high-burnup spent fuel." Because I'm puzzling over what you mean. Like, a lot of people will talk about validating a model, and we could talk about that a little bit. Is the technical basis, is that a model?

I would phrase it more as it's just the grand picture that we have. You know, us as researchers, I think most people with NRC, we say, "Hey, we know this is safe. We've been doing it." But what we were lacking is a lot of publically available data. And I want to make that distinction. The fuel vendors do an awful lot of testing to prove that their assemblies are good for reactor operations. And we know -- let me rephrase -- they know and NRC knows a lot about that, but it's all proprietary data. And so we, on the DOE side, don't get to see all of that. So when we're talking about the technical basis, I would phrase it as we're going to have publically available data to validate, you know, yes, the models, the previous experimental work that says, you know, "We believe this stuff does not fail, that it's going to meet, as I said, its intended safety function."

So do you mean you have a numerical model of what these rods are going to look like or these assemblies are going to look like after ten years, do you mean that? And then you're going to collect data and compare the data to that numerical model?

So there's a number of different models. There's fuel performance models. Like this morning when I showed the Oak Ridge report on the hoop stresses, that was done with a model called FRAPCON. You can use these models to predict creep, you know, what happens with hydrides. So a number of them exist. I wouldn't phrase it that we have any sort of singular grand --. We don't have a performance assessment model for everything.

I guess the choice of words, you know, I think to most people "validate" means prove true, and "confirm" means show that what I already thought is true is true.

And I think that's right. We don't think we're going to be surprised. We think everything we're doing on the small-scale separate effects test is showing that things will work. But one of the -- and then, actually, thank you for bringing this up -- one of the main reasons for doing a demonstration at this scale is to make sure that when you have all of the mechanisms working simultaneously, did we miss anything? We don't think we have. The earlier demonstration, when they opened it up and looked in, the easiest way to paraphrase the report that's that thick is we looked in and everything looked as good as it did the day we sealed it up. We're pretty confident that that's going to be what happens on this high-burnup demonstration. But we need to prove it.

I think the point I'm making, or I'm kind of getting around to making, is those words, "validate" and "confirm," really imply some level of confidence that -- you know, you're still running the test; right? You think it's worth running the test.

Yes.

It's worth -- what is this test going to cost us? It's going to cost us some money; right?

Mm-hm.

So it's worth making the test, and yet you're stating at the start you're going to prove yourself right. And I guess that's kind of setting -- that's what I mean about DOE speak, it's kind of setting yourself up that it looks like we're going to prove ourselves right, no matter what, which, of course, is not really what a scientist does, and I don't think you would do; right?

Right. Correct. I appreciate that. So thank you. And actually, because of what you just said, I do want to throw out one thing to put things into perspective. So this contract with EPRI is a cost share program between DOE and EPRI. That portion of the contract to get the casks, to do everything to modify it, the licensing, the loading, the sealing it up, we're talking on the order of $20 million. On the laboratory side, just the work with the sister pins -- I'll echo what Bruce said yesterday, he was hoping to impress on people how difficult it is to do work in a hot cell and just what that does. We're talking few tens of millions of dollars just to do that kind of testing. So you're right, this is a huge program, and we don't want to presuppose the answers. So I'd actually like to work with you to say let's come up with something better in terms of terminology.

To bring the public along, you're basically saying that we're collecting data to compare ten years out to what we thought was going to happen. I mean, that's not "well-wordsmithed," but that's, in essence, what you're doing.

Correct.

Okay. Mary Lou.

Mary Lou Zoback, Board. And I apologize, I wasn't able to make the tour yesterday, so I'm struggling a bit with just some of the basics of what you're doing. So the sister rods -- Lee asked the questions, I was going to ask about those -- those are largely similar rods to the rods in the 32 fuel assemblies that you've loaded into the cask. Now, one point, you said you let a contract for the cask, but then the cask had to be checked and repainted because it'd been sitting around for so long. So why did you have to let a contract for a cask that's been sitting around for a long time?

Plain and simple, it was owned by AREVA. They had built that. These systems are very expensive.

But it wasn't specially built for your experiment. It was one they had sitting around.

Correct.

Okay. Okay. And then the other question I have is the rods you're testing in the cask demonstration are sitting in dry storage. The sister rods are going to be tested in the hot cell.

Yes.

So they're, like, completely different environments, aren't they?

That's why when I say when we want to test T1, we're going to subject our rods to a simulated drying process.

In the hot cell.

In the hot cell. So we believe that, you know, if we do it under the same temperature and internal pressure, that we will match what goes on. But, again, as Dr. Brantley alluded to, we need to prove ten years from now when we pull it out, is that or isn't that the case.

Okay. And to get back to this -- you know, it seems to me you really need to know the initial conditions in order to work forward. And what would happen if the humidity is ten times higher than you expected it to be in that first gas draw?

So, really, in terms of potential degradation mechanisms, again, NRC went through and looked at how much water do I need in order to really cause degradation. I think the simplest answer to say is if I have a humid air environment, as long as I don't have any through-wall defects in my cladding, I'm not going to affect the fuel, can I oxidize or add hydrogen to the Zircaloy will be dependent not nearly as much on how much water is there but what the temperature is. If it's too cold, it just isn't going to do anything.

Okay. Thanks.

Back to Bob.

Brady, you've got a pretty ambitious characterization program on the sister rods that's going to cost beaucoup bucks. Maybe those aren't bucks that the DOE has, or will ever have. I don't know how good Ned's magic is in drawing money out of the DOE. But have you looked --

I still have work to do on that [from the audience]

But have you looked at that plan and saw exactly what is necessary, the absolute minimum you have to do to support the program, and what happens in terms of data loss and modeling loss if you don't do the rest of the plan?

Excellent question. And all I can say is with the six laboratories represented in the meetings, there's at least 12 different opinions on that. But I'll just say I think Ned, as the program manager, Ken Sorensen as the control account manager for us, us "labees" are doing the best they can bringing us all together to try to answer that question. And actually, I believe we're meeting later this week to discuss, again, what's the priorities to do exactly as you said, what must we do versus what's the nice to haves. And

I do want to reemphasize, Ned has made very clear that both volumes of this test plan, once they're finalized in draft form, will go out for comment. And we would appreciate your input as well as NRC's, the Board's, and members of the public.

I'd like to just discuss something on your previous talk this morning where you had the data needs gaps. Those reports were done in a 2011 timeframe, before the Blue Ribbon Commission report came out, before there really was a consideration of storage after transportation. Has any evaluation been done on those gaps to determine whether there's any additional gaps that need to be addressed based on the fact that you may have to store, and transport and store, and transport, whoever knows how many times?

Again, good question. Our opinion, as we've looked through this and had discussions, is the gaps are the gaps. They're general enough to cover anything. So, ultimately, what I think you're asking is we've had numerous discussions of how do we make the brand model to account for cumulative effects? I know I've beaten up Nick a number of times at

PNL to say that's eventually where you have to go, make sure that any degradation I have during storage feeds into that transportation model. And any effects that we find when Bruce does his -- so I'm not sure if we said it good enough, Bruce, but I'll say it again is, with the sister pins, one of the things we really want to look at is not just the CIRFTtest that he's done under continuous same conditions, but, for lack of a better term, I'm going along, I'm vibrating it, now I'm going to subject it to the shock of I've gone over a crossing, a bridge, whatever -- you know, that does what Paul showed -- a big jump. Then I keep vibrating. What does that have? Well then I do it again. So we haven't figured out how, I will openly admit, but we know we have to get to this cumulative effects model and combining all the data in that fashion.

All right, Brady, you've stood up very well to extended discussions. So we'll give you a break, although there's still time for more questions.

Oh, dear.

The audience must realize that we've gone over time, and that's because Travis Knight from the University of South Carolina is not available. So we've used his time to subject Brady to the extra questions. We still have some time, and so what I'd like to do is invite Board members to ask any of the day's speakers additional questions that they have in mind. And I think, Gerry, you had some.

Yeah, thank you. I would like to bring up a topic. So I believe that Mike Billone said that the critical aspect of the performance of hydride cladding is the hydride distribution, so the concentration of hydrides and their orientation and their distribution and their connectivity. So, at the risk of being denigrated again and being called an academic, you know, I want to think about this notion of using non-radiated samples. So we heard from Mac. Are you still here? Where's Mac?

So we heard that non-irradiated samples behave differently. So you have to test the radiated samples. And you said -- but then you said, well, you can use non-irradiated samples to check for trends. But I didn't get any details on that.

I mean, it seems to me that if you could develop, in non-irradiated samples, distributions of hydride that match different typical microstructures that you see after the appropriate thermal and mechanical treatments, that you could do a lot of tests. You know, this whole thing suffers from a dearth of data. And the tests are hard, you know, there's no doubt. So you could collect a lot of data if you could replicate these samples in a way that would be valid.

So, first of all, would non-irradiated samples with the same distribution of hydrides behave in the same way? I guess maybe you can talk about how non-irradiated samples behave. What is it about the radiation? So is it the radiation damage that changes the nucleation sites? So, I mean, there are other ways to, you know, fix or create nucleation sites. So, I don't know, can we have -- maybe Mac wants to address this, too. I don't know.

I just wanted to make amends about my academic --

Mike, identify yourself, please. Identify yourself.

Oh, I'm sorry. I'm Mike Billone from Argonne National Laboratory. First of all, going back to my comment. The irradiated materials we have are what they are, and they don't match the study you were talking about. So my comment was really directed towards that, that M5 you'll never find 300 ppm of hydrogen in to compare it with the other alloys. Yes, you can do that study with non-irradiated materials, you can learn a lot. Non-irradiated materials have a very ductile metal matrix. So what you would not want to do is do mechanical tests and use that data directly for irradiated cladding.

Secondly is the hydride distribution across the wall of the cladding is challenging to duplicate in the laboratory. People are making progress on it. But there isn't one unique distribution. I showed you really two extreme examples. The Zircaloy-4 we're using were really lower-power rods irradiated for many cycles. And the hydrides are distributed throughout the cladding. They get -- the spacing between hydrides decreases as you go from the middle to the inner surface. But it's a relatively diffuse hydride rim that is characteristic of low power in the reactor.

The other extreme would be the ZIRLO samples I showed you with very dense hydride rim. Those were irradiated for four cycles. And the fourth cycle was extremely high power compared to any other fourth-cycle rod. So, because of the high power, you have a high temperature gradient across the cladding, you're driving more hydrogen towards the end. So let's consider those two extremes. And then the reactor, you have a whole spectrum in between, which makes this problem a lot more complicated. That's worth studying with non-irradiated material, including the study that you said, each alloy under the same conditions. You could learn a lot from non-irradiated materials doing that.

So, you know, our job as a board, if I can continue, our job is to assess the technical work of what DOE is doing. So you're spending a lot of money and time to get very few data points.

Could I follow up kind of in the middle? Just one quick question for you. Have you considered using, say, IM beam irradiation techniques to add the damage in a more modest way and controlled way that would allow you to then handle the samples and characterize them?

We've considered a lot of things, but what makes Argonne unique when we started this program was that we had cladding that was irradiated in commercial reactors where we didn't have to second guess what the damage would be, it was already there. We didn't have to inject hydrogen into the material, it was already there. And so we didn't -- me, personally -- didn't look beyond making a contribution of using what I'll call "real cladding materials" that were irradiated in commercial reactors and testing them. There are a lot of studies throughout the country, a lot of universities, with non-irradiated materials. There's a lot of studies of radiation damage using the IM beam approach. And I think that's better done, to some extent, by universities. It's better for my national lab to do what we can do uniquely, that universities can't do.

Right, but I guess my point would be that you pay a price for working on the actual radioactive material -- that is you pay literally more dollars -- plus you get fewer data points. And if that could be supplemented with IM beam studies, are there other ways to do it, more data points --

I might just tell a quick anecdote about interactions with Bob Einziger that took us a quite a while to get the safety approval to actually run these tests with irradiated material. So we ran tons of tests with non-irradiated material with hydrogen added to it. And I had this big argument with Bob, I said, you know, this stuff looks good. The problem was solved. And Bob said, "Show it to me with irradiated material." And the very first test we ran with ZIRLO there was just absolutely no -- it was very brittle compared to what we had measured with non-irradiated ZIRLO, the same amount of hydrogen, different hydrogen distribution through the material. So the point is -- I guess I was trying to make the point that simulations can be done with reservations as to how you're going to use -- use that for fundamental studies and you use that to establish trends. You would never, in my mind, be able to exactly duplicate the material and rate it in the reactor with any of these techniques. But that's just my own personal opinion.

And other interpretation -- and I think both would be correct -- is that when you see these discrepancies, what that's telling you is that we don't understand the underlying physics and chemistry of either process. But when you can explain both, the simulations as well as what you see in spent fuel, then that's where the confidence comes from in your understanding of the process.

Yeah.

So let's let others speak. Identify yourself.

So, Brady Hanson, PNNL. And it's bizarre that I'm actually volunteering to be up, but Gerry's and your questions are very near to my heart on this. So when we first started on this five, six years ago, we asked the exact same question. We looked at Mike's data, what he and Bob had discussed, and, indeed, the un-irradiated material behaved markedly different than the irradiated. So maybe overly simplistically we said there's two main differences.

Number one is the un-irradiated lacks the radiation damage. We did not think about the techniques you just mentioned, but we did start a program here at Oak Ridge to take un-irradiated cladding, no fuel in it, no anything, and we actually put it in HIFAR so that we could blast it with neutrons. And it took, if I remember correctly, about a year-and-a-half in HIFAR to equal the same neutron fluids you would get over five, six years in core.

Unfortunately, we made a mistake in how we designed it and the samples didn't work out right. We've always been hoping to repeat those, but I do think that that is something very worthy of -- well, as Mike said, I don't want to just say, "Hey, that's something for universities to do," but he's right. Within the DOE budget, because of how limited we are at the laboratory level, we're saying we need to focus on what only we can do. I would love to see somebody propose similar techniques or doing it again in HIFAR to look at radiation damage.

The other main difference, as Mike alluded to and we had some discussion this morning, was what's that effect when I have the hydride distribution the way he describes, this thick rim at the surface and then less concentration throughout? And when you get some that reorient radially but then they hit a circumferential one and you blunt it, it gives you different behavior. So, actually, at PNL, over the last three years, the thing we've been working on is how do you take un-irradiated cladding and, in a low-temperature mechanism -- and we focused on low temperature because the hope was can I take something that comes out of HIFAR and now add the proper hydride concentration and distribution to it without annealing out all the radiation damage I just put in. And I think we're there. We've come up with a way, very crudely, I'll call it sandblasting the surface, subjecting it to a hydrogen gas stream. And you can control the temperature, which controls how thick the rim is and how much total hydrogen you put in. The goal being, I think as you said, by doing these separate effects tests, can I determine is it one or the other, or is it a combination of the two, or is there still some other mechanism that we're missing? But we, within the useful disposition program, haven't been doing that much recently, again, because we're so focused on the spent fuel side, but would love for that to be a NEUP or an IRP or something like that.

Right. So thank you, Mac. You'll have the last word. We're nearly at the end of our time for this period.

I don't want the last word, I just want a word.

Okay.

Mac Louthan, Savannah River. When you say "Can we duplicate," we can duplicate the hydride distribution across the cladding. PNL has techniques for doing that. Savannah River has techniques for duplicating the hydride distribution across the clad. I want to go back to 1960 when we were first looking at this. And we found that if you take a Zircaloy tube -- and we were using tubes about two-and-a-half inches in diameter -- if you take a Zircaloy tube like that and put a little -- we were doing tensile tests. And it was so long ago that when you did the tensile test, you put a gauge mark on the sample. The act of putting a gauge mark on the sample would change the way the hydrides distribute in the sample.

If you take a plate of Zircaloy-4 and tap one end of it, the hydrides will precipitate along the body of the Zircaloy-4 parallel to the surface of the body. Where you have tapped that end and not put enough deformation in where you can see that it has deformed, the hydrides will precipitate so that they're perpendicular to the direction you tapped the surface. If you take a tensile bar with random hydride distribution and pull it to fracture, the hydrides will change in orientation from randomly distributed to all parallel to the way you pulled it.

During irradiation, we put in a lot of crystallographic defects. And I don't believe that we can duplicate the way the hydrides are going to precipitate and the way the matrix around that hydride is going to perform in a non-irradiated material. We can get an awful lot of information. And one of the things I said this morning was I think we basically understand -- we may not understand the specifics, but we basically understand the mechanisms and the things that are going on. And I think DOE is better served spending their money, right now, looking at what happens to irradiated materials than they would be spending that money and looking at what happens to non-irradiated materials. If you want to move -- you said you were an academic. I love to move the science forward. And if we wanted to move the science forward, if that was the purpose, then I'd say we ought to do an awful lot of work with non-irradiated materials and very little with irradiated materials. But if you want to move our understanding of the way the spent fuel is likely to behave -- and I think we're close to there -- then we need to do more work with irradiated materials.

All right. Thank you. And I've had a request for just one sentence, and then we get onto the public comment period. One sentence.

Paul McConnell, Sandia National Labs. I think it's true that there's a difference in the irradiated material because of the hydrides, but all I've heard from Mike, Brady, and Mac is they're talking about behavior with gross plastic deformations, pinches, tensile test. The fact of the matter is that most of the loading is in the elastic regime of the stress-strain curve, and both un-irradiated and irradiated, at least Zircaloy-4, have the same stress strain curve.

Okay, we'll have to stop there. That was --

One clarification [from the audience].

Okay, one clarification, because I really don't want to cut into the public comment period.

Bob Einziger, Board staff. And Paul is right, for normal transportation. But most of the work that Mac and Mike are doing, with respect to hydride reorientations is applicable to accident conditions, and especially the 30-foot side drop where you are getting into the different range.

All right. So my thanks to everyone for the vivid discussion. And now we'll move to public comments. And we have Kevin Kamps. And I'd ask that comments be kept to five minutes, please.

Thank you for another bite at the apple here. So I'll probably submit some comments in writing because I have too many for five minutes. But I just wanted to remind everyone about the quality assurance violations with Holtec containers that Exelon Nuclear Commonwealth Edison whistleblower Oscar Shirani revealed some years ago, and was supported in his whistleblowing by Dr. Ross Landsman of NRC Region III. I just bring this up because it refers back to Mr. McConnell's presentation that seemed to assume structural integrity of the transport containers in combination with the rough road conditions that he was describing. And Shirani warned that the structural integrity of the transport casks, specifically the Holtecs in this case, he questioned sitting still as zero miles per hour. And Dr. Landsman of NRC compared it to space shuttles hitting the ground, launching these materials at high speeds down the rails.

Again, referring to Mr. McConnell's presentation regarding the welded canister, I wanted to raise the issue of security vulnerabilities and the June 1998 test performed at the U.S. Army's Aberdeen Proving Ground with a TOW anti-tank missile fired against a German CASTOR cask. So, certainly, the structural integrity of all those layers of containment would fail when subjected to such an attack.

Again, regarding Mr. McConnell's presentation, the shake tests were all on non-irradiated nuclear fuel, let alone high-burnup irradiated nuclear fuel. So that's a significant thing. And regarding his point about one to two million vibrations per shipment, the phrase "death by a thousand cuts" came to my mind. And I wanted to ask what about the Maine Yankee private fuel storage scenario, and what I mean by that is private fuel storage in the Skull Valley Goshute Indian Reservation in Utah assumed that the waste would move to Yucca Mountain after 20 or 40 years. Then Yucca was cancelled. PFS did have a license from the NRC. So what was the plan B? It was return to sender. And Maine Yankee's good example of what that would look like, over 50 containers, shipped by train, Holtecs, to Utah. That's a 2,500-mile one-way, only to return 2,500 miles back to Maine. So, 5,000 miles of round-trip risk, accomplishing absolutely nothing. And so that one to two million vibrations may have to be doubled.

And I wanted to point out that I hit some incredible potholes last night driving in for this on I-75 in Kentucky and/or Tennessee. So I think they would vie with that photo of the dirt road at the Nevada test site, or if that was at Sandia.

Regarding the Sandia shaker tests, the slides show that they did not use uranium pellets, nor a substitute for zirconium. NRC statement is that they are proposing to -- this is referring to another presentation by the NRC spokesman -- NRC statement is they are proposing to redefine retrievability as the ability to retrieve the canister rather than each fuel assembly. This ignores the DOE standard contract requirement for fuel assembly retrievability. NRC is ignoring this DOE standard contract requirement. Doesn't the legal standard contract prohibit NRC from changing its requirements in this way? Am I out of time?

Your time is about up. So I'd advise you to submit written comments.

Yeah, I sure will. And just this one last point, I want to thank Donna Gilmore, again, for those last two comments I made. And this one actually is from Linda Lewison on the Board of Directors of the Nuclear Energy Information Service in Chicago, and she quotes Dr. Arjun Makhijani, who I references earlier, from Institute for Energy and Environmental Research. And this is in the context of the spent fuel pools at the Zion decommissioning now being dismantled going away, leaving the dry casks with no plan B if something goes wrong. And this is Dr. Makhijani's quote, "It is surreal that the NRC is closing options for the future without even having done a single transfer out of a dry cask to another, with or without a spent fuel pool. Not one." And so referring to Dr. Hanson's presentation about the paint shack with a HEPA filter ad hoc system over a dry cask situation being opened up, let alone transferred, we're really painting ourselves into a corner with these risks, and we would advocate that while the pools are emptied into a safer, still not safe dry cask storage configuration, and we would advocate hardened on-site storage, that the pools really should stay in place, even though empty, to give an option for cask-to-cask transfer, if needed, in the future. They're already there. The money's already been spent. And we think that's a responsible thing to do. Thank you.

All right. Thank you. And, again, I encourage you to submit written comments. Okay. Next is Rob Howard.

I must have signed the wrong sheet.

I wondered.

[Inaudible].

All right. Next, Don Safer.

Thanks for this second opportunity. I appreciate it. I'll keep the comments short. It's been a long day, but illuminating. The ten-year check on the high-burnup spent fuel is important, but I'm also very interested in how the rods will behave over multiple decades, centuries, and the millennia that they must be protected from the environment. And I hope that that's part of the deep repository studies that -- I mean, I haven't had a chance to read the recently released study, but I just think it's key that that be kept in mind. High-burnup fuel has been produced since the 1990s, and it's now being produced in virtually all the reactors. The Nuclear Regulatory Commission has allowed that to occur. And yet it was not until 2012 that the DOE decided to actually start studies. I find that incredible, as a private citizen. You know, I just seem to have a Pollyanna view about how the government should be operating and not subjecting all of us to unknown risks, whether it's genetic engineering or GMO foods or pesticides, or whatever it may be. But, anyway, the results now are going to be ten to 15 years out, according to the study. So that's a big concern.

I want to also just echo the comments about the language that the DOE uses, and to take that a little bit further, for me, as a critic of nuclear power, I found EPRI to be an apologist and as a strong supporter for nuclear power. To have had EPRI be the agency that cooperated and worked with DOE to make this study does not give me a lot of confidence that the study really will be unbiased. Studies can be designed to find whatever you want them to find. And I've seen that numerous occasions in the nuclear world. As citizens, we're left sort of like we are when we go to have anything repaired, where you've got to rely on the expert. The expert's telling you that you need a new car or that you need a new heater system or you need a new whatever it may be.

And so the fact that in that EPRI study all of the cooperating agencies were nuclear AREVA Federal Services, Transnuclear, Dominion, Virginia Power, AREVA Fuels, and Westinghouse Fuels, it just kind of shatters my hope that the study is being designed with an unbiased. And yet, in reality, we have all this stuff, it's not going away, so what would happen if you found, in the study, "Oh, no, this stuff is not going to hold up. What are we going to do with it?" So it's kind of a catch-22. And I know I really shouldn't expect more, but I do expect more. My comments are directed more at the DOE and at the NRC. I appreciate the work of this Board, and I know what you do is not binding, it's advisory, and it's critical.

And the last thing I'll say is that there are several people around the country that have been watching the proceeding. It's wonderful that it's available online. And having some mechanism for them to bring questions in, whether it's even to read them, as happens at some NRC hearings, that a question can be emailed in and then read would be, I think, very useful in the proceedings. But thank you very much.

Thank you for your comments, and also for your suggestion. We'll certainly consider it. This brings us to the end of a very interesting day. I want to thank the speakers and the questioners. This has been, in my memory, one of the more technical discussions that we've had over the entire day, and I really very much appreciated it. So my thanks to everyone who's here. And we'll see you at our next meeting. Thank you.