REALTIME FILE

NUCLEAR WASTE TECHNICAL REVIEW BOARD MEETING
DOE R&D ACTIVITIES RELATED TO MANAGING AND DISPOSING OF COMMERCIAL SPENT NUCLEAR FUEL
WEDNESDAY, OCTOBER 24, 2018
10:00 A.M. EDT

BOARD MEMBERS PRESENT:
JEAN BAHR, Ph.D.
STEVEN M. BECKER, Ph.D.
SUSAN L. BRANTLEY, Ph.D.
ALLEN G. CROFF, M.B.A.
EFI FOUFOULA-GEORGIOU, Ph.D.
TISSA H. ILLANGASEKARE, Ph.D.
KENNETH LEE PEDDICORD, Ph.D.
PAUL J. TURINSKY, Ph.D.
MARY LOU ZOBACK, Ph.D.

STAFF MEMBERS PRESENT:
NIGEL MOTE, EXECUTIVE DIRECTOR
BRET W. LESLIE
DANIEL G. OGG
ROBERTO T. PABALAN
KARYN D. SEVERSON

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(Music playing.)

>> BAHR: Okay, with that traditional introduction, good morning and welcome to the U.S. Nuclear Waste Technical Review Board Fall Meeting. Today's presentations and discussions are going to focus on the U.S. Department of Energy's research and development activities related to managing and disposing of commercial spent nuclear fuel. I'm Jean Bahr, Chair of the Board. I'll introduce the other Board members in a moment. But first I want to briefly describe the Board and tell you why we are holding this meeting today and what we plan to accomplish. As many of you know the Board is an independent federal agency in the executive branch. It is not part of the Department of Energy or any other federal organization. The Board was created in the 1987 amendments to the Nuclear Waste Policy Act to perform objective, ongoing evaluations of the technical and scientific validity of DOE activities related to implementing the Nuclear Waste Policy Act. The 11 Board members are appointed by the President from a list of nominees that are 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. The Board's activities also provide objective technical information to Congress, the administration, DOE, government and nongovernmental organizations and the public on a wide range of issues related to the management and disposal of spent fuel and

high-level waste. Copies of some of the Board's most recent reports can be found on the document table at the entrance to this meeting room and are also available at the Board's website at www.NTWRB.gov. A lot of effort went into planning this meeting and arranging the presentations and I would like to thank our speakers who traveled to Albuquerque today to make presentations. I'd also like to thank Drs. Allen Croff and Paul Turinksy, who are the Board members who acted as Board leads and coordinated with Board staff to put this meeting together. Finally, I want to thank the DOE staff who arranged for some members of the Board to visit the WIPP site underground test area on Monday prior to this meeting. That was a very informative visit. We expect to focus on some of those kinds of issues in our spring meeting next year.

Now I'm going to introduce the Board members and tell you a little bit about the schedule for the meeting. First, the introductions. I'd like to ask that as I say their names the Board members raise their hands so that they can be identified. I'll begin. I'm Jean Bahr, the Board Chair. All of the Board members serve part-time, so, we all have additional jobs. In my case I'm a Professor of Hydrogeology in the Department of Geoscience at the University of Wisconsin-Madison.

Dr. Steven Becker is Professor of Community and Environmental Health in the College of Health Sciences at Old Dominion University in Virginia.

Dr. Susan Brantley is Distinguished Professor of Geosciences and is the Director of the Earth and Environmental Systems Institute at the Penn State University.

Mr. Allen Croff is a nuclear engineer and Adjunct Professor in the Department of Civil and Environmental Engineering at Vanderbilt University.

Dr. Efi Foufoula-Georgiou, is Distinguished Professor in the Department of Civil and Environmental Engineering at the Henry Samueli School of Engineering, University of California, Irvine.

Dr. Tissa Illangasekare holds the AMAX Endowed Distinguished Chair of Civil and Environmental Engineering and Director of the Center for the Experimental Study of Subsurface Environmental Processes at the Colorado School of Mines.

Dr. Kenneth Lee Peddicord, is Director of the Nuclear Power Institute and Professor of Nuclear Engineering at Texas A&M University.

Dr. Paul Turinsky is Professor Emeritus of Nuclear Engineering at North Carolina State University.

Dr. Mary Lou Zoback is a Consulting Professor in the Geophysics Department at Stanford University.

I have just introduced eight Board members plus myself, not the full complement of 11. Due to other commitments, Dr. Linda Nozick is unable to join us today. Dr. Nozick is a professor in the Department of Civil and Environmental Engineering and Director of the College Program in Systems Engineering at Cornell University. And the Board currently has one vacant position.

As I usually do at Board meetings, I want to make clear that the views expressed by the Board are their own, not necessarily Board positions. Our official positions can be found in our reports and letters, which are available on the Board's website. If you would like to know a bit more about the Board a one-page handout summarizing the Board's mission and presenting a list of Board members can be found in the document table in the entrance to this room. And you can also visit the Board site at www.NTWRB.gov and all the Board reports, correspondence, testimony, and meeting materials are available there.

During the meeting, there will be two opportunities for members of the public to make comments. Before the lunch break and at the end of day. We ask that if you wish to make a comment, you add your name to the signup sheet at the registration table that is outside the room. Written comments and other written materials may also be submitted by providing the materials to one of our staff members today or by sending the material by email or mail to the points of contact who were noted in the press release for this meeting. The press release is also posted on the website.

Documents submitted by the public will become part of the meeting record and will be posted on the Board's website along with the transcript of the meeting and the presentations.

If you make a comment during the meeting, please state your name and affiliation first. Please use one of the microphones. I only see one microphone. Use the microphone so you will be identified correctly in the meeting transcript.

The meeting is being webcast live. If you see cameras in the room and depending on where you are sitting you might be part of the webcast.

I encourage presenters to speak loudly enough so those in the back of the room can hear. It is helpful to those watching the webcast if presenters summarize questions before answering them. The webcast will be archived after a few days. It then will be available on our website. To assist those watching the live webcast the meeting agenda has been posted on the Board's website and can be downloaded. The presentations will be posted on the website right after the meeting and will also be part of the webcast. So, you'll see them simultaneously with the talks.

So now I would like to provide you some information about the meeting topics for today.

Over the past few years the Department of Energy has been conducting research and development activities related to the management and disposal of commercial spent nuclear fuel. First, DOE has been doing research to determine the performance and potential degradation of high burnup spent fuel during storage and transportation. In 2014, DOE initiated the high burnup spent fuel data project, nuclear fuel data project. DOE previously presented the results of some of these activities at a public Board meeting held in February 2016 in Knoxville, Tennessee. Since that time DOE has made more progress on its research related to high burnup fuel. Today's meeting includes presentations on the more recent results.

Secondly, DOE recently completed a test program that included transporting spent nuclear fuel transportation cask including fuel assemblies on a journey by truck, cargo ship and train to monitor the stresses that nuclear fuel experiences during transportation operations. We will hear the results of that during today's meeting.

Third, DOE has been evaluating the feasibility of direct disposal of spent nuclear fuel in dual purpose canisters. This is an alternative to repackaging the fuel in two other containers prior to disposal in a geologic repository. We will hear from DOE about the results of previous and ongoing studies regarding direct disposal in dual purpose canisters. Mute your cell phones and let's begin an interesting and productive meeting. It is my pleasure to turn the podium over to Bill Boyle, who will get the meeting started. Bill?

>> BOYLE: Good morning. Thank you, Jean, for the introduction. Just so everybody knows, it is my understanding not all the slides material have been made available yet. So, you won't find any for me. That's because I didn't have any slides. For some of the other speakers, when I came in this morning their slides weren't there yet.

Now, most of the Board members are here, and the staff and DOE and presenters, which I take is a reasonable assumption none of us bought the winning Mega Million ticket. Nice. Here we are today.

Keeping on the topic of money, for the first time in a long time we, DOE and the Board actually have our appropriations for the entire fiscal year. We had it actually before the start of the fiscal year. That doesn't happen that often, but it did.

Then this area related to storage transportation and disposal of spent fuel, the President's budget requested no funding for integrated waste management systems activities. That was more the focus of the meeting in Idaho Falls, but Congress appropriated 22 and a half million dollars and in the area of R&D, some of which you'll hear today, the President's budget requested $10 million, but Congress appropriated in round numbers

$64 million. So, it really does -- it makes for better management to know at the beginning of the year how much money you are going to have. We appreciate it and look forward to doing good things with the appropriations.

The next item I have is the President has nominated a person to be the assistant Secretary for the office of nuclear energy. Her name is Rita Baranwal. B-a-r-a-n-w-a-l, I think. I have never met her. You can Google her. I'm sure you'll find her background. Rose Montgomery apparently worked with her 30 years ago. Rose knows her. I've never met her.

Remains to be -- I think her hearing -- I don't know how many confirmation hearings she may or may not have. The first, perhaps the only one is scheduled the week after the election. So, we'll see how that turns out. And it remains to be seen what particular interest she has in the topics of storage and transportation and disposal.

Then the last item I had before leaving a lot of time for questions from the Board or the staff, is the presentation Sylvia is going to give later today on what I think one of the newspapers when they wrote an article about it, they made an analogy to the Steve Martin-john candy movie planes, trains, and automobiles in the headline, the multimodal transportation activity.

Sylvia and all the other participants, many of whom are here, are very modest and I don't think they have in the slides a recognition that last month the team won a Secretarial award for that activity. So, they are all to be congratulated. And some of us in the room were actually at the award ceremony with the Secretary. Just watching the other teams or people that won awards, something that struck me as possible about the multimodal team, it may have been the only group that won that actually had foreign participation. When Sylvia speaks you'll see that Spain participated and South Korea did as well. And the other nice thing about the team is we even had multiple National Labs participate. Everybody got along.

(Chuckles.)

>> BOYLE: So, with that, those were the only prepared remarks that I had. So, I'm open for questions.

>> BAHR: Questions from the Board members?

(There is no response.)

>> BAHR: Questions from staff?

(There is no response.)

>> BOYLE: All right.

>> BAHR: No? Okay, I guess we're ahead of schedule then.

>> BOYLE: More time for questions on the detailed technical topics for Brady.

>> HANSON: Good morning. Thank you for this opportunity to talk. As Dr. Bahr said in her introduction, DOE has been working on this high burnup spent fuel data project. Some people will shorten it and call it the demo project, which is fine with us. We have been working on it for a number of years. I'm pleased to report we met a major milestone within the last year. My talk will be giving some of those results.

I'm Brady Hanson, Pacific northwest National Laboratory, been doing spent fuel work for 25 years now. I'm very proud of this program that DOE has, the integration that we have that Ned Larson will talk about later and the wonderful results that we have been getting, sometimes very surprising results.

My outline is, I would like to give a little bit of background on the demo. Why did we do it? And what was the process behind it? Discuss some of the thermal modeling. That is one of the issues we identified very early on as a primary data gap, technical gap to understand what are the real temperatures that we have in these types of systems. Then talk about the measurements that we actually got from the demo and how they relate to our models. Talk a little bit about the gas sampling that we took. And then what our future plans are.

By way of background, I want to talk about what we call the low burnup demonstration. Before we even started doing loading of dry cask storage systems here in the U.S., the Department of

Energy and then EPRI worked on joint projects in the early to mid '80s to understand and run experiments to make sure that the models we had of temperature performance and dose on these dry cask systems actually made sense. So, all of these casks that you see in the photo here were loaded as part of that program in the early to mid '80s. I will specifically be talking about the nice green cask there, which is the CASTOR 521. That was loaded in 1985. It has low burnup fuel in it. You can see the range from roughly 30 to 36 gigawatt days per ton. The interesting thing about that fuel is compared to what we load nowadays, on average it was very short cooled. Only about two years to less than four years. As you can see, the assembly heat loads were pretty high, as high as 1.83 kilowatts per assembly. Total heat load of only 28.4. That's a little smaller than what modern systems are loaded to, but because of the high individual heat load and the fact that the older casks were brand new. We were just starting out doing this and so designs had not progressed to the point where they are today. From the temperature measurements, we actually had thermocouples inside each of these casks. Based on those thermocouple measurements we estimate that the Peak Cladding Temperature got to 424 degrees C.

Often over the last years you’ve heard people talk about this 400-degree C limit. That is guidance that the Nuclear Regulatory

Commission has for the industry. A lot of it came from the initial demo where we actually got to temperatures that high. That was basically the end of the test. Through the thermal, through the dose calculations and those casks have been sitting at what is now Idaho National Lab for some time. After a number of years, 14 to be specific, NRC, Department of Energy and industry through EPRI said we want to know what is going on inside. Are our models correct? What is happening? So, they decided to open that green cask in September of '99, after 14 years of sitting there. They pulled each assembly out one by one to examine it, just photographically, see that everything looked okay. They looked inside the canister and the basket to make sure everything was all right.

Then they actually pulled 12 different rods to do examination, three of which were then sectioned and sent to Argonne National Lab where our colleague Mike Billone, who will be speaking later, did some detailed testing.

The long and short from the quote from the EPRI report that you see at the bottom was it was pretty boring. Nothing happened. When they were done, the quote that is often given is: It looked as pristine as it did the day that we loaded it. There was minimal if any cladding creep. Nothing happened that would affect performance. And based off of those results, that is where the Nuclear Regulatory Commission said we have full faith and confidence that loading of low burnup fuel is fine. We have no problems with it. Well, as years went on, we started burning fuel longer and to higher powers, getting to higher burnup.

So, by definition, from the NRC, high burnup is if you have a burnup higher than 45 gigawatt days per ton. So the question arose: does high burnup fuel behave as low burnup? We know that with higher burnup you tend to release more fission gas which could increase the rod internal pressure. You tend to have higher cladding oxidation depending on the alloy you are working with and you tend to get increased hydrogen content.

Again, Mike Billone will be giving much more detail. You can see an example of what the hydrides look like in the cladding. When you go through the drying cycle you have the potential, if the temperatures and hoop stresses are high enough, to reorient the hydrides. I guess that's not showing very well. But into the radial direction. And when you look at it, you say, well, gee, that looks like a crack. You're absolutely right. It tends to act like one. It makes so the material becomes less ductile.

The question is, do we have an issue with high burnup fuel? And the answer we came up with was: Well, let's do a demonstration just like we did before, this time with high burnup fuel.

One thing I want to point out because I think it's very important, the NRC limits your rod average burnup to 62. And there's also some very practical limits here in the U.S. We limit enrichment of the U235 to under 5 percent. And the U.S. cycle lengths, meaning the time in between reloads in your reactor are 18 to 24 months. You combine those things together it tends to limit how high of a burnup you can get. If you look and you hear what the Europeans do and you see burnups much higher than what we report, they are in the 70s, 80s, even greater than 100 sometimes, they are able to do that because their cycle length is only 12 months. Every year they replace some of the fuel with fresh fuel. That allows you to push things harder, if you will.

So, we have very practical limits. The most important thing, if you look at the table in the upper right, this is from what is called the GC859 database. So, in 2013 the department gathered information from all of the utilities. They have to fill out this form that is put together by Office of General Counsel, and say where is all of their fuel, what is the burnup, how many assemblies they discharged. Are they in the pool? Are they in dry storage? All sorts of information. In 2013 was the last time they compiled that data. You can see what the trends in burnup are. It is really not changing much.

Now, that is not to say that some utilities don't push things harder than others, but on average we are just barely at that definition, the threshold for high burnup.

I do want to point out that the Office of General Counsel is currently collecting additional data this year to update this database and we expect probably in the next year to two that this database will be updated and it will be very interesting to see, have we begun to increase the average burnup? But it's important to note when we talk high burnup, we still have an awful lot of low burnup fuel being generated today.

So, in doing this demo, DOE put out a call and issued a contract with EPRI, with a cost share program that was done in April of 2013. The project manager for EPRI is Keith Waldrop here in the audience. I want to recognize him for the outstanding job that he did in leading not only the National Labs but the industry team. They teamed with Dominion Energy, specifically the North Anna power plant. The biggest reason, A, they were willing to do so which we very much appreciate and B, they had four different cladding types that fell into high burnup. That was important to us to look at as many cladding types as we could.

They used the TN32-cask, a bolted lid cask similar to the CASTOR 521. That was also important because one of our criteria is we wanted to open that cask after it sat for a number of years and at this point it is rather difficult to cut open a welded canister and try to do that again. So that is the reason why this cask was chosen.

You can see the burnups that are listed here. We definitely, every assembly is in high burnup region. We want to point out, that is assembly average burnup. So, there are definitely rods that are higher than that. Cooling time anywhere from five to 30 years. The 30 years was to accommodate the two older cladding types, the Zirc-4 and the low tin Zirc-4 which had been irradiated a long time ago. You can see even though it's high burnup because of the cooling times the individual heat load is lower than what was in that low burnup demo cask.

We went through a number of iterations. We tried to get the temperature as high as we could. We wanted to approach 400 degrees C. We went through a lot of modeling with different assemblies to see what we could get. And when we weren't high enough, we actually got so that Dominion was willing to load these assemblies in the middle ring. They were relatively short cooled, only five-year cooled. The sole purpose was to help drive up the temperature as high as we could get.

With the cask like in the low burnup demo, the lid had to be modified so we can put thermocouple lances in, you can see the location in this diagram here where they are. This is an actual picture of the lid. Notice there's two extra holes, if you will. One is for the drain port. The other for the vent port. Those exist in all systems so you can remove the water from the cask.

This picture shows -- it's hard to see but the thermocouple lance being lowered into the cask. This was a design done by Areva. They have used this for reactor operations, they have many years of showing that these things last in high radiation environments and perform very well. Just as importantly a lot of study was done to say where in each of these lances do we want the nine actually spaced thermocouples. And it was decided we wanted to make sure that they were spaced throughout the axial length, but they weren't lined up with any of the grid spacers or flow mixers, so that we made sure we could readily model what the temperatures were.

Just a quick picture to show when the cask arrived at the North Anna plant and to point out kind of the scale. The cask itself is a little over 15 feet tall. A little less than 17 feet when you add in what is called the protective cover. It is about 8 feet in diameter. And weighs, I believe it is about 105-tons fully loaded. This shows the actual loading of the cask. I want to point out that you are looking through approximately 30 feet of water right here. So, there is some distortion. Don't try to take exact measurements of and dimensions of things here. But a few things that I want to point out that will become important as I discuss temperatures.

In this design, you have what is called the shell, which is an inner stainless-steel liner inside this cask. To put a squarish type basket into a round hole, you need to make modifications. So, these that you see on the sides are what we call the rails. Those are attached to the canister wall, screwed in. And there is an assumed gap because it is not going to be completely tight, between those rails and the wall. And then you have your basket, which is where each of the assemblies goes in. Again, there is what we call a gap and you can kind of see that here between the basket and the rail. That will become important later.

This is actually the last of the assemblies going into the cask. What you are seeing here are six poison rod assemblies. We inserted those purely to facilitate what I'll call an early transport of this cask. Since the whole idea is to send it somewhere to have it opened. We wanted to make sure we could transport it sooner than you normally would. So that is purely for neutronics and to satisfy NRC requirements for transportation.

In this picture you can see again examples of those poison rod assemblies. It is hard to see here, but these are funnel guides. So those were inserted into the assemblies where we would be putting a thermocouple lance. Solely when you are putting the lance in through that hole in the lid it would hit that funnel and guide it into the proper guide tube where we wanted it to be.

Just a few pictures showing the cask coming out of the water and the initial installation of the 48 bolts in the lid.

This is now the cask being lowered into what they call the de-con bay. And I would like to spend some time on the timeline that Keith put together for us. Absolutely amazed me. I never had seen this process before. From the time you put in the first of the 32 assemblies until the 32nd, it only took four hours. It very much surprised me how fast they are able to go. And yet the attention to detail, verification, safety, et cetera, still done in that time period. The cask then comes out of the pool. And you will see kind of a delay between the cask coming out and draining. In a normal cask you would start draining right away. In this cask we had to install those thermocouples. That meant bringing them in one at a time and a worker standing on top guiding that thermocouple lance down in, tightening it up, connecting things to make sure it was recording data and all that. So, it took a little bit longer than it normally would because of the instrumentation.

You begin draining. Draining is pumping the water out of the cask. While you do so you fill it with helium so that you are not leaving the fuel exposed to just air. You do have an inert environment that also helps facilitate heat transfer. Again, that draining happens very quickly.

Once they are done with that they did what is called a blow-down. You pressurize the cask and open a valve and that allows any remaining free water to, as the term says, blow down, blow out of the cask. That was done, I believe, 14 times for this cask. And dominion says that that was right in the middle of the range of what they are used to doing. And once when you are doing this blow-down they have a sight glass, if you will, so you can see if water is still coming out.

Once they no longer see water, that's when you begin the vacuum drying process. Again, this one went very quickly, eight hours to pull a vacuum all the way down to, I believe it was 0.4 torr. After you get to that vacuum, you isolate the vacuum for 30 minutes and look at what you pressure rise is. If you stay under three torr, it is considered a successful test and you move on. We remained below one torr even after the 30 minutes. We moved on. Did the helium backfill. That is roughly two atmospheres of helium. And then we let it stay in that de-con bay for almost two weeks. The whole purpose was let it sit there in a relatively controlled environment so that we can see what the temperatures were when they stabilized at steady state. You can see where just a little bit, a month away from reaching our anniversary of that cask going out on the pad.

I want to talk a little bit about gas sampling that we did. So, in this picture you can see that we have three vessels that are hooked up to the vent port. We took the first sample -- by sample, that means filling all three of these vessels, about five hours after the helium backfill. The second was five days later. The third was about seven days after that second sample.

We did three containers each time. This is the vent port. When you connect the quick connect connector up to it there will be a little bit of air trapped in between. We used that first vessel basically to capture that air. It turns out it really wasn't all that bad.

The second sample was then tested at North Anna. Specifically looking for krypton to determine did we have any fission gas release, any leaking rods, looking for oxygen, hydrogen and the biggest thing we were interested in was water. The third vessel was then sent to Sandia where they did the same analyses. Did want to point out, in addition to the thermocouple lances inside, the 63 locations, we also had temperatures that we took on the exterior of the cask. It is very hard to see in this picture. I apologize but these red dots that represent the locations where we wanted to take them are actually painted with these little green dots on the exterior of the cask. This is row A, row B, and row C is out of frame.

But this is the cask sitting in the de-con bay. This is before we actually loaded it. We did not take as many measurements as a lab person would have liked, but we need everyone to understand that NRC controls the dose that people, the workers at the utility receive. They have a budget. You can't go over that budget very easily. So, we took only a few temperature measurements using an IR gun where the worker would stand a way back, aim at the green spots and record them.

After about two weeks in the de-con bay, the cask was transferred to the pad, what we call the ISFSI, independent storage installation. In this case, we had a nice solar panel. We wish we could have powered it by nuclear, but it was easier this way to power the data acquisition system. We collect data. The data is recorded every hour.

Then quarterly, the workers go out and download that data. It gets transferred to EPRI. EPRI puts all that data into reports that then go to the various parties that are interested in it.

So, I want to walk through the thermal modeling and help you understand what is and isn't important and address some of the questions that you have on uncertainty.

When we first started, we were given the decay heats from the utility. They said we are putting 36.8 kilowatts in this. We assumed as NRC had you do, that the ambient temperature would be 100 degrees Fahrenheit. We used all the dimensions and material properties of the cask from what is called the final safety analysis report. This is what the cask vendor submits to NRC and NRC has to then approve and give a license to. By doing that, we came up using what is called the COBRA code, a temperature of 315 degrees Celsius as Peak Cladding Temperature. Not quite the 400 that we wanted but we couldn't go higher because if we tried to, we were actually exceeding the temperature limits for other materials in the cask, namely the neutron absorbing material.

You can see the wide range. This is the peak for the same rod. Much colder, down at the bottom. After we did that analysis, we said, well, wait a minute. We know that one of the biggest conservatisms or biases that the industry has, they calculate the decay heat very conservatively. We had Oak Ridge use their suite of codes to look at the same assemblies, the power histories in the reactor, et cetera. They said gee, if we remove all the conservatism, you're only at 30.6 kilowatts. Everything else stayed the same in the model. That dropped the peak from 315 to 271. You can see how important calculating accurate decay heats are to knowing what the temperatures are.

This is what we call the phase two blinds prediction. Through the EPRI ESCP Committee, they ran a round-robin. This is one of the participants, to say without seeing what the data was from the cask, please do your best estimate of what the temperatures were.

The biggest thing that change, very slight drop in the decay heat. We assumed this would load in July. We actually loaded in November. The decay heat decreased a little bit there. The biggest thing is the ambient temperature, instead of assuming the 100 Fahrenheit it was actually measured at 75 degrees. Again, you can see that big drop from 271 to 258 is almost entirely based on that drop in ambient temperature. Again, the second biggest thing that we need to be concerned about.

So, the biggest question then is: What did you actually see in that cask? What did all these thermocouples show you? This is just showing from the hottest cell what we've got. Again, you can see the locations of the nine thermocouples that are identified here. Thermocouple one is 9 inches above the bottom of the cask. It is the coolest. Whereas you expect in this type of conduction cask, the thermocouples in the middle to be the hottest. Sure enough, you can see four, five, six, and seven are all fairly tightly bound together.

Again, we put the thermocouples in when there was still water in the cask. Began collecting data. You go then through that process of draining the cask, the blow-downs and the vacuum drying. And again, I want to point out that because of that time delay that we had putting in the thermocouples, lances, normally if you are doing a loading campaign at a utility you have full shifts, one right after another working on this. Because this was a single cask we didn't do this. We actually every now and then had a two or three-hour delay before we started the next process. And the reason I say that's important, you can see that the peak temperature recorded at any time on any thermocouple was 237 degrees C. You then see the temperature decrease as you put in the Helium which allows for much better thermal conduction and you get over the two-week period where you actually rapidly reach thermal equilibrium and at that equilibrium our peak temperature was 229.

If we didn't have these delays, I want to point out odds are we would not have had a higher temperature during vacuum drying. It is all very dependent on how long you spend doing something. Industry has gotten very good and very efficient at these loadings. So, whereas we used to always say peak temperatures had to be during drying, that is not necessarily the case anymore. But obviously these temperatures are much lower than the 400 we wanted, even though we tried to get there. They are also significantly lower than the model temperatures.

Just to point out again near the bottom of the cask, so cell 14 is the hottest temperatures measured. Cell 28 is in the corner where you have very good conduction out of the cask, were the coldest and these are the thermocouples down at the very bottom so you can get a feel for the differences. Between about 145 in the hot part to just under 125 in the colder areas of the cask.

So now how do we compare? The plot on the left is all 63 thermocouples. So, starting at the bottom, working to the top of the cask. Identified by the different cell locations. And what we see is the two cells that are in the middle, not surprisingly, are the hottest. The cells that are in the middle ring of that cask, the second hottest. Those that are on the outer ring, these two here, cooler. And then the ones -- you have the four corner assemblies that have a lot higher conduction out are the coolest. So really, no surprise there. But you can see the wide variation of temperatures that you get in a cask where each assembly was pretty close to the same in terms of burnup and heat load.

What we then did here, this is one of the modeling teams that normalized the temperature. So, the data is on the zero line and said how far from predicting the actual temperature were we? And it is very hard to see, but the red is from what is called the COBRA model, and blue is from a computational fluid dynamics model called star-CCM plus. And for the most part you can see for most of the cells you agreed within about 15 to 20 degrees. And from a modeler standpoint, they were very upset when they found that out. They wanted to do much better. They are only happy if they are within ten. There were a couple cells that tended to be the ones in the outer corner where you were as much as 30 degrees off.

What is very interesting to note is that the CFD model actually did better at the top than the COBRA model. And the strength of the COBRA model is predicting fuel temperatures. If you go into how CFD models work, they tend to average out everything in along the length of the fuel assembly. It's good for peak temperature, not as good for axial distribution, but it did pretty well here. I wanted to explain why. There we go.

So, this is a conduction cask. You don't have holes down at the bottom of the basket that allow circulation of air like you do in the welded canister systems. But even then, using the CFD model they said what kind of flow am I getting? You can see that in these rails on the side, not in the basket itself where the assembly is but in these outer rails you are actually getting what is called a thermal siphon effect where in one part of that rail I've got hot air, in this case helium flowing up. In the other part, colder helium flowing down. It actually turns out that kind of not surprising, these cells in the corners that are touching that rail, those are the ones where the CFD model did a better prediction at the top showing that having the small amount of flow does indeed impact the temperatures. That is one major thing we learned.

The other thing is, again I said the modelers were very upset that they weren't closer. So, they did a sensitivity study and said gee, if I increase basket emissivity, what does that do? Not surprisingly, radiative heat transfer is a function of temperature to the fourth power. When the textures are relatively low like we had it doesn't have much effect. That's why it had a 2-degree C difference.

If you close that, again this is part of the rail. This is the shell. If you say I had perfect conduction there, that still is only changing things by 2 degrees. We played around with decay heats and said gee, we have to be 10 percent off to help explain the difference between model and measured. We don't think we were. One reason we can say that with a fair amount of confidence is, when you compare the measured temperatures on the outside of the cask with the model predictions, those were typically within 5 degrees C. The reason you can do that, the outside temperature is only dependent on how much heat do I have on the inside. What is my heat flux out and what is my outside temperature?

I don't care about conduction paths and all that. Whatever heat you have had to get out. It becomes a relatively simple calculation and the outside temperatures matched, which to us said that Oak Ridge did an excellent job in estimating what the real decay heat was. So that didn't make much sense to us.

The next step was, as I showed you in that previous picture, the gap between this basket and the rail. We took those values from what the vendor submitted to NRC in their license application. And that number is proprietary. I won't say exactly what it was, but we said what if that gap actually gets smaller? Which it will because the basket has a lot of aluminum in it. As you heat it up it will expand and actually then touch the wall. That is especially true in these middle regions where the basket wall thickness is twice what it is everywhere else. They said if I keep reducing that gap, this is what happens and you can see how sensitive the temperatures are.

What I want to point out and what I hope we as a group and industry and everyone can agree on, we need to distinguish between what is an uncertainty and what is a bias. And I realize that when industry submits an application to NRC, the only thing they are interested in is making sure that their temperatures stay below the regulatory limits that NRC sets.

So, to do that, it is easiest if you purposely pick parameters that will make your temperatures higher. Things such as the conservative decay heat, having a larger gap between your basket and rail. So, I like to say that the reason why all of the temperatures were higher in the model is because we were using parameters that bias it high. Now, I still have some uncertainty because this basket being made out of aluminum will expand differently, depending on what the localized temperature is. As we saw, the temperatures are hotter in the middle part. That middle part is going to expand, close the gap more than the upper and lower ends will.

So, I still have uncertainty in the models we pick a single gap to represent the entire circumference and entire axial height.

That really isn't true, and that is an uncertainty. However, if you look, the black line is the actual measured data. The gray is if we use that industry supplied gap width between the basket and the rail. And then the green is if I close it -- excuse me, the orange, if I close it a little bit. The green if I close it a lot. You can see for most of the cells, I can make so that the model falls right on top of the measured temperature. The one exception again is out here in the very corner cells where you now have a little bit of convection going on with your conduction. This model does not account for that. So, taking that into account and saying we are going to assume that those gaps have closed, we ask the modelers to say okay, rerun your codes. What do you get? That's what we are calling the adjusted best estimate. You can see that we've now dropped down that at steady state, the peak temperature is 238. And that compares relatively well to the 229 that we actually measured. As I said, modelers are pretty happy if they are within 10 degrees C.
Again, using this normalized curve, you can see that for all but that one cell in the corner, we are now within that ten-degree region. We feel pretty confident about that.

The other big thing, so I did present this to the Board at that last meeting that Dr. Bahr talked about. It is important to realize that with these distributions, when we talk about what are the material properties' effects, that you realize because I have such large temperature distributions, my cladding, you can see my peaks, I have a number of assemblies that are above 230 degrees C. Yet this is -- sorry you can't see, 230s to 240 right there. There is only two and a half to 3 percent of the entire amount of cladding in this cask are at those temperatures.

So, you can see the bulk are much, much cooler. Therefore, when we talk about things such as creep or hydride reorientation you need to realize that even if I say yes, my cask reached a peak temperature of 400, only a small fraction will be at that amount. All you would do is shift this curve to the right. You would still have a very small fraction in the 400 regions.

To summarize, this is put together by the EPRI ESCP thermal subcommittee, Al Csontos is the EPRI ESCP co-chair. Sam Durbin, who will follow me up here as the other co-chair.

The FSAR is what the industry submitted to NRC. They said we're gonna have a decay heat of almost 37 kilowatts which is substantially higher than what that cask was originally rated for. We are going to assume that 100-degree Fahrenheit ambient temperature. When they did they said the peak temperature would be 478 degrees Centigrade. The problem was the temperature of the neutron absorber was too close for comfort for NRC and industry. They went back and sharpened their pencil and said let's remove some of the conservatisms from the decay heat and dropped it below 33. They said let's look at 50 years of temperature data at that North Anna plant. We don't ever have an average temperature of 100. We are going to drop it down to 93.5. In doing so, that dropped your Peak Cladding Temperature by 30 degrees C.

I want to point out that the modelers that were done for what we call the phase two round-robin were predicting temperatures in this range, using this decay heat and this ambient temperature, whereas the actual measurement was this. Again, not accounting for this closing of gaps, things like that. We were biased 30 plus degrees high. But when people say, well, gee, don't you then have a problem? Again, you need to realize that from an industry standpoint, even though we know the temperature was only this, because they licensed it at this temperature and for that matter really at this one, that is the temperature they go by. You need to realize that every time we report for a loaded system, this is what industry does. They use these conservatisms. So, when we are saying yes, this cask was close to 350, in reality it's much less than that.

Just a quick word on the gas sampling. So, we did, DOE has funded for the last three, four years an independent research project at the University of South Carolina. They were teamed with Areva, did some excellent work, looking at drying. Their final report should be out pretty soon. The nutshell is they saw no detectable water except when they tried to dry a simulated failed fuel rod that left a whopping 5-milliliters of water behind. From the gas samples we took from the demo cask we are still in the process of evaluating. Samples were analyzed at North Anna. We just completed running those calibrations about a month ago. And still going through that data. And actually, Sandia just completed their calibration last week. So, we still have to analyze that data, do temperature and pressure corrections, all kinds of fun stuff like that.

We should have the actual data on what was the moisture. We are hoping definitely by the end of the calendar year, but again we need to get both teams together, see what the numbers say, see what makes sense and put that story together.

In summary we think our models are really good. We think we understand the physics of what goes on in dry cask systems. The modelers say they have full confidence they can accurately predict temperatures, as long as the inputs that they are given are good. And so, there's a big difference between saying is your model good? Or is your input off? And I would argue that based on what we've seen, we know that people tend to make conservative inputs to the model. We know those are mostly from decay heat. That's the most important. Ambient temperature second most important, and these conduction gaps, also right up there. When you talk about do I need to know or do I need to test horizontal systems, a horizontal system is pretty much a conduction system. It becomes easier. When you have it on the side, you know that your assembly is touching the wall of your basket, whereas for a vertical system we always assume that the fuel assembly is perfectly centered within the basket and not touching. If it is touching that will lower your temperature because you will have much better conduction.

That said, the next speaker, Dr. Sam Durbin, will discuss results from what we call phase one and where we want to go with phase three, to try to prove that on convective systems we have as good of an understanding as we do on conduction. We are working very closely through the ESCP thermal subcommittee which has international participation. Sam will talk about some of the international groups who are modeling the experiment that he ran to help us address these issues, but the point is how well do I need to know temperatures? The answer is it depends. The closer I am to an actual temperature limit or threshold or some sort of material degradation occurs, then I need to know it better. If I'm down in this below 250 degrees C, we know we won't have the hydride reorientation or cladding creep. I can have a lot more uncertainty or bias, if you will, in my models.

I want to point out and Mike will discuss this in his talk, it really looks like hoop stress. That is, what is the pressure inside that rod that exerts this hoop stress on the cladding? Is becoming far more important to know than knowing what the temperatures are. And both Rose Montgomery and Mike will discuss that in their talks. What we are finding out about rod internal pressures.

We still have to quantify the amount of water. We can't do that now. Qualitatively we can state what is happening.

In terms of future work, most of this is going to happen this fiscal year because of the good budgets that Bill Boyle discussed. Sam will discuss completion of the modeling efforts for phase one, EPRI is supposed to be releasing fairly soon the results of phase two, which is this demo cask, round-robin thermal modeling. We expect that within the next two to three months.

We will also perform transient analyses. When I pointed out those temperatures when we first put the thermocouple lances in, for a system like this, what modelers often have done is say I don't know how long that cask is going to sit with water in it before I start drying, so I'm going to assume that my cladding is right near the boiling point. 100 degrees C, before I even start doing my vacuum drying.

As you saw, we were significantly below that. And that leads to much lower temperatures during your drying process. So that's one of the biggest things we learned from this demo is just how cold things are when you start. We will be looking at horizontal configuration. Again, Sam will discuss that. As a group we'll get together to say do we need to look at other fuel types, BWR versus PWR, do we need to look at the vertical convective systems? We will have discussions within industry and with NRC to see how that goes. As I said, we are almost ready to quantify the amount of water that were seen in these gas samples from the demo.

The DOE program is supporting, along with that, a comprehensive analysis that was asked for by both NRC and the ASTM international committee on spent fuel to look at consequences. We really don't want to get people spun up. The fact that water remains after drying should not be a surprise to anyone. I think many of you probably use tools at your universities such as XPS or Auger spectroscopy. If you work with anyone who works with those, you pull very hard vacuums on those systems, heat them up and you still will have monolayers of water there. We are not getting close to that in these systems.

Like I say, it is no surprise at all that there is some water. The biggest question is: How much water does there need to be before you even care? So, going along with quantifying what there is, we want to do a detailed assessment of at what point do we care? At what point does it become potentially damaging to any of the components. We issued a follow-on call for an IRP to build on the one that University of South Carolina did. And we will also be conducting small-scale tests where we know exactly how much water is in a system to start with, vary the temperatures. So, we have differences between saturation and super-heated conditions, take samples and make sure that as a community we can relate what is in a gas sample to what is really in the canister. So that is work that will be going on this FY.

Then finally, what is going on with the demo? Well, it remains on the pad. As I said, data recorded hourly, and quarterly will be collected, put together by EPRI and distributed.

There is a slight change to our plan. We are not planning on taking any additional gas samples while the cask is on the pad and working with Dominion Energy, we agreed that it is just too risky to keep connecting something to that vent port while we very much want to know the results, the risk of having, breaking that valve, having an uncontrolled release outside, that could go off site is not worth it. So, we won't do another gas sample until right before we transport that cask. We will bring it back into the fuel handling building and do a sample then.

But we are looking, working with EPRI, to see can we take gas samples from other systems right after they have been dried and backfilled. And we are so far getting some positive input from industry take we might be able to do that.

DOE, I defer to Ned Larson for any questions on this. We are actively exploring where can we ship this cask after ten years. It is a combination of who has the facilities, but as you are aware it is also a political hot potato of will a state allow this cask to come in? We are looking at multiple options for that.

The last one to me is the very important one, the sister rod testing is expected to bound the behavior of the rods in the demo. The reason is, Sylvia led a team to put together what we are calling our phase one testing. And because the temperatures were so low, 229 degrees C in this demo, we said that is going to be boring. If we spend all this money looking at hydride reorientation at that low a temperature, we know what the result is going to be. We were purposely doing all of our sister rod testing both at Oak Ridge, at Argonne and at PNL where we are taking the temperature up to 400, but we are using the same rod internal pressure as what we are measuring now. The advantage is by going to 400, I'm going to a higher temperature. I'm also going to a higher internal pressure.

By that, we feel we will be bounding any of the higher burnup fuel that is currently in dry storage systems. So, the good news is we think that using that data, industry will be able to address what few questions NRC still has on higher burnup behavior.

With that I'm done. I think it is best, Jean, if Dr. Durbin goes next so that all of your questions come together. But up to you.

>> BAHR: Well, I had a question about the drying procedure. So, it looks like the temperatures are increasing during that vacuum application.

>> HANSON: Correct.

>> BAHR: So, if you kept the vacuum on longer the temperatures would have gone higher, correct?

>> BRAD: Correct.

>> BAHR: What determines how long you keep the vacuum on that? Is there a specified time? I think you specified eight hours, I think.

>> HANSON: The real criteria are NRC says we want you to have a pressure below three torr for at least 30 minutes. So different vendors and different utilities have different methods, but typically you will pull a vacuum down. Once you are below that three torr, you now turn your vacuum pump off a look at what your pressure does for that 30 minutes.

>> BAHR: Could you wait until it gets to 2.9 or take it down to 1 or .5?

>> HANSON: It is variable and depends on the vendor and utility. In our case here, the vacuum went down very rapidly to .4 torr. And that is where we decided, okay, let's valve off.

>> BAHR: Then you only keep it closed for 30 minutes after that. So, if it's monotonically increasing, it could be approaching the three torr. If you waited longer it might go above that?

>> HANSON: That's correct.

>> BAHR: Does it continue to heat during the closed off period?

>> HANSON: It does, so that is one of the issues. The pressure will definitely be going up during that time just because of the increased heat. Having a vacuum, things get a lot hotter because they don't conduct heat very well. So, you need to weigh that. How much of that pressure increase is from temperature only versus anything else, but the guideline is, as long as you are below 3, if you got below 3 at any point, the argument is that you have below the threshold limit of the oxidizing species, whether that is remaining oxygen or water, or whatever, that you won't create any degradation issues.

>> BAHR: It sound like there's a lot of variability in that process and that would mean there could be a lot of variability then in the peak temperatures that casks see, just as a function of how they do the drying process.

>> HANSON: Very true, yes.

>> BAHR: Even though in this case you didn't get to those high temperatures, if people follow different protocols, it might have gotten hotter. Is that correct?

>> HANSON: Definitely can. Each system has their own what we call technical specifications. And in almost all cases it is things such as if you did not reach the desired pressure within a certain time period, usually it's less than 24 hours, then you

have to backfill the cask with helium and start over again. So, there are procedures and rules in place so that you don't ever just keep it under vacuum and let it keep going.

>> BAHR: I guess you've got one specific case here and one set of temperatures. I'm trying to get an idea of how typical those really are going to be given some of that uncertainty in procedures.

>> HANSON: I would say probably the biggest uncertainty in my opinion would be how long are you at any one step. How long am I pulling that vacuum? And I don't think it is as much procedure to procedure as did something happen that caused you to be shorter? I believe two and a half years ago when I spoke at the meeting in Knoxville, I showed some of the data from industry and again all I can say is they have gotten so much better in the last decade that they are well below the times that they are allowed to get to. So, you're right, I could have higher temperatures than I do here. In some systems I expect that. But we are still not -- the statement that we feel fairly confident in where again Oak Ridge has developed this UNF standards database where they looked at the input from that GC859 report and as modeled, what temperatures they expect in any system that has been loaded, and so far, we feel pretty confident to say that the Peak Cladding Temperature that we calculated is about 325

degrees C. Yes, hotter than this, but still substantially lower than the 400 limit.

Again, we are going to test sister rods to 400 to cover anything that either has or in the future may get to that point.

>> BAHR: Dr. Peddicord.

>> PEDDICORD: Am I on? Ken Peddicord from the Board. First of all, this is excellent, Brady, this is interesting stuff. But I want to ask you to help me understand a couple of details on this. Pretty simple question. Maybe it's got a straightforward answer as well too. In the temperature profile you showed on slide 16, which is one of the ones you were benchmarking against as well too, that I think it's really excellent that you moved to making it much more reasonable assumptions on things like ambient temperatures and so on.

But my question is that using the marvels of technology it turns out that this morning the temperature in Mineral, Virginia, is 52 degrees, not 75. And so that's 23 degrees below the assumed temperature.

Now, maybe again you were still inside the facility, but I wonder if you could go back and actually use the actual temperature of the environment in which the cask was placed. The end result of this is maybe as the modelers are beating themselves up over the wrong thing. Here is a place where you can use real measurements to calibrate against the actual temperatures you made and the models as well too, put that number in the model.

So where in all this does this come down?

>> HANSON: I appreciate that comment. You're right, the answer is pretty straightforward. Number one, if we had loaded in July of '17 we would have expected the temperatures to be much hotter. That would have affected what the Peak Cladding Temperature were. If we had loaded in January, it could have been cooler.

>> PEDDICORD: You loaded in November. What was the temperature in November?

>> HANSON: It was probably, outside temperature was probably in the 40s, but again one of the reasons why we left it in the building was to allow us to get to that thermal equilibrium without too big of swings in the ambient temperature.

>> PEDDICORD: What was the temperature in the building? What was the ambient temperature?

>> HANSON: The 75 degrees C, sorry I didn't mention, that was measured with a thermocouple down in the de-con bay. It was about 3 feet away, I think it was, from the cask. So that 75 was the ambient temperature in the building at the time.

>> PEDDICORD: Okay, thank you.

>> HANSON: But to go where you were going, one of the deliverables that we will have at the end of this fiscal year is the modelers will take the data from one year's worth of the cask

sitting on the pad and see how do the temperatures track, not only with the diurnal variation in ambient temperature but the long-term. As I heat up in the spring, how long did it take for the fuel temperatures to track? So, it will be a way of determining, I guess the proper term is the thermal mass of the system is and what is the lead and lag time. We will be doing that. Like I say, that report is due the end of FY19.

>> PEDDICORD: If I may, a follow-on, you talked about in slide 8 trying to intuit some of the things like the gaps here, like the water. It's a tough one and really probably hard to get your arms around an accurate measurement.

But that gap is really quite key in the thermal models.

>> HANSON: Yes, it is.

>> PEDDICORD: Is there a possibility, even looking at the pictures it looks like it's asymmetric. The gaps are not the same on either side. Were you able to get a handle on what the actual gaps might be that the cask has when you were again doing these measurements and trying to predict against it?

>> HANSON: We did not in this test, and I would say it would be very difficult to do on a real system. So that's actually something that I'm going to ask Sam to make note of when we are doing our future testing using just heaters so it's not radioactive that we can look at exploring how to do that better.

>> PEDDICORD: Thanks.

>> BAHR: Paul?

>> TURINSKY: Can't you just do a coupled thermal hydraulic structural analyses and calculate the expansion of the system? The answer maybe it totally closes. So, do you have any plans to go in that direction?

>> HANSON: You're absolutely right, that can be done, realizing that -- I mean, you can't just do it uniformly. It will be a function of the circumference around there; what rods are hottest. And it will vary axially, so it can be done.

>> TURINSKY: I know the assembly position within the grid is going to --

>> HANSON: We hadn't thought about doing that. Mostly because, like I say, once they assume that gap closing and got very good temperatures, like I say we have pretty good faith in being able to model. Sam will discuss the tests we'll do to look at internal convection. Once we've done that, you know, the question really becomes how important is it to decrease this uncertainty or bias. If we are nowhere near degradation temperatures, it doesn't matter too much. And I think we'll probably expend resources elsewhere.

>> TURINSKY: Yes, but I almost think you can do an approximate calculation and answer the question, are we going to get firm contact or not?

>> HANSON: I know that at least one of the modeling teams looked at that. And when they assumed the gap closing, it was based on the thermal expansion of the aluminum. But again, it was assumed uniform throughout.

>> TURINSKY: Okay. A second question is, you mentioned future work being looking at the consequences of having water. Can you say a little bit about the experiment you are thinking of doing? Because the time scales involved are ...

>> HANSON: Right. So, the consequence report is a separate item. You may know that a few years ago the NRC had the Center for Nuclear Waste Analysis do a model for them. And they fairly arbitrarily said we are just going to pick up to 1 liter of water remaining. And the results were, yeah, if you are below 1 liter there's no issue. It is incorrect to assume that if you are above the liter you will have an issue. They did not analyze that.

That's part of what we are going to do in the consequence analysis.

For the second part that you mentioned we are looking at just doing a small-scale test where we are controlling the wall temperatures and the axial profile, if you will, to simulate a fuel in there. But introducing known quantities of water. So, to make sure that when we take a gas sample and analyze it, we understand what is the uncertainty and how do I relate this back to what is actually in the cask.

As you can imagine, think of it this way. When we took the gas sample, we were at right around two atmosphere of helium pressure. Average temperature, gas temperature is estimated to be about 150 degrees C. And when you take that and it goes into those sample containers that I showed you, they are at room temperature. I'm immediately going to cool down and whatever water I have is going to start to condense, not necessarily as a pool of water but physisorbed on to the wall.

Sandia in their testing has been making sure to heat up those gas cylinders to dry to drive off that physisorbed.

The simplest answer to your question is, we want to make sure on the small-scale tests we do an entire mass balance on the water. We know what we are starting with, so when we measure it, how do we account for physisorption, chemisorption, et cetera, and make sure we understand what is going on.

>> TURINSKY: Are you going to look at the material consequences on the fuel and the basket?

>> HANSON: That's part of the consequence analysis, yes. Our report will be similar to what the Center did for NRC but just expanding it to say, okay, at what point does -- how much water can I have before cladding degradation or degradation of the aluminum basket, things like that.

>> TURINSKY: Is there material data now available to do that?

>> HANSON: For most things, yes.

>> BAHR: I think we'll take one more question from the Board but we have a second -- two more questions, Efi and then Steve. We have the second part of the presentation and we'll have more time for questioning both Brady and Sam after that. Efi and then Steve.

>> FOUFOULA-GEORGIOU: Yes. Model diagnostics are never easy, but some models are more consequential than others. I appreciate all the work you presented here.

My comment is the following ... you presented the sensitivity analysis to two external parameters, which is fine, but you also convinced us that through the comparison of two different models that have different physics, the COBRA and the fluid dynamics, it revealed some consequences. Which are within the error, within the bias. Meaning they were significant differences.

So now if you get outside the standard domain and you go to what you call critical conditions, you have thresholds coming up, material degradation, nonlinearities piling up. Is it going to be the external parameter that will mostly contribute to the model sensitivity or uncertainty? Or they will be more the internal parameters and dynamics that you have to be more careful?

And do you have a program that basically focuses on exactly that issue under critical threshold conditions? What matters the most?

>> HANSON: Excellent question. So, I think that is one of the things that we want to examine during this consequence analysis and really link the thermal work to materials degradation and understand how tight do I need to be before I'm concerned about this? At which point hopefully then we can answer your question of what matters more.

At this point we can't, but that's an excellent point for us to include as we move forward.

>> BAHR: Steven Becker.

>> BECKER: Thanks for a very interesting presentation. The table with burnup data early in your slides, I think you noted only goes up to 2013. But if I heard you correctly you also said that you were expecting more recent data soon. I'm wondering if you can say a few words about how that information is collected and how often.

>> HANSON: Okay. The process used to be done under what was called the RW859 database. So, for when Yucca Mountain was -- Yucca Mountain was still alive and going, the department said we need to know what fuel we are going to receive, what is the burnups, the cooling times, things like that. They developed that 859 database.

It went dormant for a number of years and then when RW was split apart, the job of collecting that data went to General Counsel, which is why it is now called the GC859 database. And the thought is there is no set time to say how often they are going to update it. Like I say, it has been about five years and they are in the process now. But it is a very formal process between the department, where they send out this very detailed questionnaire to each of the utilities. The utilities then fill this in and send it back. It goes to EIA. They compile it. It becomes publicly available. Like I say, that's what Oak Ridge uses to do their UNF standards calculations.

>> BAHR: Okay. Let's turn it over to Sam Durbin who will continue this.

>> HANSON: Thank you.

>> DURBIN: Okay. I'm good? Okay.

Welcome. As the first local to present, let me welcome you to Albuquerque on this beautiful day. Any day that we have precipitation in the desert is a beautiful day.

I'm going to be presenting to you today on a complementary and supplementary project to what Brady has already described. We call it the dry cask simulator. It looked at a single BWR assembly inside of a simulated cask. And so, we have two different configurations that we studied, both an above ground and below ground configuration. And just to cover another temperature scale, we are going to be presenting all of our results in Kelvin. You've seen Celsius and Fahrenheit mixed in, but we will shy away from Rankin.

The purpose were to validate assumptions in computational fluid dynamics used for spent nuclear fuels thermal design analyses. I will show some evidence that by using a single assembly we have been able to mimic conditions that are still prototypical validation. We were able to measure temperature profiles for decay heat powers and helium cask pressures and we were able to with some simple modifications perform tests that were valid for above ground and below ground configurations.

So, I would be remiss if I didn't point out previous studies. The Castor V/21 mentioned by Brady earlier looked at unconsolidated, unpressurized, unventilated storage as well as the REA cask and VSC17, heavily utilized for validation efforts in the past as well as some small-scale single assembly tests, FTT, SHATT and the Mitsubishi tests done in the mid to late '80s. For all three studies, these were unconsolidated fuel with controlled boundary conditions. The wall temperatures were controlled. They weren't studying the natural air convection on the outside of the canister. They were also unpressurized. None of these systems in our judgment were appropriated for the elevated helium pressures that you find in modern ventilated vertical canisters.

So, the current approach, we simplified by going down to one fuel assembly and were able to put this inside of a simulated canister. Again, looking at above ground and below ground configurations. For below ground we added in an extra wrinkle with some cross-wind conditions.

Because it was not spent fuel, it was an electrically heated assembly we were able to take advantage of that and directly attach our thermocouples to the cladding. And we were able to instrument with a slightly higher density than was available for the higher burnup demo in phase two.

I should take one step back and say that colloquially this is known as phase one. What Brady was presenting for the actual spent fuel loading at North Anna was phase two.

Here you see in the pictures, these are the canister components. This is the canister. It is actually a 10-inch pipe. This is the basket that we had made, as well as the pedestal and base plate. This T which attaches to the bottom of the assembly is an instrument well. This is how we funnel our thermocouples out of the assembly. Over here on the right you can see this is a cross-section looking down into the assembly. Here we have the BWR fuel assembly with the electrical heaters, kind of a blue color. That is the channel box.

This next structure, this is the basket. And this is the canister or the 10-inch pipe that forms our pressure vessel.

So, this is also a convective system. The helium comes up through the fuel and heats as shown by red. It gets to the top of the assembly and it goes down. This down-comer marked in orange. Externally this is where the air circulates up and removes the heat from the system.

These are our prototypical components, BWR 9 x 9, the most common in the U.S. fleet. We have the top tie plate, bottom tie plate, channel box, water rods and then the grids. We also had eight partial length rods that are about two-thirds of the length of the assembly.

So, the fuel was discretely represented by heaters. And inside of that fuel we attached approximately 100 thermocouples, 97 to be exact and an additional ten thermocouples mounted to the channel box, seven to the outside and three to the inside.

And then we also instrumented additional thermocouples to the basket and the pressure vessel inside, as well as on the outside.

This gives you kind of a pattern arrangement for the thermocouples inside of the assembly at different axial levels.

Throughout the assembly we had a high density of thermocouples attached to the cladding of the heaters.

You might be asking yourself, well, why is one assembly valid for the studying of these types of systems when in an actual system you might have 68 of these fuel assemblies. The simple answer is dimensional analyses. We took a look at the dimensionless groups, namely the Reynolds number, the Rayleigh number and the Nusselt number. We calculated those for this system. It turns out we have scaling distortion with decay heat. We have to crank up the decay heat to match more closely with the prototypic system. This is the dry cask simulator for low power 500 watts. This is the dry cask simulator for applied power of 5 kilowatts. This is a prototypic system here, 37 kilowatts. If you compare these numbers across the table you can see prototypic is about 250 for the high-power case we can get up to about 190.

This is the Reynolds number in the downcomer, the orange region. For Rayleigh number we can bracket the Rayleigh number by exploring decay heats in this range as well as the Nusselt number of 200 for the prototypic. We are in the ballpark for that dimensionless group.

For the external dimensional analysis, we are showing the air region in green. If we look at the Reynolds number in that region, prototypic cask would be about 5700 we can again bracket that number and for the Rayleigh number in the annulus we are able to get close, 2.3 times ten to the eighth and for the low power we are almost on that, 2.7 E to the 10th -- sorry, E to the eighth.

This is a scaling parameter often used for the convection on the outside of these types of cylindrical systems. You can see we are a little bit higher than that. On the Nusselt number we are still in the range of interest.

So, this is the above ground configuration. On the left you see this is a vertical cross-section of the assembly. The technologists are messy, like my kids, they left a football and soccer ball in there for scale. This is the instrument well. The dashed red line that you see marked on the assembly, that is the pressure boundary. That is where we have control of the internal atmosphere of the vessel.

We have high temperature insulation down here at the bottom. We have roughly adiabatic condition for the boundary modeling conditions.

I'll walk you through this. The helium comes down on the outside of the pressure vessel -- inside of the pressure vessel, I should say. It comes down, goes up through the assembly, picks up the heat, goes to the top, turns around and recirculates down towards the bottom. On the outside of the pressure vessel we have air inlets that allow cooling air in through the bottom, up through the annulus and out through the top. This is what it looks like in isometric view. We have tighter control, I would say, than say an industrial system. We can vary the decay heat. We can vary the internal pressure of the vessel and study those things independently.

We measure the air velocity at the inlets. From that calculate the external mass flow rate.

All the testing for this type of configuration was completed in August of 2016. We have 14 data sets. We are currently in validation exercises with staff at the NRC, as well as in Spain. PNNL, I'm not sure what the progress is right now but they do have plans to validate against this system and report that in the upcoming fiscal year.

So here is a snapshot of, this is Peak Cladding Temperature on the top graph and air mass flow rate on the bottom for different decay heats on the assembly. Here on the different symbols you can see these are internal pressures. This is .3 KPA, 100, 450, 800. These are all absolute.

So, you can see as you apply more decay heat to the assembly, the Peak Cladding Temperature goes up. Here we have an elevation in Peak Cladding Temperature for the low-pressure case because of basically the vessel was leaking air into the canister. Rather than having a helium backfill for these conditions we have an air backfill.

And this is an example of the instrument density that we have available. So, these are temperature contour plots, which I normally associate with computational fluid dynamics. Because we have such a high density of instrumentation we are able to produce pretty graphics like this with high level of fidelity.

This is a comparison between the testing and computational fluid dynamics performed by the NRC. So here you have the experimental results on the dependent access and the CFD results on the independent axis. If you have a perfect agreement you would be on this 45-degree line running right through where I'm pointing at. This is a Peak Cladding Temperature on the top for three different pressures, 100, 450, and 800 kiloPascal internal helium pressure.

On the bottom this is mass flow rate, experiment on the vertical and CFD predictions on the horizontal. You can see overall there is a very good agreement, almost within the experimental uncertainty for most cases. We did have a couple that perhaps not for -- this is the above ground configuration, but for below ground, there were a few data points outside of the experimental uncertainty by a hair.

Overall this is a very good agreement and I think it's an example of the input deck that Brady mentioned. When you have really good inputs into your model, the physics that are captured by the models do reflect what is going on inside the system.

This is the below ground configuration. We added another annular shell. The air comes in the top, goes down, makes 180-degree turn and goes back up towards the top.

This testing was all completed in April of 2017. We have another 14 data sets recorded for both transient and steady state. They are available for validation. NRC staff have performed validation exercises. I believe it is their intent to capture the lessons learned from all these validation inside of a NUREG, which would be a best practice guide for industry.

This is the same summary style graph here. Peak Cladding Temperature on the top graph, mass flow rate on the bottom both as a function of decay heat for four different pressures internal to the pressure vessel and we were within 2 percent for the Peak Cladding Temperature and within 5 percent for the mass flow rate on the model comparison.

And that is shown perhaps better here in this slide. Peak Cladding Temperature experiment here on the vertical, Peak Cladding Temperature CFD on the horizontal. Again, if we had perfect agreement we would be falling on the black line. You can see that we tend to get a little bit lower at the higher decay heats, but still pretty good. Certainly, within the 10 degrees that Brady described earlier.

Then on the mass flow rate, you can see the dashed red line is the least squares regression of the experiment compared to the CFD. It does show that there is an under prediction in the CFD, but we think that is probably due to the way that the annulus was constructed. We have flow straightener element in there that was perhaps compressed. We were measuring asymmetries inside the annulus. Once that is accounted for in the model, this gets better. But again, it is an instance of having good input conditions to your model to know accurately what the behavior of the system is.

Here you see a picture of the facility and the apparatus. The apparatus is here in the middle. And it is in the below ground configuration in this picture. Here is a cross-section and isometric view of the same system. The big green box you see here with the yellow hoses attached, this is a wind machine. We were using pneumatic blowers to introduce a uniform air pattern across the inlet and outlet of the system. Over here you can see some CFD simulations. This is the air blowing across it. And we were able to simulate cross-wind conditions of up to 12 miles an hour. The reason for doing that, there was some indication through earlier modeling attempts at the NRC that a sustained wind condition could decrease the mass flow rate of the air into these types of systems. And so indeed, this is normalized mass flow rate. This is the mass flow rate measured divided by the mass flow rate with no cross wind. They start off at one. As you increase the cross-wind speed you can see that there is a decrease in the inlet air mass flow rate. We were able to measure that.

This is for 5 kilowatts and for three different pressures, very similar behavior. And then down here this is 800 kiloPascal internal helium pressure measured for three different decay heats, 5-kilowatts, two and a half, and 1.

You get a bit more spread as you change the decay heat. Again, very similar behavior. You see a decrease and then kind of a minimum approached here.

The CFD results do go on, up to about 20 miles per hour. They do show, this trend starts to reverse. So, the cross-wind begins to help you at higher wind speeds.

In summary, we have 14 data sets for each configuration as well as 13 additional data sets for cross-wind. The comparisons with CFD simulations in my opinion show favorable agreement. I'm a bit more skeptical when it comes to agreements some. . . But these show the physics are accurately captured in the CFD modeling, almost within experimental uncertainty for almost all cases, I should say. Additional steady state comparisons for the basket canister and overpack also show good agreement. We are in the middle of a validation exercise as I mentioned with NRC. Some of our colleagues or compatriots in Spain and we will be presenting that at the EPRI ESCP meeting.

Future testing, I'm going to skip ahead on these. I thought it would show the colors all together, but it doesn't.

What you saw here was the phase one testing. So, it was a single BWR assembly and convective heat transfer system, multiple thermocouples attached directly to the cladding. What Brady presented just to give you a little context was the higher burnup demonstration cask. What we intend to do is move on to phase three, which is taking the assembly from the phase one testing and putting it into a horizontal configuration and looking at that system's behavior.

To do that, we have to build a vault-style enclosure for the assembly. And that design is underway. And we will be monitoring the flow through inlet ducts, much like we did for phase one testing. It will be the same assembly, we will have the same thermocouple layout that we had for phase one. We will be able to again fill the vessel with prototypic internal helium pressures.

This is what it is going to look like. These annular gaps will not exist. It will be inside of a vault. We are going to brace it and add a bridge plate so we have a good conductive link between the fuel assembly and basket. We have to add stabilizers to keep everything centered, a compromise we have to make, the system was always meant to be concentric. We have to add these extra braces in to keep it in that configuration.

So, it is currently in this vessel and we will have to move it up to the third floor. We can't lay it down horizontally inside the vessel but we would like to. It's nice and quiescent inside of there. We can move it up to the third floor and reconstruct it up there, reconnect it and continue testing.

There is also effort ongoing to explore different concepts for these types of thermal hydraulic studies. There are a limited number of full-length assemblies for potential inter-assembly heat transfer. The CFD models tend to use what is known as porous media. Rather than discretely model the fuel elements, they blur it into homogenous media.

Getting some data much like what was done with the higher burnup demo but perhaps with a bit more control would be useful and challenging some of those model assumptions. There is also some work on scaled assemblies which are simplified but representative of the fuel. But perhaps more economical to field and maybe a mix and match with having a fuel assembly that is prototypic geometry mixed with an assembly that is simplified.

So, the bottom line is we still are interested in investigating sources of modeling uncertainties, particularly the basket to canister contacts mentioned earlier, the gaps, as well as some other intricacies such as bore-out construction.

The goal is to refine best practices guidelines and offer insights for future modeling.

With that I'll stop and turn it over.

>> BAHR: Great. We have about ten minutes for questions before we are scheduled for a break. So, are there questions from the Board members? Dr. Peddicord?

>> PEDDICORD: Going back to your tables on page 9 and 10, putting the comparisons, this may be an artifact of having the academics in, but would it be worthwhile -- I'm not sure it is -- to round these out by also showing the comparisons across these, but also the Prandtl number and the Grashof number to capture the spectrum of what is going on in this? Maybe it's an extra credit opportunity, but you kind of capture the other phenomena going on here too.

>> DURBIN: Right. Well, the Prandtl number we are dead on because we are using pressures and helium. For the Grashof number it is another incarnation of the Rayleigh, it's Grashof times Prandtl, but you're right, good point.

>> PEDDICORD: Thank you.

>> BAHR: Other questions from the Board members? Dr. Brantley?

>> BRANTLEY: In both your talk and the previous talk, I really appreciated, you know, seeing what measurements were made and the geometry and the modeling. But there was always a jump to when the models didn't predict the data, there was always a jump to here is the explanation. And can you just talk about that jump? Why do you think the annulus is the problem, why does the previous team think the gap is the problem? There are multiple working hypotheses and then they converge. Can you talk about that?

>> DURBIN: Yes, as an experimentalist it is always my fault when the data and model don't match.

To your point, you are probably referring to this. So, when we have an observation between the experiment and the modeling that shows a definite trend, you know, the red dashed line which is the least square regression for this is definitely shifted down.

>> BRANTLEY: It's systematic?

>> DURBIN: Systematic error. When we were looking at our measurements we saw that we did not have uniform flow in the annulus which we expected. We were drawn to the fact that we had this flow straightener inside our system that was introducing this nonuniformity. So, I guess to your question, we were forced to try to explain the discrepancy. This was what we came up with as a possible source of the discrepancy.

>> BRANTLEY: But does your team try different hypotheses? Does everybody just converge on one thing?

>> DURBIN: This was the most likely that we were able to identify. If we had enough time, we probably would have modified or changed our flow straightening to test the hypothesis. But we did not.

>> BRANTLEY: Thanks.

>> BAHR: Other questions from the Board? Dr. Tissa?

>> ILLANGASEKARE: So, the Rayleigh number will change with the horizontal configuration; is that correct?

>> DURBIN: That's correct.

>> ILLANGASEKARE: The Reynolds number probably will not?

>> DURBIN: The correlations used for horizontal cylinders will be different, yes, sir.

>> BAHR: Dr. Zoback?

>> ZOBACK: I have a question for Brady. So, if other people have a question here, I'll wait.

>> BAHR: Dr. Turinsky?

>> TURINSKY: (Speaker away from microphone.)
That's more widely used today to do --

>> DURBIN: COBRA and CCM plus, yes, they will.

>> TURINSKY: Is anyone going to use other than porous media to mesh the heck of it?

>> DURBIN: CCM will use porous media, I can't imagine they will use discrete but the COBRA has the elements.

>> TURINSKY: A single assembly, you could discretize?

>> DURBIN: Yes, in fact one of the submittals we have from the Spaniards models the fuel discretely with CCM.

>> TURINSKY: Okay.

>> BAHR: Other questions from the Board? For Sam? We had a question from the Board for Brady.

>> ZOBACK: (Speaker away from microphone.)
Brady, well, both of you guys gave great talks, I really, Brady, appreciated all the background information on the experiment

because we heard it a few years ago. I'm a seismologist so I'm not staying up on this.

My question is about the end point for the experiment. And you mentioned something about you hope to move the casks in ten years. I assume at some point you hope to open the casks because in the end the only way you can say with confidence that these things haven't happened is to actually look at the fuel rods, correct?

>> HANSON: That is correct.

>> ZOBACK: Why is it being moved? You can't open it where it is? You want to actually move it as part of the experiment?

>> HANSON: Well, in order to examine the fuel, you need a hot cell facility to do that. And utilities are not R&D organizations. We are very lucky to get them to do what they did for us here. So, we are looking at probably a DOE facility somewhere or ...

>> ZOBACK: Is there a facility?

>> HANSON: As I said, we are looking at a couple different options. Right now, a lot will also depend on relationships with states to say yeah, will we accept this or not. But yeah, there's at least two facilities that are in the U.S. that could handle this.

>> ZOBACK: My other question, since this is higher burnup fuel, are you going to wait a longer time to open it? You put it in hotter, too, didn't you?

>> HANSON: Put it in hotter. The reason for the ten years, right now there's three different utilities that in their relicensing to extend the time period are banking on this data. That's kind of what is driving the time right now.

>> ZOBACK: Okay. One other question about the database, which I assume is an aggregate of all the fuel in the U.S.

>> HANSON: Yes.

>> ZOBACK: When you say that General Counsel Office is doing it again, so will there be a data gap between 2013 and 2018? Or are they asking the utilities to go back and provide the data for the past five years?

>> HANSON: Yes, you have to go back and provide everything.

>> ZOBACK: Okay, thank you.

>> BAHR: Okay. Other Board member questions?

(There is no response.)

>> BAHR: Questions from the staff? Dan Ogg.

>> OGG: This is a question for Brady. You can defer to Keith Waldrop if you need to. On the timeline for the demo cask draining and drying, you showed a time period, fairly long time period for the vacuum drying process. Was that just one single vacuum drying evolution? Were there multiple cycles in that?

>> HANSON: It was just one evolution. I'll look to Keith. I believe you had hold points throughout. So you pull a vacuum to a certain level, stop for a bit and go down so that you don't have any ice formation. It's just one cycle.

>> OGG: And the pressure rise test, you were down to 0.4 torr and held for 30 minutes. Was that test done just once?

>> HANSON: Just once.

>> BAHR: I saw Bret, Nigel first.

>> MOTE: Brady, good to see you again and thanks both for the excellent presentations.

Obviously, the HDRP program has advanced understanding of temperature distributions and profiles in a vaulted cask and you were very clear at the beginning why you chose vaulted cask and the impractically of doing this on a welded canister. Can you comment on how applicable the results are to a canister? Physical differences, material differences, canister being in a transfer cask rather than a single piece cask? And the pull when you are doing the blow down? Maybe you can comment on how applicable these are to the larger community of canisters.

>> HANSON: Sure. So yeah, the conduction systems are in some ways easier to model, but I think the important message is by combining what we've learned from the phase one convective study that Sam has done, from the additional phase three studies that he hopes to do, like I say the modelers are convinced that they

know the physics of what is going on in an internally convective cask. So, we think we understand that well. And the experiments that Sam will do and has done will help verify that.

I don't think there's many other differences other than you will still have certain assumptions. If you assume that your assembly is centered within the basket and not touching, you know, depending on what the temperatures are you have a larger radiative heat transfer effect. So, there are minor little differences, but we have modeled convective systems in the past. UNF standards database does the same. Like I say, it is actually those systems that have the higher temperature that I said that we found, the 325 degrees C maximum.

>> MOTE: Okay, you are confident with the understanding that Sam is getting in other models that you can take this model and apply it to canisters?

>> HANSON: Yes.

>> DURBIN: I would add to that, it's a really good question. You are absolutely right, there are different construction materials. But when those are taken into account properly in the models, I think the accuracy is there.

>> MOTE: Okay, thanks. Brady referred to the thermal cycling effect you get in the vaulted cask that you don't get in the canister. The physical layout is different when you have the canister in the storage over pack. The centering issue is

critical and we saw this at San Onofre. In longer term performance rather than blow down there will be differences, there are no concern about those differences having a long term effect?

>> HANSON: As long as both Sam and I have said we have the proper inputs, so for the internally convective systems, knowing what the size of the, what they call the mouse holes down at the bottom of the basket are to make sure that you can model those convective flows properly, as long as you have those inputs, then there shouldn't be any issue.

>> MOTE: Okay, thanks.

>> BAHR: So, we are at time for a break. Maybe if people have additional questions they can catch Sam and Brady at the break. Thank you very much. And we are scheduled to reassemble in about 15 minutes, at 10:20.

(A break was taken.)

(Silence.)

(Music playing.)

>> BAHR: Part of my job is to keep us on schedule. We are going to get back to the meeting here. We have heard a bit about the higher burnup spent fuel canister. A big part of that program is the sister rods that they are examining that are parallel to the rods that were placed in the cask. Rose Montgomery is going to tell us about some of the post irradiation program.

>> MONTGOMERY: Good morning. Thank you all for inviting us here to talk. I'm Rose Montgomery with Oak Ridge National Laboratory. I am going to tell you a little bit about what we have been doing with the sister rods. Brady talked a lot about our spent fuel storage cask experiment. What we did, though, before we closed up that cask is we took some rods from some of the assemblies in that cask and we've taken them to the hot cell and we will look at those in detail to get a baseline of what these rods look like before they go into dry storage. We are also doing targeted experiments to look at what happens when they go through some things like the dry storage vacuum drying procedure. What I'm going to show you today is the results of our nondestructive test and then some results from the start of our destructive testing and then I'm going to go through our schedules for what is going to happen in the next year or so.

So, when we first got these rods, we did, we took bunches and bunches of photos, lots and lots of images for each rod.

What you see here are some of those images. What we found is, of course, what you would expect that the ZIRLO and M5 rods had expected oxidized appearance and the Zirc-4 and wow-tin Zirc-4 had some heavier oxidation and CRUD spallation. Some of them had grid rod fretting in varying degrees. I'll show you these, there are some in the photos up here. These marks are made by spacer in the reactor during the operation.

And we also could see in some cases in the higher burnup region the pellet-pellet interfaces. You can see those here. There's a pellet and then there's an interface between pellets. Here is a pellet and an interface and so on. So, you don't see those on every rod or at every elevation but you see those quite frequently.

Something else that we also saw were some gaps between pellets. And we didn't, you know, at the top of each of those rods there's a spring inside. And one of the purposes of that spring is to hold the stack together. So that you don't get a gap. We were surprised at how many gaps we did find. The largest one was the 5-millimeter stack gap which is shown over here. You can actually physically see that dark band where that occurred due to the lower temperatures during operation on that rod at that location.

We also took gamma scans of these rods. We put them in and counted the number of gammas in 1 millimeter increments. You see that over here. That's a typical profile for one of the sister rods. And they were very high resolution. We were able to see every single pellet in the stack. And you can also see the spatial grid burnup depression. The spacer grids are parasitic to neutrons which produces a lower burnup at that location which produces a lower gamma count on our gamma scan. You can see the grids here and here. In the case of this rod it was an M5 rod that had flow mixing grid as well, three of those in the upper stands. You can see smaller depressions related to those.

Now, because we were able to virtually see every single pellet in a stack of these rods we were able to measure a lot of things. One thing we could measure was the average pellet length in each of the rods. We also were able to count the number of pellets in each of the rods. We also were able to measure the plenum length and the length of the stack of the fuel stack.

We had a lot of good measurement data from that. We also measured the length of the fuel rod itself at the outside. So, one purpose of this was to identify the best locations for cutting these rods, which we have started to do in our destructive examination process.

The next thing we did, we did some profilometry. We measured the outside diameter of the rods. All of the measurements we took follow the expected trend showing that the diameter is higher in the high burnup regions as expected, and lower in the low burnup regions.

So, we measured these diameters in two different ways. The first way we used linear variable differential transformers or LVDTs. These are contact probes, we measured them, the diameter, touching the rod in one direction and had a second pair that was oriented 90 degrees from that that touched in the other direction. We basically measured along the entire length of each rod. And the second way of measuring, we took around 840 photos of each rod at every elevation and every rotation.

And we thought, well, surely, we can use this data to measure the diameter. The way we did that, we got a calibration rod of known diameter and took photos of that and developed a link to pixel ratio to calculate the diameter of the rods and we got a lot more diameter values from that. So, we had, we were also able to extend that into the ends of the rods where the LVDTs couldn't reach. The graph over here shows you on the top here is the gamma scan. I picked a typical ZIRLO rod and the blue is the LVDT measurement. As you come from the bottom and move up, the diameter is increasing and it peaks here in the high burnup region and you can see the grid depressions which corresponds to the grid depressions in the gamma scan.

The yellow dots are the visual method. You can see there's some bias in that method. It is a method that we would like to develop further because it is a noncontact method. But we need to be more refined about how we develop our link to pixel ratio because that is where our problem is. At the end of the day those two agree within .03-millimeters which is really good. The uncertainty in the LVDTs measures, we reckon is plus or minus .02-millimeters.

Over here I have the groups by cladding type M5, M5, the ZIRLOs and the Zirc-4 types and by parent assembly. If you remember back to the map that Brady showed on the cask, he showed you the fuel assembly ID numbers. So, the rods from, the sister rods shown here came from one of the assemblies that is in the demo cask.

You can see the measured ODs here, this is the maximum measured on that rod and here is the average measured for those rods. Those were in the range expected mainly.

Another nondestructive test that we did was eddy current. What we wanted to know from the eddy current was, what can we expect for oxide thickness? We did a measurement called lift off. With lift off we are looking for the distance between the fuel cladding, which is the conductive material, and the tip of the probe. It is separated from the fuel rod cladding by whatever CRUD is there. The lift off is the sum of the oxide plus the CRUD.

One thing I should point out is if there is spalling there and the oxide -- so imagine that the oxide layer and the CRUD are essentially missing, you'll see a dip in the measurement. You can see a sharp dip in that. You'll see that in some of our data.

Here is the data that we did get. When we did the eddy current what we did, we did a trace in every quadrant of each rod. We did a trace on one side, rotated 90 degrees, did another trace, rotated again. We took an average. There are 25 sister rods up here, the M5 clad rods. You can see they have a very low lift off level. This gets to be about 30 at the max. But it does follow the expected trend. Lighter at the bottom. Increasing towards the high burnup regions and then dropping off again as you get up in the plenum.

For the ZIRLO rods it is a little higher. You can see there is a thicker lift-off you can see where the spacer grids were here and here and here. And you get up to about, I think this is around 65, 70-microns of lift-off for the ZIRLO rods.

The Zirc-4 and the low-tin Zirc-4 had the highest. I bumped the scale up here. These two were 100. These are 200. Much higher lift-off values here, much bigger between grid. Excursions here, you can see evidence of spalling there even in the average clads.

What I've shown you is the maximum of each of those alloys, plotted again -- actually for each rod by alloy, plotted with some historical data for each of those cladding types. For the M5 it's the red dotted line. You can see the M5 lift off data falls right in the middle of that. Pretty much where we expected that to be.

For the ZIRLO, the envelope, historical envelope that was publicly available in our ZIRLO measurements, right in the middle of that.

The Zirc-4 and low-tin Zirc-4, what I found publicly is only Zirc-4 data, so that's what I'm showing with the green dashed lines in the envelope. One of our rods is right at the tip of that. Three of them are slightly outside of that but follow that trend.

And here is the data up here on the average maximum measured lift-off and the maximum rod average. You can see the average versus the maximum there.

So, we did have historical data on some of these rods. We wanted to compare it and also as a part of our measurement we did repeated scans. What we did, we scanned one rod. We put it away. Then a couple days later we took it out and scanned that one again. That's what I'm showing you here, how repeatable is that measurement. You can see we have extremely good repeatability, especially in the higher lift-off rods you can see it's virtual on top of each other. This is in the lower lift-off range, it’s right around 30 microns, this is the M5 rod. It is matching fairly well. You have a little bit of differential in the axial location there, I think that's what that is.

Very good repeatability on our measurements in hot cell. We also had EPRI come in a measure the rods independently. They brought their own eddy current system. They are actually developing this system to measure hydrides in the cladding. They had a different purpose for doing the measurements, but they used the same type of measurements. They also gave us some information on what they thought the lift-off was. You can see that down here. Here is the ORNL measurements for M5, ZIRLO, low-tin ZIRLO and Zirc4, and here is what EPRI got when they measured the exact same rods in the hot cell with their system.

Low-tin Zirc-4, they got about 50-microns less, ZIRLO about the same and for Zirc 4 they got about ten microns less. Theirs is a point measurement system. They will go to a place on the rod and take a point measurement and that's their answer. Perhaps they missed some of the higher lift-off locations. In addition to the EPRI data we also have historical data from 2002 from a pool-side exam. Two of our rods were lead rods meaning they were pushed much harder than typical to test the rods to see what their response would be in the reactor. When they did that testing, they did pool-side examinations to where they measured lift-off. That's what I'm reporting here. You can see they got even lower numbers at the pool side when they did this measurement than we got and then EPRI got.

So, it is clear that there's differences in these numbers. We are not sure completely, we are very confident in our numbers, obviously. We are not sure what is lift-off, which part is CRUD. It could be part of our lift-off numbers that are making the difference is the CRUD. The metallography we do at the end of the day will be the thing that tells us what the actual oxide thickness is and what part of that is CRUD. We expect to do that fairly soon.

I mentioned that we wanted to do some separate effects tests to try to understand what happens to the cladding and what happens to the fuel when it goes through the vacuum drawing process. And so, we built a heat treatment oven that was capable of taking an entire rod and heating it up. It is zoned so you can make it hot at the top and cooler at the bottom or make it hot in the middle and cooler at the end and simulate most dry storage cask heat environments on this.

And we put three rods in. We picked one and five -- one M5 rod and one ZIRLO and one Zirc 4 rod and put them in the oven one at a time and heated them to 400 degrees C.

For this particular test, we decided to do a flat temperature profile. The rods were heated up to 400 degrees C at all locations on the rod. And they were heated up to that and cooled down slowly. At a rate of 5 degrees C per hour, less than 5 degrees C per hour and that to be consistent with what ANL has done with their hydride reorientation testing and consistent with the slow cooling you would see in the field. I do want to point out the slides that you have that you are seeing maybe don't have that per hour on there. Somehow that got left off. It's 5 degrees per C per hour is the rate there.

So, we put each rod in the oven and heated it up. Took about 38 hours to heat it to 400, we held it at 400-degree C for eight hours and let it cool down at that slow rate for 100 hours. We are now in the process of getting metal graphic and mechanical test results from the heat-treated rods. What we are going to do is compare those directly with rods that are comparable to those in the sister rod collection.

We should see directly what effects, if any, we had from the vacuum drying temperatures on the cladding.

The next thing we did, we took three of our -- so what we are doing in the first phase, we are taking baseline rods, one from each alloy, and heat-treated rods, one from each alloy. Six rods in total. We are working with those first because we want to find out from those, we want those first rods to inform what we do in our next experiments. So we got Zirc-4, low-tin Zirc-4, M5, and ZIRLO and we’re working with six of those four on our metallography and mechanical testing. Plus we have two more that we’ve used for gas communication testing which I'll talk about in a minute. We have the results of eight rod internal pressure measurements here and they are hard to see within the cloud of data.

This is historical data mainly that EPRI had available in public domain published in 2013. This is mainly the hollow symbols there. You can see our sister rods are in the middle here, the more solid ones. And the heat-treated ones are the ones outlined in the orange. One here, one here, one here. The data falls really within that envelope of past data, very comparable to past data, on rod internal pressure measurements. We don't think we have a difference in rod internal pressure from the heat treatment that we did. The other thing we measured on the eight rods are the free volume inside the rod. Just to remind you what the free volume encompasses, we know at the top of each of the sister rods there is a plenum. It is empty, has a spring in it. That is a reservoir for fission gas and also in the pellet stack, each pellet in most of the sister rods are designed with champers and dishes. Those allow within the fuel stack some volume for fission gas to collect. Of course, these pellets crack during operation and even the cracks inside of the pellets allow for the fission gases to have a volume.

So, what we measure in the hot cell is the sum total of the stack plus the plenum. It is the whole volume, free volume available in the rod.

And what we've measured so far is 9.9 to 13.3 ccs on the sister rods. Like I said, that includes all the different flavors of cladding that we have within the sister rod collection. It falls slightly lower than the EPRI historical data which ranges from 11.1 to 39.5. But I want to point out in terms of a comparison point, rod internal volume -- the end of life rod internal volume -- is largely connected to correlated to what the rod internal volume was designed at the beginning of life. So, you can't really compare rods one to one if they are a different rod design and had different beginning of life rod free volume. The same thing goes with rod internal pressure as well. Even though our rods fell within that range of rod internal pressure there is more of a correlation between the fill pressure and end of life pressure and the beginning of life volume and end of life volume than there is with burnup.

And again, on the volume measurements we didn't really see an effect of the heat treatment there. They didn't appear to behave any differently in terms of the free volume available.

So, we tested some rods. All of the rods we tested for decompression. First let me explain to you what this test is. If you look up here you will see a schematic of our fuel rod. We punch a hole up here and it is sealed off. So, for this particular test it had, we had a vacuum on it after we measured our volume. We backfilled the rod with argon to a selective pressure around 180, 140, in that range. And then we sealed that off. And we are monitoring the pressure. Then we cut the end off.

So, all of the pressure inside the rod wants to depressurize through the stack and come out the bottom of that rod where that opening is. And we measure that for all of the rods that we've done so far. So, you can see we've done, these are M5 rods. Here is a heat-treated M5 rod, here is a ZIRLO rod and we had a heat treated ZIRLO in there, and here is low-tin Zirc-4 and a

Zirc-4 rod that was heat-treated. They all pretty much behaved the same way. They immediately began to decrease in pressure until they come down to equilibrium with atmospheric conditions in the hot cell.

The longest took around 22 hours to do that. Some of them were shorter in time. A couple of hours there. It does take a significant amount of time for the fission gases to depressurize. You can sort of liken this to a rod that had a large leak. It would take it about 24 hours to decompress completely.

I do want to point out this is at room temperature. So, as we know these rods are not completely at room temperature. If they were warmer, that behavior could be different.

So that was one of the tests that we did to see how the gas, the fission gases can move along the entire fuel stack. We did a second test with just two rods. We picked two rods that we could do this on. So, after we had taken the bottom off of those fuel rods to do this gas depressurization test, we put a fixture on here. And we pulled a vacuum again, sealed this off and were monitoring the pressure up here. Right now, at the beginning of the test we have 0 PSI up at the top of the plenum. Then we applied the pressure at the bottom. In this case you are watching the pressure and the gas flow through the pellet stack and into the plenum. We watched it until it made it up to the pressure we had applied, pretty much.

You can see these are the tests for the low-tin Zirc-4 rod, the gray, orange, and the blue, and the green, purple, and yellow are three different tests that were done with the M5 rod. Each of these tests there was a different pressure applied at the end. The higher the pressure, the quicker it comes to equilibrium. You can see that these tests or in this case with an applied pressure that is maintained, that it is much quicker. It can come to equilibrium in about three hours in the case of the M5 rod. And it was probably half an hour for the low-tin Zirc-4 rod. You can liken this movement of gas, let's just say maybe the bottom of the rod is hotter and the pressure wants to equalize. So, the gases will move through the rod. It's sort of that kind of idea where you have a fixed pressure but you've got a transient going on inside of there.

So, what is coming up? We are planning to begin the destructive testing -- mechanical testing. But the mechanical testing we are planning to start the beginning of November. We will do that with our CIRFT tests. That's the room temperature test. We have that rig already in the hot zone. We should be able to begin testing samples very soon. The other tests that we are planning to do, fuel ring compression test at room temperature, beginning in January. Axial tension test at room temperature and at 200 degrees C beginning in February; four-point bending at room temperature and 200 C beginning in April; micro-hardness – we’re planning to do those also, beginning in April. And then we plan fuel burst tests, we're not sure what temperature we will be running that at or if it will be variable temperature with fixed pressure, we are not sure, beginning in June. These tests will include as I mentioned earlier one baseline rod for each alloy and one heat treated rod for each alloy. There will be six rods involved in this testing.

The other data we are getting to support mechanical test is we are characterizing the rods. We want to know what we are going to do metallography to look at the hydrides in the cladding, what the orientation is -- doing total hydrogen measurements to try to understand what the total amount of hydrogen is in there.

We are doing burnup analysis on the fuel to verify that it is at the burnup we expected. We are doing fission gas composition tests right now. When we run internal pressure measurements, we pull the fission gas sample measurements and we are looking at that right now. We want to take a closer look at mechanical testing on the effect of grid to fretting and the stack gaps we saw. We know those can make a difference in the mechanical performance of the rod in some of the situations like venting.

And finally, one of the things that we want to do is to try to collect any aerosol particles released from the fuel during the mechanical testing program. For example, when we, if we bend a rod and create a crack in that rod, is there any UO2 particles that come out? We want to collect those if they do come out and characterize the size of those. That's one of the things we are working to achieve.

I think I am at my summary slide. All of the nondestructive testing that I talked about here is already available in a report that is out there. You can find it at this link right here. If you want to read it. The rod internal pressure free volume gas transmission test I mentioned, we completed those. We've done eight rods so far. And that is all we will do until we complete the mechanical testing and the supporting work that we are doing on these rods.

And as I mentioned we are including three baseline rods, three heat treated rods. We will be beginning the mechanical testing next month with CIRFT testing and we are developing a lot of supporting data to characterize each of those rods. I want to point out that we do publish a status report once a year. We give regular updates at the EPRI ESCP meetings twice a year. And that's my last slide.

>> BAHR: Okay, thank you. We have about nine minutes for questions. So, start with the Board, Paul?

>> TURINSKY: Rose, maybe you mentioned this, but for the pressurization test, the difference between the low-tin Zirc-4 and ZIRLO was pretty dramatic. What is the reason for that

difference? Why does one have much better communication than the other?

>> MONTGOMERY: Can we go back to that slide? I think you are talking about this one. This is where they had, we pulled the vacuum on one side and we put a pressure source and watched it go up.

I believe, and this is just a theory at this point. I don't have any real data to support it. I believe that -- let me say we've done a correlation, a porous media correlation to the stack flow. It fits very well. We are publishing that hopefully soon.

I believe it is related, I've done some looks at correlations with the linear heat rate and operating temperatures. So, I believe that it is more related to the number of cracks, density of cracks that are developed during operations. And you know, the more cracks you have, the easier it is to flow through.

>> TURINSKY: Are the pellet compositions the same? They change over time too. There are additives.

>> MONTGOMERY: I haven't looked at binders and additives, but that's true. I don't know the answer to that.

>> TURINSKY: Okay.

>> BAHR: Board member Peddicord.

>> PEDDICORD: Back on slides two and three where you talk about the measurements you took; do you have the as-fabricated data

from the vendors Westinghouse and Areva as you made your measurements and compare back?

>> MONTGOMERY: We have, what we have is nominal design data.

>> PEDDICORD: Design data?

>> MONTGOMERY: Yes, for the pellet length. We know the pellets, how long they were intended to be. We don't have a measurement of every single pellet that is in each and every rod.

>> PEDDICORD: Then on the particular one on slide 2 where you showed the gap that you identified, have you reached any conclusions? Is that an artifact again of the fabrication process? Or alternatively of the stack expanded and then cooled down and the top half hung up against the clad to create the gap? Any way to determine how that came about?

>> MONTGOMERY: I don't think there is. My personal opinion is that it would be a manufacturing gap. The 5-millimeter gap is a pretty large gap.

>> PEDDICORD: And well done in catching that as well. Well done on catching that.

>> MONTGOMERY: Yeah. So, I really don't know the source of it. But that would be my guess.

>> PEDDICORD: Last quick question. When you are talking about determining the free volume later on in the presentations, talking about the chamfers and dishes, at temperatures, those dishes are closed. Did you take that into account when you were

calculating free volumes? Is that's why they are there to accommodate the higher center line temperatures.

>> MONTGOMERY: When they are at power in the reactor, they do expand and you have a lesser free volume. Since we are concerned with dry storage, the temperatures are much closer to room temperature, that is not really true during vacuum drawing. Vacuum drying is another situation where we would want to know. One of the experiments I would like to do is to put one of the rods in the oven, let it get hot and do the gas transmission test and see what effect temperature has. I'm not clear on what that is.

>> PEDDICORD: Thank you.

>> BAHR: Okay. Tissa?

>> FOUFOULA-GEORGIOU: You mentioned the free volume at the end of life depends as expected on the initial conditions. The same is possible for the other parameters. So, my question is, given you cannot repeat the experiments for a large range of initial conditions, is there any effort to understand, even through modeling, the possible scaling of end of life to beginning of life, any relationships? That will not be a trivial scaling relationship. It will depend on many parameters, but I think would be useful. Is there any effort along in this direction?

>> MONTGOMERY: I understand that you are asking about the relationship that I mentioned about rod internal pressure,

initial condition at the beginning of life and end of life and the same with the free volume.

And I agree, there are a lot of parameters that matter. Of course, burnup is a parameter that matters because that's what is producing the fission gases that get released which in turn get released from the pellet and make this pressure in the rod. There are a lot of parameters. That is being studied by other folks. This project is not studying that.

>> BAHR: Thank you. Allen Croff?

>> CROFF: Are there plans to retain rods or pieces of rods for future use in this program or others.

>> MONTGOMERY: You might want to have Sylvia answer that. We are working with a certain number of rods and the rest will remain with us until we decide if we want to do further testing. I don't know the ultimate disposition of those.

>> BAHR: Other questions from the Board.

>> SALTZSTEIN: You want me to address that quickly? We have 25 rods. We are using about ten of them right now. A little more than ten, but chopped up, about ten equivalents.

So, we have quite a few remaining rods. We have 15, 14 remaining rods that we are reserving for this phase two testing. After we get this initial destructive and nondestructive mechanical information we are going to look at that holistically and decide what we want to do with the remaining rods. But you know, if budgets are good, we hope to use all of those rods to get as much information as possible.

Though we may make the decision that yes, we want to just keep some and maybe see what happens to them in ten years. Again, we will look at that with this data holistically and decide what to do in phase two.

>> BAHR: Dr. Zoback?

>> ZOBACK: You took out 25 rods. Where there 25 fuel assemblies in the cask?

>> MONTGOMERY: I don't know how many there are in the sister rods, we have 25 fuel rod assemblies. Some of them in the cask and some not. Brady, do you know how many -- obviously very few fuel assemblies in the cask.

>> HANSON: All of the (Speaker away from microphone.)

>> HANSON: All of the M5 cladding that we are testing came from two different assemblies that are in the cask. All of the other fuel came from, if you will, sister assemblies. So, the, we'll have an assembly in the cask that has a certain burnup enrichment cooling time. We took rods from one that was just like that but not in the cask. And the same for the Zirc-4 and low tin. Only the M5 have assemblies in the cask.

>> ZOBACK: Just so I'm clear, I understand, of these 25 rods, two of them were taken from one type of fuel assembly and that's in the canister.

>> HANSON: We took a total of nine M5 rods, I believe it was five from one assembly, four from another.

>> ZOBACK: Okay, you have nine.

>> HANSON: Yes.

>> ZOBACK: Ultimately when you reopen this thing those are the ones you will be able to compare most directly to what you find when you take out -- so you have nine of them?

>> HANSON: Correct.

>> ZOBACK: Is it only one type of manufacture -- I don't know how you define the fuel assemblies. One manufacturer?

>> HANSON: By cladding type, and that is tied directly to who the manufacture was, yes.

>> ZOBACK: Okay, thank you.

>> BAHR: Okay. Do we have any pressing questions from the staff? Nigel?

>> MOTE: Rose, thanks for another interesting presentation. On slide six you showed the lift-off for Zirc 4, low-tin Zirc-4, M5, and ZIRLO. The results for the two Zircs were significantly higher. That's for high burnup fuel. Can you talk about how the experience that you have there for the M5 and ZIRLO at high burnup compare with Zirc-4 and low-tin Zirc-4 at lower burnups? Does the cladding change more than offset the lift-off?

>> MONTGOMERY: I'm not sure I understand your question.

>> MOTE: Okay. You are looking at the lower curve on the left that shows on the scale, north of 200 microns, you have Zirc-4 and low-tin Zirc-4, and you are pushing 190 microns. You have more lower experience at this burnup at high burnups for M5 and ZIRLO. If you did the same test with low burnup on Zirc-4 and low-tin Zirc-4, where do you think the results would come out on that plot? The same as ZIRLO and M5? Lower than? How would lower burnup on Zirc-4 compare to higher burnup on M5 and ZIRLO?

>> MONTGOMERY: You can see from historical data in the burnup range below 40, 30, between actually -- 20 and 50 that you get this overlap, right? Right in here. Right in here ZIRLO and Zirc-4 historically have performed about the same and M5 historically performed a bit with lower lift off. So, I would expect at lower burnups for these to be down here.

I think what is happening here is perhaps that there's CRUD on these rods. That's what we registered and why it is a little bit beyond the historical data. I expect once we get the metallography we will be able to answer that question better.

>> MOTE: Okay, thanks.

>> BAHR: Okay. The next speaker is Mike Billone. And he is going to talk about fuel cladding hydride reorientation research.

>> BILLONE: Okay. Mike Billone. I'm going to talk about spent nuclear fuel cladding hydride reorientation research.

We will talk about the rods that would discharge from the reactor, sitting in the pool and then sent to hot cells. We'll talk about previous results and current results on radial and circumferential hydrides and mixed with that plans for sister rods testing.

Let's start out with background information. And I don't find the laser pointer very effective. Hold on a second. First of all the PWR cladding alloys, these alloys behave differently in reactor. Your starting material for storage is quite different. Let's sort of define what the alloys are. For pressurized water reactors and just roughly what decade they were prevalent in.

So, we'll start with Zircaloy-4, basically zirconium, weight 1 percent tin alloy, the cladding of the 1970s, continued through a little bit of the '80s. The problem with it, you try to go from 30 gigawatt days per metric ton burnup. I -- forgive me for the units, I didn't pick these units. To 40 or 50, the oxide layer on the coolant side continues to grow. You continue to pick up hydrogen. So, you get excessive hydrogen pickup with increased corrosion as you try to push this to higher burnup.

The tin that is in this alloy is good for stability and strength but leads to added corrosion. So, we'll move over to low-tin Zircaloy-4, where you drop down to 1.3 weight percent tin, that's the cladding of 1980s, continuing a little bit into the 1990s, but still you have trouble getting up to 50 gigawatt days per metric ton or higher, because eventually the corrosion layer and hydrogen pickup will continue to grow. We'll jump to the '90s. It is not universal for each reactor as to when they switched over. To get to the higher burnups, Westinghouse developed ZIRLO which is lower in tin, 1 weight percent tin and 1 weight percent niobium, and Areva (Framatome) changed name several times, has M5 which has no tin, it is zirconium 1 weight percent niobium, the second row of alloys are what are currently in reactors, pressurized water reactors in this country. In terms of what is in storage you have a lot of Zircaloy 4 and low-tin Zirc-4. The other factor that is important not just the chemical composition but how they are fabricated. I will use the word thermomechanical treatment or heat treatment. If you go through the final step to 600 degrees C for one to two hours, you essentially anneal out all the cold work in the material and you have an alloy with more, a different texture of grains, more randomly oriented grains, let's leave it at that. If you want to stay with higher strength and lower ductility and more control over the hydride precipitation you are only subjected to 500-degree C for four hours that relieves internal stresses but leaves what is called cold work in the material which increases the strength of the material and the texture is such that you tend to precipitate hydrides in a benign direction which I'll show you in a second.

Those are the materials that are in the sister rod program and those are the materials roughly we use.

In terms of hydrogen content and hydride orientation, in as irradiated cladding, that's prior to drying and storage. Let me show you some images of ZIRLO with 320 and 650 weight parts per million of hydrogen, if I slip to wppm, which I hope I don't, it always refers to weight parts per million hydrogen. On some of the graphs I just have wppm. And then we’ll contrast that with Zircaloy 4 with comparable hydrogen contents low and high. M5 tends to hardly ever pick up more than 100 weight part per million. That's very low corrosion rate, has no tin in it.

So just some pictures. And there's a couple important points to make. These first two pictures are from the same cross-section of the cladding, which means essentially a cut and I imaged the cross-section at anywhere from 10 to 30 locations around the cross-section.

And what you see is for this particular hybrid of ZIRLO, 320 wppm, most of the hydrogen -- I forgot to orient you, the top of the photograph, the dark region below the black is the oxide layer which is about 30-microns, in this case. And then these dark areas here are the hydrides. And you'll notice that the hydrides, they have a hydride rim right under the oxide layer, very high concentration of hydrogen. And then the high concentration of hydrogen decreases as you go across here. This is typical of a rod that was irradiated at high power, where you have a large temperature gradient. Hot here, colder on the outside, hydrogen migrates from the hot to the cold. You tend to get that.

As I go from this low -- this is roughly from a little below the midplane of the fuel element. This is the same cross-section, by the way. These two, just different locations. I just want to point out that you will see radial hydrides, again, sorry about this. You will see those radial hydrides as well as circumferential hydrides and isolated images, maybe one out of eight in this case up here.

So, they are fairly benign, short, isolated. You don't see them as you go around the whole circumference but they do exist. If we jump to the high hydrogen content, this is a different elevation of the rod but a high-powered rod. You see a very dense zirconium hydride layer. This is the oxide layer, about 60-microns in this case, all for ZIRLO.

If you go to another, this is a 3:00 o'clock orientation. If you go to the 6:00 o'clock orientation which I can't reach from here, but the hydride rim over there is not as dense. And you will on the inner surface see a radial hydride, a couple of radial hydrides. They exist in the material coming out of the reactor, most likely from pellet clad localized in the interaction during the cooling phase.

That is one alloy. Let's switch to Zircaloy 4. This top picture is 300 wppm. The dark, the circumferential hydrides which is what you see in this picture, are more diffuse, not as concentrated. This is a lower powered rod than the previous one. Much less of a temperature drop from hot to cold across the cladding.

And then strange things happen when you get the high hydrogen content. I do want to come over here, sorry.

The hydrogen is very nonuniform. These two pictures are from the same cross-section. And you have a very high hydrogen concentration on one side, a lower hydrogen concentration on the other. That's due to circumferential variation in temperature around the cladding.

This hydride rim is not as dense as what I showed you before but you do get hydrides through the cracking, through the cladding. These are important points of view. This material tends to exhibit a cracking at early displacement, whereas the previous ones tend to be more ductile with the hydrogen concentrated at the rim. This cladding is less susceptible to forming radial hydrides. The previous cladding is more susceptible.

So your starting material is important. In the reactor world of all fuel rods you have a wide range of powers, ranging from these extreme pictures to what I'm showing you here.

We will switch to M5 with the low hydrogen content. And this is the alloy that is called recrystallized, more random grains. You expect to see more radial hydrides but you have very low hydrogen content. This is about 76, let's call it 80 wppm, but you will see evidence in this picture of a few radial hydrides, the circumferential hydrides and the picture in the upper left you'll see longer radial hydrides.

Under reactor conditions most of the hydrogen is dissolved. This is not an issue. And in dry storage you don't have enough, your hydrides, radial hydrides are too short to cause any degradation. That's the extreme in the alloys that we are looking at.

Okay. I am going to jump back and forth between objectives of phase one sister rod testing and what our expectations are and try to justify the expectations, if I have enough time. So, in phase one, sister rod testing we are doing characterization and material properties. Characterization would be what is the oxide layer thickness. What is the wall, cladding wall thickness, how much hydrogen is in there, how is the hydrogen distributed?

We would like to generate a data that can be compared at ten-year stored PWR fuel rods. You saw the peak cladding temperatures in Brady's presentation, they are low. We cannot imagine that there would be radial hydride precipitation at such low temperatures other than what is in the cladding when you start. We don't see any annealing or irradiation defects. We believe that our baseline data for as irradiated cladding would serve as a good basis for comparison to the demo rods.

Also, in the program as Rose showed, we determine end of life rod internal pressures. Prior to what Rose showed we had limited public database collected by EPRI. We want to generate properties, mechanical properties for M5. Published data are really inadequate. And Areva has kept their mechanical properties very tightly controlled, as well as for ZIRLO.

Is radial hydride induced embrittlement an issue? I thought so five years ago when I addressed the Board. We will use measured rod internal pressures at 25 degrees C and Rose showed you eight of those measurements. We will be measuring approximately 18 rod internal pressure in phase one. Now we will heat treat it at 400 degrees C, which is the NRC-recommended limit. It is not a regulation. It is a recommended limit for high burnup cladding. At this temperature you will put about 200 PPM of hydrogen in solution if you have that much hydrogen to begin with. For M5 you'll only put the 76 weight parts per million in. The less than or equal sign means you have to start with 200 ppm to put 200 ppm into solution.

Also by going from 25 degrees to 400 degrees C we will be increasing the room temperature pressure by a factor of 2.26, because you need to do the ratio of the absolute temperatures in Kelvin.

The peak cladding hoop stresses at 400 degrees C should be a reasonable upper bound for standard PWR rods because these rods are stored with average gas temperatures much less than 400 degrees C.

We will be cooling at a laboratory slow cool rate of less than 5 degrees C per hour. That is when your precipitate hydrogen in the circumferential direction. Performing ring compression tests to determine ductility and ductility transition temperatures.

I'll use this acronym as I continue.

Okay, potential for radial hydride re-orientation is something we are exploring. In the demo cask, the Peak Cladding Temperature, less than 250 C is what I saw in the measured values. At 250 degrees C you only put 44 wppm into solution. You don't have much to begin with in solution to be able to precipitate the radial hydrides. Internal gas pressure should be less than nine megapascals. At that peak temperature of 250 degrees C and the peak hoop stress should be less than 68 megapascals. When I show you the previous results if you are less than 80 megapascals you don't have an issue with radial hydrides. That's coming. You may observe short radial hydrides, but these would not decrease ductility under the demo cask conditions. You open it up in ten years, this is what you would expect.

For the sister rods we are going to higher temperature. The pressure at 400 degrees C would be less than 11 megapascals and the hoop stress would be less than 87.

I will justify those in a moment. They are conservative upper bound type numbers. We expect for ZIRLO and Zirc-4 there might be radial hydrides, they are fairly short, ten to 20 of the cladding wall. Which are not very damaging and you should have very good ductility above 50 degrees C and maybe down to room temperature. We expect longer radial hydrides in M5 but with smaller hydrogen content they may not be as damaging as they look in the pictures.

What are the anticipated range of peak cladding hoop stresses? The computer code FRAPCON predicts for 400 degrees C and a temperature profile -- do it this way. For the rods, less than 54 megapascals for standard PWR rods and close to 90 megapascals for integral fuel burnable absorber rods that happen to have boron-10 on the inside, the neutron-born-10 reaction produces helium; for each reaction you get two helium atoms. Inside the rod, you have an additional source of generation of gas in this one particular design.

So, let's look at the database for rod internal pressure. Rose showed you some, but I want to show you in a different way with the same data point. This graph is too busy. We are not going to spend too much time on it. This is the graph that EPRI generated for pressurized water reactors. The data come from a variety of reactors within the United States and Spain. There is a variety of designs, but what I want you to focus on is the first two. The initial fill pressure when you fabricate the rods is as low as two megapascals and as high as 3.5 megapascals. This has a multitude of designs. It is a multitude of decades, '80s, '90s, so forth.

But I do want to focus on the burnup region from 40 to 60 gigawatt days per metric ton. It’s easier for me to do by simplifying this, leaving off a lot of this information. The data points I'm showing you, the initial fill pressures were between two and 3.45 megapascals. Between 40 and 60, even with all the differences in design, that the data average between 40 and 60 is about four megapascals. The three-sigma upper limit is about five. I've added to these EPRI data points that you saw in the previous slide the ORNL data points for the sister rods. They do fall below that five. Five is just a convenient number. It is not a disaster if you go to six. It's a nice number for calculation.

We will skip the data above 60 gigawatt days per metric ton. Those are all lead test assembly rods that were not designed to go to higher burnup. And essential what happens as you go to higher burnup, the volume inside the rod decreases and the pressure increases. So, the calculations I showed on the previous slide that were mine were based on five megapascals and they are upper bound because some of the rods started out much less than five.

I included a cup of FRAPCON predictions on here. And Rose pointed out that SR, I use for standard rods also looks like it stands for sister rods. SR means standard fuel rods that do not have boron-10 on the inside of the fuel rods, just UO2 pellets and cladding.

Okay. To convert from pressure to stress. Let PI be the internal rod pressure. P0 will be the external rod pressure, under vacuum it's about zero. In some casks designed for convective heat transfer it can get up to .7 megapascals, about 7 atmospheres.

This is the inner radius of the cladding and H is the wall thickness. So, calculating hoop stress is relatively straightforward. It is the ratio of the inner radius divided by the wall thickness times the pressure difference. You can forget the last term, that is just being pedantic on my part and insignificant.

So this could be anywhere from 0.7 megapascals and this would be somewhere on the order of ten to the 13 megapascals depending on the situation.

So how do these parameters change? Obviously internal pressure increases with burnup due to free volume decrease. And if you have IFBA rods you have helium generation.

This rod internal, I mean rod inner radius basically from zero to about 40 gigawatt days per metric ton. You have a higher coolant pressure that is creeping the cladding down onto the fuel. So, this actually decreases with burnup, up to about 40. That would decrease this ratio.

The wall thickness decreases with corrosion or oxidation due to coolant-side oxidation. So, with as-fabricated 17 by 17 cladding, which is what we have, you start out with a ratio of 7.3. This is actual data on ZIRLO. If you have 60-micron oxide layer it can go to up to 7.7. And a real upper bound would be a 100-micron oxide layer and you can go up to 8.3.

Any values I've given you for stress based on pressure, I have ignored these terms to get an upper bound and I've assumed that this is returned from creeping down to creeping out to its initial position.

Let's quickly review before we go into the results. As irradiated cladding, hydrides are primarily in a circumferential direction. This distribution of circumferential hydrides across the wall has a significant effect on ductility and radial hydride precipitation.

And we showed you short isolated radial hydrides in ZIRLO and M5 from pressurized water reactor fuel.

Conditions for significant radial hydride precipitation based on our results, which I haven't shown you yet. You need enough hydrogen in solution to form long enough radial hydrides that are continuous enough along the axial direction to get significant embrittlement.

To put 60 wppm in solution requires 285 degrees C. I'm just saying Peak Cladding Temperature should be above that. You need high enough internal pressures at Peak Cladding Temperature, high enough hoop stresses at that Peak Cladding Temperature, and also, I should say if I didn't point this out, I'm pointing it out now, the cladding microstructure is very important because the more randomly oriented grains are more susceptible to precipitating radial hydrides.

So distribution of circumferential hydrides at Peak Cladding Temperature is an important factor. Let me go through some results that form the basis for these statements. I chose to make the statements first and then show you the evidence afterwards.

All right. This is all ZIRLO results. What I'm basically changing is the peak stress during the simulated drying and storage period. So, if your ZIRLO is only exposed to 80 megapascals and you cooled slowly, you get very short radial hydrides. If you go all the way around the wall of the cladding looking at 30 images, they only come to about 9 percent of the cladding wall thickness.

So those are benign. This material if you subject it to the type of tests that we do, is highly ductile and behaves the same as as-irradiated ZIRLO. Going to 400 degrees C and subjecting it to that pressure and cooling, we could see no difference between the behavior of that material and the behavior of the as-irradiated material with comparable hydrogen.

We increase the stress. We go, let's call this 110 megapascals. What happens is now at this stress level you precipitate longer radial hydrides and more radial hydrides. If you go around the whole circumference of the cladding you jump from 9 percent to 32 percent of the cladding wall. That means effectively this length here divided by the cladding wall thickness is what I'm giving you.

The slides on the far right, the top one I must apologize. These are based on 100X images. We missed this area right here. In the 100X I'm showing you 200X image which is no longer 200X when I reduce the size but at 88 megapascals, you do see longer radial hydrides but only 20 percent of the wall thickness. You have ductility down to room temperature.

So, this is 88. This is 89. Same idea, if this is about -- this is the longest radial hydrides, about 36 percent of the wall thickness under these conditions, about 36 percent of the wall thickness. Both of them are really benign.

If you stay below 89 megapascals, with this material you have very good ductility down to room temperature. Those are tests where Peak Cladding Temperature was 400 degrees C. Things got a little more confusing in a way, perhaps less clear when we dropped to 350 degrees C as Peak Cladding Temperature. This is now 87 megapascals and I'm showing you the longest radial hydrides we observed going around the circumference. However, you are back to the 20 percent on average of radial hydrides if you go around. So, you maintain ductility down to 28 degrees C, about room temperature.

Interesting when we go from 87 to 93, the hydrides are much longer. And on average they are again about 30 percent of the wall thickness. You jump from 20 percent of the wall thickness to 30 percent. You jump about 100 degrees C in this ductility transition temperature.

And then this is 94 megapascals, the radial hydrides look even longer and the transition temperature is up close to 140 degrees C.

Those are what the images look like. What we show for ductility, let me spend a few minutes on this curve. This is offset strain, which is essentially how much did you displace this ring that we test. This is the ring that we test, we squeeze. How much did you displace this ring and before significant cracking, what was the plastic deformation of the ring, if you subtract off the elastic? And this is the test temperature. So, these are data, red means 350 degrees C, 400 degrees C in green, but essentially these are data in 87, 88, 89 megapascals. You have ductility down to close to room temperature.

As you cross over from 87 to 93 and 94, you come over to these curves over here. And you essentially have 100 degrees C increase in the ductility brittle temperature. This is clearly ductile. Below 1 percent is clearly brittle, between 1 percent and 2 percent is a transition region where we are not 100 percent sure that the material is ductile.

These are very artistic curves. They are best fits to three of the four data points. We don't have enough data points generated in the sister rod program. We are looking forward to being able to generate more data points and have repeat tests.

You'll notice strange behavior out here that I did not do a curve to. With high hydrogen content at 350 degrees C. That's the kind of test you would like to repeat a few times to get some statistics on it.

Okay. This is just another way of showing the stress sensitivity where I'm plotting the peak hoop stress and the ductility transition temperature and you see the sharp increase in our data. Okay. So future hydride reorientation testing. As far as the ANL cladding we’ve been testing, it's all from lead test assembly rods. It did not experience a typical recycle type of radiation where the third cycle is very low power.

We would like to clean up this issue at 350 degrees C and demonstrate whether or not there is such a sharp transition in ZIRLO between 87 megapascals and 93 megapascals.

For the sister rod cladding at 400 degrees C Peak Cladding Temperature, you should have more prototypic linear power histories, the fuel cycle is 18 months. Wider range of hydride distribution through cladding wall. More cladding samples available. That is very important, allowing for repeat testing and immediate temperature tests. It allows us to stop a test halfway through and examine, correlate the microstructure of the hydrides with the extent of the wall cracking.

And it offers the possibility of M5 with perhaps greater than 100 weight parts per million hydrogen.

That is my presentation. I prepared some discussion slides which were meant to help me answer some of your questions, but thank you very much for your attention.

>> BAHR: Thank you, Mike. Are there questions from the Board members? Paul?

>> TURINSKY: I noticed the radial hydrides tend to form deeper in the CRUD. Yet these were uniform temperature heating of the samples, right?

>> BILLONE: Yes.

>> TURINSKY: Why does it form deeper in the CRUD layer?

>> BILLONE: I thought it was the opposite.

>> TURINSKY: Maybe I'm oriented wrong.

>> BILLONE: I thought what I showed you ...

>> TURINSKY: What is the outside of the clad there?

>> BILLONE: I'm sorry. Move that toy. This is the inner wall of the cladding.

>> TURINSKY: Right. There's moreover over.

>> BILLONE: When you start with hydrogen very concentrated you have a lot of room, first of all, to grow these radial hydrides. If you are talking about this here, the high density.

>> TURINSKY: No, I'm talking about the radial hydrides on the bottom, they are deeper into the layer of the --

>> BILLONE: This is cladding. They are deeper into the cladding layer. They hit the circumferential hydrides, they can't easily grow continuously through them. Sometimes in the other picture, they come along here and you have another radial hydrides going along like that.

>> TURINSKY: But they start basically very deep into the cladding.

>> BILLONE: Yes. There's something about ZIRLO and I'm hoping to resolve this with the sister rods. We see this with this one

alloy. We see it with an alloy with most of the hydrogen over here.

>> TURINSKY: Right.

>> BILLONE: There may be fabrication differences between ZIRLO and others that Westinghouse knows and we don't know.

>> TURINSKY: Second question. You talk about hoop stress. Are there stress risers to give places like -- give stresses like cracking and bonding?

>> BILLONE: All our tests are defueled cladding. Oak Ridge will do testing with fueled cladding.

If you had a pellet clad interaction crack, even at the lower stress levels, you have a stress concentration and a radial hydrides concentration at that point. We have never run into it in any of our samples. You are absolutely correct.

>> TURINSKY: That would be a very difficult thing to model?

>> BILLONE: Yes, first you have to find it.

>> TURINSKY: The crack models tend to be the sort of homogenized models.

>> BILLONE: Right.

>> TURINSKY: Okay.

>> BAHR: Mary Lou?

>> ZOBACK: That was a nice talk but I want to make sure I understand. So, the radial hydrides are beginning from the interior and radiating out. So, the hoop stress, the interior is

pressurized. The hoop stress, the circumferential hoop stress is extensional around the inner diameter. Are these things related to extensional stress? The hydrides forming?

>> BILLONE: I should have mentioned one thing. The stresses I'm showing you are average across the wall. The hoop stress is higher on the inner surface. We're using the higher hydrides and lower on the outer surface. Maybe by 10 percent.

>> ZOBACK: So, are these stress induced? That's why they are --

>> BILLONE: I'm not sure because we don't see it with the different cladding alloys. The Zircaloy 4 we see a lot of hydrides intermediate in here.

>> ZOBACK: They don't all begin at the inner wall and radiate out?

>> BILLONE: The M5, I didn't show you pictures, you tend to get long radial surface from the outer surface growing in.

(Overlapping speakers.)

>> ZOBACK: I see.

>> BILLONE: They each have a different microstructure, some mystery to them in terms of the fabrication process because that's not something the vendor shares. It's tough for us to pinpoint the differences.

I have a question I keep asking myself, sorry, I just would like to know if the different behavior we observe is the alloy itself and the finishing process or is it the fact that some are high power, some are low power and that's what I'm looking forward to with the sister rods.

>> ZOBACK: Thank you.

>> BAHR: Lee Peddicord.

>> PEDDICORD: When you showed some of the other pictures, maybe you even had a number in there of how often you see these. Did I see a number that was typically nine?

>> BILLONE: The as-irradiated? Okay. Depends on how many you take.

>> PEDDICORD: Yeah.

>> BILLONE: All right. So, this is part of our LOCA program. We only did images at eight locations around the cladding. Like one of the eight we saw that picture on the far right.

>> PEDDICORD: Okay, thank you.

>> BILLONE: This is something we just did recently and we took 12 images and maybe three of the 12 had those radial hydrides. It is interesting that they are also at the inner surface, where the stress would be higher if the fuel pellet were pushing on the cladding and during cooling they are trying to keep track but still have stress.

>> PEDDICORD: You don't have enough low burnup examples to see when these might form? When you examine the creep down as you talked about.

>> BILLONE: The lowest one we examined was for the low burnup demo program which had cladding at 36 gigawatt days per metric ton – in that case the fuel-cladding gap wasn't completely closed for those rods. We didn't see any of this type of behavior. And you wouldn't expect pellet clad interaction. So, it's kind of a gap, I don’t like the word gap. We looked at 36 and all the rest of our samples are between 57 and 70. We are missing the range you're interested in.

>> PEDDICORD: Thank you.

>> BAHR: Efi.

>> FOUFOULA-GEORGIOU: Efi Foufoula Board, in slide 14 you have the data and you have some what you call artist’s curves – of course they are not fit to the data, but you implied two regimes going from the 87 to the 93 pressure. My question is, you mentioned that lots more experimenting already done in the higher-pressure regime.

>> BILLONE: Right.

>> FOUFOULA-GEORGIOU: Do you expect when you are unfold that space, a lot of complex behavior? I mean, it seems to me that you will not be able to even collapse in the two-dimensional space that you saw here. Could it be several regimes that you need to unfold and say higher dimensional space? That is temperatures separate from pressure and so forth, to see the relationships? Is that right?

>> BILLONE: I think these data are very useful guides. If all of these stresses that we generate are less than 87 megapascals, then there might be scatter. We might see 5 percent ductility. You might see 7 percent, but you wouldn't see this transition necessarily.

I wanted to make a point about this curve. This high hydrogen, we just recently tested this material with about 650 weight parts per million in the as-irradiated condition. This is the material with the thick hydride rim on the outer surface. That had like five to 7 percent ductility at room temperature -- didn't behave anything like this. This behavior is clearly a consequence of those long radial hydrides that I showed in one of my pictures. We will get variability -- changes that result also based on the fact that we get a wide range of initial radial hydride distributions across the cladding wall. I expect to have this picture change a little bit. But for now, in the short-term I would really like to just run two more tests. This high hydrogen content material at the lower stress of 87, that red curve over there with the lower hydrogen content at 93 and then we close out the Argonne testing and say wow, we found something fantastic, interesting. This ZIRLO has a high stress sensitivity in that narrow stress regime.

>> FOUFOULA-GEORGIOU: Thank you.

>> BILLONE: Again, this is all a guide, all to be used as a guide for the future testing.

>> FOUFOULA-GEORGIOU: Exactly, right.

>> BAHR: Susan Brantley?

>> BRANTLEY: Thanks, everyone, I enjoyed your talks. I'm curious, are there models being developed, like material science or metallurgical models to predict hydride formation?

>> BILLONE: That's a difficult subject. There's -- forgive me, I call them the animal code. I know the National Laboratory has codes called moose, bison, marmots. They have universities working with them. Penn State currently has an integrated research project, so they have a team of people, including Westinghouse.

Part of their work is to improve the modeling of hydrogen, dissolution, precipitation, orientation in these particular codes.

So there have been modeling efforts going on. I'm very interested in the Penn State one because I'm on the advisory Board and I am the DOE technical point of contact. I review their quarterly reports.

So, I have input to that one.

>> BRANTLEY: As a Penn Stater, I'm glad to hear that. I wasn't trying to get you to--

>> BILLONE: Art Motta is who I work with.

>> BRANTLEY: You mentioned texture and that's a piece of the puzzle here. Could you talk about the texture a little bit?

>> BILLONE: Texture refers to grain orientation.

>> BRANTLEY: I understand. How does it fit in? You probably have some thoughts about it.

>> BILLONE: I glossed over it. For the cold work alloys, those are fabricated and textured in such a way that most, 99.9 percent of the hydrogen will precipitate in the circumferential direction. That is relatively benign, based on grain orientation and how easy it is for hydrides to precipitate on a grain phases. The grains are kind of elongated and aligned in such a way to minimize radial hydride precipitation for in reactor operation. In the M5, the one that is crystallized-annealed, you will see more radial hydrides. What saves M5 is that the hydrogen content is low ...

>> BRANTLEY: Interesting.

>> BAHR: Paul Turinsky?

>> TURINSKY: I have a basic question. We now have these road tests and know basically what the displacements of the rods are, the only place they are going to pinch is at the grids. Assume we get into the brittle regime. Given that data, will the rods fail?

>> BILLONE: I'm going to give you a probably not because I should have had it on my slide or I didn't read it. We have a

three-pronged approach. Argonne and PNNL are going to be doing the testing of cladding without fuel in it called bare cladding. That material has strength. It may have reduced ductility, but it has strength. If you now put fuel inside, I once used the expression it is folly to rely 100 percent on fuel because it is fickle, but the fuel should support the cladding. And if you put a particular load on the cladding with fuel in it you should get less displacement from it.

The third thing is the transportation loads themselves. Sandia has done experimental work; Pacific Northwest has done the analytical work modeling.

As far as the vibrational type loads, what they are measuring for normal conditions of transport is very small, way within the elastic regime. The next step is to simulate experimentally and analytically the one-foot drop, the side drop you would be getting, some contact between the fuel rod and the wall. It may be possible that the pinch load we refer to, they may be small for normal conditions of transport and it may also be true that this added margin or added feeling of comfort, the fuel does support the cladding to keep it from deforming too much. That all works in the same direction of making it more of a benign situation.

>> TURINSKY: Actually, under --

>> BILLONE: I think that's where we are going.

>> TURINSKY: Under the grids it is less irradiated because there is less of a dip because of the parasitic capture.

>> BILLONE: You might have more hydrogen under the grids, lower temperature. It will be interesting to see that study.

>> BAHR: Other questions from the Board?

Questions from the staff? Dan Ogg?

>> OGG: Yes, Dan Ogg, Board staff. Mike, you showed a lot of data here for internal rod pressures on standard rods. You've shown a few data points, I believe that all were calculated for burnable absorber type rods.

>> BILLONE: Yes, Westinghouse has not publicly released any data for the IFBA rods, the boron rods.

>> OGG: Do you know of any plans to get data on burnable absorber rods?

>> BILLONE: NRC made a deal with Westinghouse, in order to improve the FRAPCON calculations for those type rods, to benchmark it, they had Westinghouse provide some data confidentially and PNNL used that data to benchmark their helium release model. That's the best we really get.

>> OGG: As part of FRAPCON?

>> BILLONE: Yes.

>> OGG: Basically, the answer is FRAPCON has been validated against some --

>> BILLONE: Limited type.

(Overlapping speakers.)

>> BILLONE: Right. I should have mentioned that this boron-10 burns out in the first 18-month cycle. So whatever increase in pressure you get, you get in basically the first cycle. That doesn't mean it's insignificant, but the vendors are very clever. I mean they use annular pellets; at the end the blanket pellets and they make design modifications to increase the initial free volume a little bit to compensate for it.

I just like to see the data to give me a warm fuzzy feeling.

>> OGG: Right, right.

>> BILLONE: But it is not included, those rods are not included in our sister rod program. They are of great interest, along with advanced cladding alloys being used now like optimized ZIRLO which I haven't mentioned.

>> OGG: In fact, that leads to my next question. Do you know of any plans to do testing on other cladding types or to somehow relate your results to the performance of cladding of other types?

>> BILLONE: Okay. Let me partially answer it. One good thing about the Penn State program is they partnered with Westinghouse. Westinghouse provided them with ZIRLO that has a microstructure all the way from recrystallized–annealed, to partially recrystallized–annealed. That's what they call optimized ZIRLO, nobody knows what that means, all the way to fully cold-worked

material. What I have pushed for is for that program to study the effects not so much of alloy composition but of microstructure and texture on this radial hydride stuff.

So, before we get any irradiated material or have plans for irradiated material which are not really in progress, we will be watching closely the results of the Penn State research. I'm using that easily, they have a team of people they are working with, so the Penn State team.

And I am pushing very hard for them to really focus on that because it is a hole in our work. We don't have anything in between.

>> OGG: Right, right. And then even going further, can any of this be applied to BWR fuel or does it need to be applied to BWR fuel?

>> BILLONE: When we look at BWR fuel and talk to industry who has this IFR program which is confidential, this research project, if the BWR cladding has a Zirc liner, the hydrogen tends to migrate to that liner and your internal pressures are less. The starting pressure when they fabricate the rod is less because the coolant pressure is so much less, the gas release tend to be higher. But we don't see the pressure getting up high enough to cause an issue.

>> OGG: Right, thank you.

>> BAHR: Other questions from the staff?

Another one from the Board, Mary Lou Zoback.

>> ZOBACK: This is a general comment for all the speakers this morning. I want to thank them. They were all very clear presentations. I appreciated that they started at the beginning a sort of described what the overall problem was they were trying to solve. And I also appreciate the attention to acronyms because that is a real problem for me. So, thank you all for doing that.

>> BILLONE: Thank you for the comments.

>> BAHR: Okay. Do we have any questions from the audience? For any of the speakers from this morning?

Seeing none, I think we do have a couple of public statements. I don't have the listing, but that is coming to me.

We have on the schedule about 15 minutes at this point. And we have four people signed up. That will give you about three minutes a piece if we can do that. The first person is John Ombusher from the Sierra Club.

>> AUDIENCE: John Ombusher with the Sierra Club here in New Mexico. I have been working with a group of nine other folks for the last year on nuclear waste disposal issues and I distributed a copy of the letter we sent in August and I'm going to grab pieces of that because it is long. The biggest question is in a using San Onofre as an example, there is no more pool there. They have a bunch of cans of fuel.

If there is any damage to those, then what happens next? What is the -- that leads into the whole rail transportation plan where New Mexico is targeted with Holtec and other facilities nearby. We have a D minus rating from the American society of civil engineers for our rail infrastructure, which would be great if we could get all that money coming into rail. I really love railroads. I think they are very efficient.

So right now, Holtec has a return to sender concept on anything that is received there if it's damaged. So, returning it to San Onofre which has no fuel pool, no hot cell, what is the strategy for providing an infrastructure of hot cells or I have listened in on the last couple of Board meetings. It's all been very interesting, and thank you for working on hot cell or excuse me, on high burnup fuel, which is a much-needed research area.

What then happens? So much of the information available is proprietary, so it makes it difficult for the public. It is already a highly technical subject but it does make it more difficult for those members of the public to understand what is going on.

So, I think I have probably -- oh, and the 30-foot drop on the horizontal, that seems unrealistic potentially. And I can see how simulations on the rods themselves, on the hydrides, et cetera, is difficult. But what about failure modes on the frame structure within the casks? That seems like an opportunity for good simulations to figure out are there modeling failures that might lead into further research on physical models? Thank you.

>> BAHR: Thank you for that comment. I don't know if there's anyone from DOE who wants to respond to any of those. I think some of the Holtec, I believe that would actually be a private transport. Bill Boyle can maybe ...

>> BOYLE: William Boyle, DOE. Thank you, chairman Bahr. My answer is going down the same path as yours. When it's storage as San Onofre, that's between the utility and the NRC. When it's in transportation, it is whoever the utility got to transport it and the NRC. And Holtec taking it in storage, that's between them and the NRC. DOE has no involvement.

So, I don't pay attention to it.

>> BAHR: Okay. Patricia Cardona, also from the Sierra Club.

>> AUDIENCE: My name is Patricia Cardona with Sierra Club. I work on legislative issues within the state of New Mexico. The state of New Mexico I think is very concerned as far as the citizens go about what is happening. One of my main concerns is even in the testing and the samples that have been taken, everything is out of context. There's no age or condition of any of the rods or any of the samples, where they've come from, their age, or anything like that that can give us any kind of look at what is the real conditions under which this testing, what is really being tested.

The other thing for us is storage. It’s not only the transportation but the fact of the context of the storage location. New Mexico has both Urenco and WIPP, we have radiation from that area and winds in that area that are very strong.

The addition of all of the radiation from all of the U.S. and possibly from Europe coming into this area is going to have devastating effects on the economy of New Mexico, not only in terms of healthcare but also in terms of the location of that area impacts the dairy industry that we have, the farming industry that we have, and also cattle industry. So, you know, I would be very helpful -- it would be very helpful if the technical Board would look at the actual context of where they are proposing to put something and whether or not it is appropriate.

Thank you.

>> BAHR: Thank you for that comment. I just note that at least under the current legislation storage and particularly private storage is not within the purview of this Board.

I think as far as the age and the history of those fuel rods, I believe there is a fair amount of information on that and DOE could certainly provide that.

Next is Constantine Borgadini, Decision Sciences?

If you use the microphone up there?

>> AUDIENCE: So yeah, my name is Constantine Borgadini, and I represent Decision Sciences, a commercial company which is a provider of tomography technology. I don't know how many of you are familiar with this technology and know its capability.

I don't see any hands raised. So, with these technologies, basically we can image different materials by looking at the interaction of cosmic rays, in particular cosmic rays' muons with different materials in different objects.

We have commercially available system for border security. And we believe with this technology it will be very useful and applicable to imaging of spent fuel in casks without opening the casks.

The reason for that is first, muons, cosmic muons in particular are penetrative and our technology is very sensitive to special nuclear material in particular and we can image the materials as well.

So, this technology has been developed at Los Alamos National Labs. I have been working at Los Alamos for about 16 years. When I moved to work for this company which commercializes it. What we would like to do is, we want to develop with the technology and provide with capability for imaging of nuclear fuel in particular, in casks. It could be applicable to storage, to transportation and in other scenarios.

What I think we will lack as a company is enough expertise to understand where this technology could feed as a piece of this -- fit as a piece of this big puzzle which you all are trying to solve. We would like to have more engagement with the community of experts who understand what are needs and together hopefully we can develop this capability which I think potentially could be very useful for this area.

>> BAHR: Thank you. And then finally, Bill Strummels from the town of Pahrump, Nevada, and the nuclear waste environmental Advisory Council and something else.

>> AUDIENCE: I'm also on the advisory Board to the U.S. Nuclear Energy Foundation.

Hanging over those proceedings is the ultimate storage site, living 25 miles from Yucca Mountain in Pahrump. I'm here to state that in spite of the NIMBY nonsense, Nevada is still in play. They now have a lot of egg on their face with news that weapon grade plutonium shipments will be sent into the Nevada National Security Site despite 35 years of chest pounding to the centralized repository. Weapons grade plutonium is going to be moved over our public highways to the Test Site with no project, no public work and no infrastructure plumes to show for it. There never has been a popular referendum on Yucca Mountain in Nevada because, with Harry Reid's obsession with stopping the project, it must be bad for us.

Any lay person, that's what I am, not an engineer or scientist but very impressed with the technical analysis presented today, has the sense to know there is no negotiating the commerce and supremacy clauses of the United States constitution. They are absolute. And without a project to mediate over, Nevada has no leverage over the federal government. My discussions with local officials, state bureaucracy below the titular heads in Carson City and even the Las Vegas mayor's office reveal frustrations with the obstinacy of our federal representatives, Senators and the governor. Their refusal to deal with the federal government places constituents at greater peril and leaves our water and infrastructure in a perilous state.

I should add New Mexico did have a project, the WIPP, to mediate over. As a result, they negotiated that they would accept transuranic waste but no high-level waste into the WIPP. That's a contrast between the intelligent way New Mexico deals with the will federal government and the nonsensical approach Nevada has thus far taken.

>> BAHR: Thank you for that comment.

Okay. Thanks to the commenters for keeping on time. Do we have any final comments? I would also like to thank this morning's speakers and look forward to another set of interesting talks this afternoon. We will reconvene again at 1:10. That gives you just about an hour for lunch.

(The lunch break was taken at 2:10 p.m. EDT.)

(Silence.)

(Silence.)

(Silence.)

(Standing by.)

(Standing by.)

(Silence.)

(Standing by.)

(Standing by.)

(Standing by.)

(The meeting is about to resume.)

>> BAHR: Okay, welcome back from lunch! We're going to start our afternoon off with a movie presentation by Sylvia and Nicholas who has been working on this multimodal transportation project and we have heard about it at previous meetings and we're going to get an update now that they have a lot more data, right?

>> SALTZSTEIN: My name is Sylvia Saltzstein. I work at the Sandia national labs and part of my job is to herd the cats you heard this morning, all very experienced, dedicated and professional people too and I'm very fortunate to be able to lead them and hopefully put all of this pointing in the same direction. That direction is to collect data in order to develop the technical basis to show that the storage and transportation of spent nuclear fuel is safe. So, for the areas in which we have concerns, we'll delve into those through research and development deeper in the future but that's our purpose, that's our goal and why we're here.

We're going to talk about today and I'm just setting the stage is the multimodal transportation test and I'm going to quickly just sort of show you what the end data turned out to be and then give you an overview of what the whole test was. That will be a movie because everybody likes a long movie after lunch. Then Nick Klymyshyn from PNNL is going to talk about the analysis and modeling we have been doing with the 8 terabytes of data that was collected during this. I also want to point out Elaina Kalinina and Pat in the middle here who did the lion's share of the analysis of all of this data.

They're not speaking today but they have devoted enormous number of hours into making sense of all of this information. So just very quickly, you'll see this in the video but this multimodal test was a collaboration between the United States, Spain and Korea. We started out with a truck test through Spain. Started in the northern part of Spain, down to the middle of Spain and back. We then took a barge up the coast of Europe with spent fuel --- all surrogate spent fuel, no real fuel was harmed during this test. It was all fake fuel. Then from Belgium, an ocean liner across the Atlantic, switched to a train and over across to Colorado where the American Association of Railroads and the Department of Transportation have an equivalent of a National Lab for rail testing.

We could put our cargo, our transportation system through very controlled normal conditions and extreme normal conditions of transport to really collect specific data and then everything returned back. I'm going to tell you what we learned and then we'll go into the details so you can keep this in your mind as we go through all of the details of what we did. The end result, well, this pointer isn't working very well. So, if you look here, this is the failure point for high burnup fuel based on data we have so far. We'll get more data on that in the next few years with the high burnup demo. I'm sorry, this is the yield point for low burnup fuel. This is the yield point for unirradiated fuel. We have done two shaker table tests, a truck test and now this multimodal test and all of our data falls down here. This is about 9400 micro strains for a yield point for high burn up and 9 thousand micro strains for a yield point for low burn up and we are around 100 micro strains for what we tested during our multimodal transportation test.

Another way to look at the results, we have failure information in the literature. Oh, where did that come from? So, failure information in the literature for all types of fuels. Most of this has not gone through drying. The large circles are the Oak Ridge CIRFT fatigue test results. Red dots mean the fuel failed during the fatigue test in hot cells. The green means there was no failure. And we have these after a million different fatigue cycles. Still no failure or some failure up here.

Our results, thank you, our results here are along the 100 micro strain level for our accumulation of tests we have done. We estimate the shocks around here, for a two-thousand-mile trip a little over 10,000 shocks. We have this margin of safety here. In terms of vibration, we estimate about a million vibrations in a 2000 mile cross country trip. At 100 micro-strains, we have this margin of safety here.

The take-home message is, our data is showing and this 8 terabytes of data we just collected confirms this. We have a large margin of safety in terms of the shock and vibration that the spent fuel will experience during a normal condition of a transport trip.

We can now start the video. I will narrate through some of the video. It's already narrated but I'll add a little more commentary. So these are our collaborators. We went through a big storm in the middle of the Atlantic. We were able to collect some good data there.

(Video played.)

So, this is one of the surrogate assemblies. This is a concrete dummy assembly and this is the assembly being loaded into the cask in Spain and the ENSA facility. The rods for the most part on the Sandia assembly are copper cladding with lead rope. We do have some lead pellets. We do have some Moly pellets.

(Video continued.)

So, this is to simulate normal operations. We had them set it down gently, medium and hard. Each crane operator has a very different technique and feel. This is interesting to see. This is us loading it on the cradle. We were able to shake it a couple of times just to get a little bit more excitement in there.

The Spaniards are most likely only going to transport by truck. That's why they care about this. They donated the cask to us. That was their in-kind contribution. You can see it was 16 axle truck. It was so heavy we had to have a truck on the back to help push it up the hills. It was 150 ton. So, they call this a barge. So that's our barge.

They placed our cask in the very center on the bottom of the ocean-going vessel and the barge and you can see how securely they tie it down. Then again, we went through a wonderful storm. These are the batteries. This is 20 marine batteries to power this and we couldn't be with the assembly. We couldn't be with the whole system so at each port, we had to let it go. It had to be completely self-efficient, collect the data and the batteries just had to work. It was nerve wracking just to have it go for our one-time test and make sure we got data at the end.

All of the batteries and the data acquisition are in the box in the back. That's another GPS system so we can watch it. That's our data acquisition system. 40 channels each from Siemens. I got to drive the locomotive. It was very fun! So, this you could go 75 miles per hour. That's faster and around curves and much higher speeds than you would go normally. They have manufactured defects in the tracks so you know exactly what the defects are for the modeling so you can really understand what those shocks or vibrations are coming from. This is a coupling test where they roll the car down to connect it to another. A reportable incident for that is 4.5 miles per hour and we got up to almost nine. Yeah, they crash into each other. So that's the overview.

And now Nick will talk about the eight terabytes of data we have looked into and analyzed and PNNL is modeling. Thank you!

>> KLYMYSHYN: Hi, I'm Nick Klymyshyn from Pacific Northwest National Laboratory and I'm a structural modeler and engineer. I have to admit that the transportation is boring. The loads were very small. I'm going to repeat many times through my presentation how small the loads were. But this is all very good.

The reason we did this test is because it fits in with all of the DOE goals of eventually being able to say we can transport spent fuel with no damage. And the transportation test determines the loads and it recorded the shock and vibration loading environment. It measured the cladding strains. We used that data to validate structural models so we can use those models to predict what will happen on other conveyance systems for other fuel types. That ties in the other work you have heard about today, Brady and Sam talked about the demo and thermal modeling analysis, testing Rose mentioned the CIRFT testing which I'll talk about in future slides and the sister rod mechanical testing -- Rose and Mike Billone talked about that. All of these pieces come together to lay the technical foundation for having confidence that we can move fuel without breaking it.

So, this test campaign involved a large number of people, two or three countries, many staff. The key reports on this work are, I listed three of them here. The McConnell report talks about the test plan. The Kalinina report talks about the analysis of the data and my report, the Klymyshyn report, talks about modeling analysis that uses the data to validate our models and estimate what is happening.

My presentation is basically a summary of my report. It should be available in the near future. My objectives are to show you some of data but just a small subset of it. 8 terabytes is too much to go through after lunch and really, I want to provide context and perspective for the test results. These loads are very low. The strains are very low. That also means that the stresses, the loads, the deformation energy on the cladding are all low which is interesting. And then finally the progress of the structural modeling. We validated some models and we performed a fatigue analysis and there's some next steps to talk about.

So, this could be my best slide. So, this is a piece of fuel cladding. It doesn't have any fuel in it. It is as manufactured. This segment is about 18 inches long which is give or take the size of a span. Some spans are smaller, some spans are larger. That's the span between the spacer grid. One of the things that we learned from the structural modeling is the deformation energies are so small they're only about one millijoule. I came up with a chart and we're talking about one rain drop. Imagine the kinetic energy in one rain drop, even an Albuquerque rain drop. Imagine it hitting this section of cladding. This is a metal tube. You can thump your finger against it. Do you want to pass it around? Show and tell. Be careful with the ends. There's nothing in there (laughing). So, the point is, the Board has been interested in this work and a lot of the technical questions can be resolved by the fact that the energy that we're talking about is so small. We're talking about a rain drop worth of energy so it doesn't really matter if it's surrogate fuel. Oh, that's not allowed! (Laughing). So, whether it's irradiated fuel or the surrogate fuel that we used, when the energy is this low, there really isn’t any reason to be concerned about the cladding failing during a shock or during a fatigue failure of a long cyclical excitation.

So, I selected the data set from Baltimore to Pueblo. That's an open rail test similar to what a real fuel handling system would experience. I'm going to talk primarily about the cladding strains because it gives an indication of how much bending happens in the fuel, what the load is on the fuel. Accelerometers are interesting, they help validate the models, but if you look at the accelerometer data, I would need another hour to talk about it.

One thing I want to point out, I'm going to talk about strain in units of micro strains using the symbol uE. That's not an SI unit. That's a shorthand way of talking about strains. A microstrain is equal to one-one millionth of an inch per inch or one ten-thousandths of a percent. Very small strains. The kind we're talking about is in a normal stress analysis of cladding, they would fall in the second and third significant digits when you're reporting stress.

This is a graphic that illustrates the trip from Baltimore to Pueblo, the time axis in hours and it takes about six days of time and it happens in a two day stretch until it gets to St. Louis and then another two days to get to Pueblo and there's another wait while they transfer it to the short line in Pueblo.

Focusing on the bottom, these are the peak strains in any strain gauge in a one-hour block of time. So, this shows you the relative magnitude of the strains.

The peak is 46. We know that peak is actually caused by power line noise. When the train passes underneath the power line, the strains are so small that they pick up the electromagnetic interference of the power line so it records 46 but we know it's really about 37. I didn't correct this plot to make this point, that that kind of uncertainty really doesn't matter much. These are still strains whether it's 46 or 37, it's the same thing.

The strains are very low. Here on the left is a sketch of the fuel basket and the three instrumented fuel assemblies were placed in these locations with the dots. I had to color code them to keep it straight in future slides. On the right, these are sketches of fuel assemblies and each one of them has a strain gauge at approximately the same location. So that's what I selected to show you today. They are directly comparable. Right here, these are time histories of a one-hour data block and this block is interesting because this is the place where the cask experiences its highest acceleration levels. We trace it down and found this happens when the train crosses a road near St. Louis. We have a Google image of the location. We can see exactly what is there.

If you look at the data, you can't really tell there's a significant shock there. This one, where the Korean assembly is, it might be the power line noise. It goes a little higher than plus or minus 20. One thing I want to point out is, you can see a little separation between the higher peaks and the lower baseline kind of noise. Every time we try today trace one of these down, we found it tracks to a feature in the railroad. So, something happens in the railroad, it perturbs it and goes through the suspension system of the rail car, goes up and the fuel is affected. Over here, I marked the point of 36.3 hours and that's pointing to a relatively long stretch where nothing happens.

And in that stretch, the strain cycles are within plus or minus one micro strain. When I talk about fatigue later, I will mention for some of these real small cycles, there's 50 million strain cycles. Well, these are tiny. These are below anything that would ever matter. So, counting the number of cycles depends on where you want to draw your line of what is a reasonable cycle or not. What is meaningful.

This plot shows the power spectral density of the strain gauges at that road crossing. It's a ten second period of time and it's plotted in the frequency domain. So, it's decomposed into its frequency content. This is one of the Board's questions was about the frequency.

I want to point out here this peak is the suspension system of the rail car. It's around 2 hertz. Any type of rail car is going to have something there. If you look to the right between say about ten and one hundred, there are some spikes. There is some higher frequency content there and we believe those are the spans, the fuel rod spans vibrating at their own natural frequency which is a little higher than anything else.

So, the question was, whether or not the fatigue testing was valid because of fatigue testing was done at a lower frequency and this is one of those questions where the energy levels are so small. Really, it's not a major concern. That's where we just don't expect any difference between the high frequency fatigue behavior and the low frequency fatigue behavior and because the energy is so low, it really doesn't make a difference.

In this plot, I wanted to point out that all of the separate tests that were performed at TTCI at their protected track system, the coupling impact and crossings, we see all of this in the open rail data. So, if we need to do a study of predicting with great certainty and great precision, what the fatigue loading is along a certain route, we could do that by simulating the different tests that were done and then using that information to extrapolate what an open rail event would be. Because the strains are so low, we're not likely to go very far down this path but if we ever need to, we can. Now, I want to talk about the progress we made in structural dynamic modeling and that involves the NUCARS code, which is a rail dynamics code, it’s specialty finite element code they use in the railroad industry to predict how a rail car would behave. We also use LS-DYNA, an implicit finite element model, to model a fuel assembly and a single fuel rod. And even one mixed case where there was a column of fuel rods.

We found that because the strains are so low, it really doesn't make sense to use the bigger, full fuel assembly model, that a smaller single fuel rod model can adequately give you the information you need to predict the response of cladding. We also did analysis to investigate different components of the conveyance system, including the simulated fuel assemblies that Sylvia pointed out and we performed a modal analysis of the cask and cradle system. So, here's the minimal model architecture. It's two parts. There’s the NUCARS model that includes a representation of the cask and cradle in it. We run the NUCARS model over certain features. We extract the response of the cask and we apply that response to the single fuel rod model in LS DYNA. That couples the two codes together and transmits the information needed to estimate how the cladding responds.

Here is a validation of that model using the test data. At the bottom are the actual strain gauge data. At the top is the baseline two-part model. You can see that it does predict strains that are higher than the others. That's okay because we're down in the rain drop arena. If we need a better precision, we could figure out how to get there.

Another one to point out is a one-part model where we directly use the accelerometer data from the cask to load the single rod model and that matches very well with the actual data. The difference here is that we won't always have accelerometer data to use, the relative uncertainty in this one is due to the NUCARS model. If we're looking at a systems, we have to do this modeling approach to make the analysis.

Here I wanted to point out, we did a lot of work on the simulated fuel assemblies just to confirm they did not alter the test data. What we found is the assimilated fuel assembly had a real range and the dampening we would expect from the simulated fuel assembly is very small, on the order of 5 percent. Not very much in the system.

So now the fatigue evaluation was a big one. A fatigue failure happens when a material is loaded cyclically, imagine a paper clip. You can bend it back and forth once or twice and if you keep bending it, it will eventually fail. That's a fatigue failure. We know that the strains were not anywhere near the single cycle failure. We wanted to make sure that the small cycles didn't add up to cause a larger fatigue damage.

Let's see. I will skip to here. These are fatigue curves--SN curves, with the strain amplitude on the left and the cycles to failure on the bottom.

The blue curve is what I call the O'Donnell curve. It was published in 1964 and it’s a fatigue design based on test data. The NRC curve in orange is a new curve that's been derived using the CIRFT test data and it is being presented in NUREG-224 as a new design curve to use.

You can see the orange curve is above the O'Donnell curve. Well, that means the damage calculated is the O'Donnell curve. So, if I switched to the NRC curve, there would be even less damage than what I'm going to talk about.

Another point is the white dot. This is the end of the NRC curve. I asked them questions about is that an endurance limit? Are you calling it an endurance limit which means anything below this isn't, you just wouldn't count it. In my case, I counted everything. If this is going to establish that as an endurance

limit, there would not be a need to count any of it. When you do the fatigue analysis, use the SN curves to determine how much damage each little strain cycle does. It ends up like this.
A value of one would be failure. That means you exceeded your fatigue life. Here, we're ten orders of magnitude below one. So rather than reporting the numbers and comparing one strain gauge versus another, it's more appropriate to say there's approximately zero damage. The strain was so low it's not in a range where it threatens a fatigue failure. This is an example of the different strain cycles that were counted. This is in the full westbound open rail trip. The amplitudes of each strain cycle is on the bottom. The number of strain cycles is on the top, and it’s a log scale. So, at one micro strain, there's 54 million cycles. When we counted up every individual cycle, we rounded up to the nearest integer for easy visualization in a histogram. So, some of these are less than one. If you wanted to, you could just throw those out. One point though is, I kept them in. They didn't make a difference. They're so small compared to the SN curve that their accumulated damage is infinitesimal.
The other thing I did was I kept the power line noise cycles in. We know that all of these are attributed with power line noise but again, they're so small they don't change the final analysis.

So, one of the questions the Board asked is how do we account for irradiated fuel when we tested surrogate fuel?

I did this two ways. One is to test it as a perfect analog and calculate the fatigue damage and then I adjusted that count to account for the differences between real and irradiated and non-irradiated fuel and that involved increasing the number of cycles and increasing the vibration amplitudes.

For one set, I had a modeling basis where I looked at old structural models to predict what the amplification would be and, in another case, I looked at transmissibility to see theoretically how high they could be. When we go into the theoretical realm, we have to remember there's only so much space inside of the fuel basket. So, if you amplify the cladding deformations, you can get to a point where there just isn't enough room for it. So here, this is the first row and that's approximately zero damage. Model base estimate adds a couple of factors. They don't change anything. They're far below one. The damage is far below one. Let's skip to the bottom. I assumed a resonance effect with one percent dampening as an upper bound estimate but this isn't practical because this would require about 25 millimeters of room. There's not 25 millimeters of space available. So, I was looking for what it would take to cause a damage fraction greater than one and it turns out we can't get there. This leads to some of the follow-on work where we want to confirm we can't get there. We can't get to fatigue failure.

One of the big ones is to evaluate the Atlas rail car. It has a different configuration than the as-tested configuration. You can see in the cradle, this is the cradle that was tested. And the Atlas rail car assumes cradles with a different kind of shape and stiffness and their cradle is mechanically attached to the rail car.

The ENSA cradle design was designed so it can go on any flat deck rail car or any heavy-haul truck trailer. It was designed with more flexibility and when we use it, the standard shipping arrangement is to place this heavy structure on rubber mats. So, there are rubber mats in the test that would not necessarily be in the Atlas rail car configuration.

So, we wanted to do some modeling, use the validated models we have developed in this work and confirm our suspicion that it is not going to change anything if we're talking about the Atlas rail car.

We also collected test data recently looking at the rubber mat material and also plywood material and also just bare metal. This is a heavy steel block that had the same kind of padding material underneath it as was done in the rail testing. It was put on a shaker table and they did frequency sweeps to determine the different dynamic properties of this material. So, this year we're going to analyze the data and see what it tells us.

A few other things. We want to look at a canister system. We don't expect a lot of difference in a canister system but there's some additional analysis we want to do to complete that task. We also want to look at alternate fuel assemblies and in particular, BWR rods that have partial length rods would potentially give a little bit more space for rod flexure. So that's one of the things we want to look at based on the analysis I showed you a couple of slides ago.

Finally, we want to evaluate the information coming in from the sister rod tests, if there's something unusual that comes up.

One thing we want to look at is the 5-millimeter pellet gap that Rose talked about earlier today. We want to see what that would do to the stresses and strains.

So, this is just some conclusions here. The shock and vibration loads are approximately zero. There is some shock and vibration but it's so small that you really wouldn't count it. Cladding fatigue damage is below the practical endurance limits meaning that all of the tiny strain cycles that we counted don't amount to anything and the peak strain energy is approximately that in a rain drop. That's important to keep in mind. And we've got structural dynamic models validated against the test data and we're able to look at other conveyance system and in going work, we'll evaluate the effect of the rubber pads on the test and we expect all of the ongoing work is just going to confirm everything I talked about here. That the strains are low, we don't need to worry about the fuel breaking apart during transit. I just want to return to this slide. We have a very good understanding of the shock and vibration loads and the other work going on is just helping to complete the conclusion that shipping the fuel is not going to result in significant damage. Thank you!

>> BAHR: Thank you! So, we have about ten minutes for questions. Paul Turinsky

>> TURINSKY: Let me ask two questions. Why were the three locations chosen and what explains the difference in the responses for the three different vendor fuels? Is it predominantly location or is it the design of the fuel?

>> KLYMYSHYN: The location was chosen based on the analysis performed ahead of the testing. They were performing some basic shock type of analysis that moved the basket around to see where in the basket the response was the highest. So those three locations were chosen based on that study. I didn't go into detail on that but we have found that in the data, all three of those locations are very similar in response.

The amplitudes are a little different but when we look at the cross-correlation, they cross-correlate very well. We're concluding that it really doesn't matter where in the basket you put it. You're going to see approximately the same loading. The other question the three --

>> TURINSKY: The three vendors had different responses?

>> KLYMYSHYN: Yes, and that's mostly amplitude rather than waveforms or whatever. The Sandia assembly is known to have relaxed grid spacers and the other vendors, I believe had un-relaxed spacers so they had a tighter grip on the fuel rods. So that's the explanation that we have for that.

>> TURINSKY: And then what we talked about earlier this morning, the rod drop test. So, this is only normal transportation

>> KLYMYSHYN: Yes. So right now, we're planning on doing a 30 cm drop test using a -- actually, Elaina, do you want to speak to that?

>> ELAINA: In mid-December, we are planning to conduct a 30-cm drop test. It will be conducted with a one-third scale cask. It's the same ENSA cask. We use the transportation test but scaled to one third of it. And ENSA again is offering this cask to us as part of the collaboration. And it will be conducted in Berlin in an organization called BAM. They are a material science test facility in Germany. So, they offer also as a part of the collaboration because both parties are interested. They are doing it for free. No charges to us. So, we get a one-third scale cask. Everything will be done in Berlin and Sandia will

install -- I'm trying to remember how many -- 39 accelerometers on this. It will be installed in 11 assemblies inside of this one-third scale cask. And also on the surface of the cask on the same location as the transportation test. So they will drop it 30 cm horizontally then turn it 45 degrees and drop it again horizontally. So that's the plan hopefully.

>> TURINSKY: How about the bigger drop that they do in the safety analysis?

>> ELAINA: It would be under the accident analysis so we didn't approach yet accidental analysis part. So, the 30 cm test will conclude everything related to normal conditional transport. That's why we really wanted to do it. That's why other countries are also interested so we found great interest in doing it because this cask was dropped at Sandia previously as part of impact limiter testing but never anything was installed inside or on the assembly. So, everyone is interested to see what kind of acceleration we're going to get.

>> FOUFOULA-GEORGIOU: Thank you. I found your analysis very clever. So, if you go to the spectral density plot. We can see clearly for a large range of frequency, it is basically white noise modulated by the characteristics of the rods and then you can see the insignificant energy after some frequency. You can see later, where you have your fatigue frequency histogram, you

said that the high frequencies, they have very low magnitudes, basically.

So, what I was wondering, if I make the analogy between the football players, the single concussion versus the repeated blows, could it be that you had conditions that the magnitude of that set of frequencies is higher and you cannot ignore them? Like 37 gigahertz and above. If you had to truck parked somewhere because of, like a hurricane or I don't know what, and 150 miles per hour wind, would that create conditions that are not associated with the road but would have enough repetitive high frequency that they are worth evaluating?

>> What frequency would the wind be?

>> FOUFOULA-GEORGIOU: I don't know how to truck will translate the winds with the frequency, within, you know?

>> KLYMYSHYN: I guess we haven't looked at that. is that the -- over here, below one hertz, the truck has a rolling natural frequency of, I think it was like 0.6 hertz. So, if the wind was blowing it, the system would probably start responding in that frequency, that relatively low frequency and that is not enough to excite the rods in the higher frequency range.

So, I think you get a --

>> FOUFOULA-GEORGIOU: Translation kind of?

>> KLYMYSHYN: Yeah.

>> ELAINA: I just want to point your attention to our impact test, impact at 8 miles per hour, which is great force and would be comparable to the situation you described so this is kind of covered in terms of high energy transmittal like, eight miles so in my report, you will see the energy which was applied to the system when it was coupled at 8 miles per hour. So it was huge.

>> BAHR: Mary Lou?

>> ZOBACK: I have a question. I think it's inspired by the comments of the gentleman from the Sierra Club. That has to do with the grid spacers, is that what holds the rods at a distance of every eighteen inches about?

>> KLYMYSHYN: Some are 12, some are 20, it depends on the design.

>> ZOBACK: Are there scenarios, conditions, at which the grid assembly units could degrade and fail so you would have much longer stretches of rod sensing the vibration and you also would have pounding between rods.

>> KLYMYSHYN: I'm not aware of any.

>> ZOBACK: It's metal like any other metal and it's been irradiated so I don't know why it couldn't.

>> TURINSKY: They have spacers have springs on them and its friction fitted so because the rods are going to grow with heating and due to the irradiation growth on it.

>> ZOBACK: But they would never fail?

>> TURINSKY: Way back when in some of the early designs, the springs relaxed so much that the rods fell to the bottom of the assemblies in the reactor – went to the bottom of the support plate. So, the springs will relax and certainly the irradiated springs aren't…

>> ZOBACK: But the spacers would remain?

>> TURINSKY: They are welded to the guide tubes, not the fuel rods. Or they're highly pressure fitted, it depends on the design.

>> ZOBACK: So, there's not a scenario in which long lengths of a bunch of --

>> TURINSKY: No, the only thing that will change is the pressure of those dimples and rings on the rod.

>> ILLANGASEKARE: This may be a detailed question. You are a model, the railroad model, and you have your own model, you are coupling this model? So, when you couple these models, are you testing the coupling of the models because one model generates something and that will go into your model, is that correct?

>> KLYMYSHYN: Yes, the motion is imposed at the nodes of the single rod model. So, the single rod model is really very simple.

There we go. All right. So here is a single rod model. It's a simple long string of beam elements and the loads are applied as nodal acceleration or velocities at the grid locations where the arrows are. This is a reduced complexity model. I have another one that has every fuel rod, all of the spacer grids and all of the springs. But that one is just overkill. It takes like 17 days to run where this one takes five minutes. So, based on the way we validated it, we can achieve good validation, we’re using the simple model and the load transmission is by direct nodal excitation.

There's other ways to expand this model and represent the grids around it and have the grid springs and make it more, more correct, more physically correct. More accurate or precise. But it just isn't necessary in this. Does that answer your question?

>> ILLANGASEKARE: So basically, you generate this model and that will generate those source terms and it is fed into the other model.

>> KLYMYSHYN: Yeah, yeah.

>> BAHR: We have time for one more question.

>> BRANTLEY: I hope no one else asked this question. I had to step out for a second but I'm just curious why the team decided to do this test across the Atlantic? Wouldn't it be cheaper to take a boat out in the open ocean and go back and forth a little bit?

>> KLYMYSHYN: Sylvia, you can correct me if I'm wrong -- go ahead.

>> SALTZSTEIN: This started out as a smaller scale test in the United States but there was a lot of interest from first the Spaniards who are just going to transport by truck. They said, hey, if you guys are interested in doing this, we will give you our first off our assembly line transportation cask and system and let's do heavy haul trucks through Spain. The Koreans expressed interest and they're only transporting by barge. This is really cool; can we add some barge in there. So just kind of grew out of interest of other countries and everybody sort of paid their fair share and we were able to make it into what it ended up.

But yes, now that we know what we do now from this analysis, if we had just gone to Pueblo, Colorado, that data replicated and bound everything we saw in the cross-country rail test and on the ship and the truck.

So, we could have just gone to Pueblo, Colorado, in hindsight. But then you would have asked us, how do you know this is real? How do you know this is representative?

>>BRANTLEY: Are we going to be shipping things across the Atlantic?

>> SALTZSTEIN: Absolutely not! We will not be shipping anything through the Atlantic and driving through Spain. But that's data that the Koreans were most interested in, was the shipping data. So, we thought, well, we would prefer to have rail data in the

United States. We wanted it to be at some --- get control data TTCI.

>> BAHR: Okay. It's time to move on to the next speaker who is Ned Larson who is going to give us an overview of work completed and the path forward on the Department of Energy Research and Development on storage and transportation activities.

>> NED: When we met with a few of the Board members in Las Vegas, one of the suggestions is, is there way to pull it together and we can see how it interacts and how it goes? We felt like it was a good suggestion so I'm going to try to do that today. There we go.

So, what I'm going to do today is I'm going to talk about the R & D goals, why we did what we did and then explain how the results are coming out and how it affects our program. So, when we started our program, we have a number of people that are participating with us. We have industry through EPRI. They have been a great partner as well as the cask manufacturers, the rail and trucking companies. We have National Labs working for us. We have 11 of them working for us. We have given 5.2 million dollars to small businesses to make sure they can participate and get their best ideas. Universities, we have given 39 awards totaling 49 million dollars at this point. We have awarded to 30 Universities. So, some universities have gotten multiple awards if you will. And then Nuclear Regulatory Commission, we joined with them on some research that has an interest to us both.

And then ESCP is a big issue for us. ESCP is the process with which we work with the international community. When ESCP started, you can see we have 13 people in the international people with this and six subcommittees. The last time we met, we had 575 members in 19 communities.

A lot of the international communities want to work with us and know and understand what we're doing but it takes too much time to deal with them individually so we find ESCP a very good forum to describe what we're doing, to share the data with us. As Rose talked about every year, we deal with this. We meet twice a year and we present the results to the international community and we also listen to what they have to say and the issues they're working on.

The things we're working on, we have developed it through the gap analysis. The things we believe we need to do research on. We work with the Nuclear Energy Institute, NEI and their top priorities. We take input from them as well as the Electric Power Research Institute. We take that and try to put it together from the cast demo and all of the other things to get the behavior, the thermal behavior, the mechanical behavior, to get to the graph on the very corner, the tiny graph that compares the stresses and strains we exert and the stresses and strains the material can withstand.

On the high burnup demo, we started off this demo. We were worried that hydride reorientation was an issue. It's not a new issue. It's an issue we had for quite a while. You heard Mike Billone talk about it. He started working on hydride orientation when he was a young man. He was still a little bit of a young man but not as young as he used to be. He had been working on it for a while and we know about it and we were concerned it would become an issue, as we load our cask, as they sit in storage, and as we transport.

So, we started off with the cask. We're going to take the normal cask, load it and understand all of the things that industry would do and then see where it was.

When we started the program, we realized it wasn't going to hit the temperature that we thought we needed to hit, for a hydride reorientation. We even talked about canceling the program at one point because we had a big concern about it. We had a meeting among ourselves and we decided we were going to continue to do the program because we never looked inside of a cask after it was loaded so we went ahead with the project.

It has turned out to be more valuable than we ever thought it was. All of the sudden we are learning the thermal behavioral which affects a lot of the properties in our cask, how it's happens. We are finding the cask is not nearly as hot inside as we once thought it was. We understand that the margin in which we are predicting the temperatures versus what we're actually seeing is quite a bit. We talked about that this morning. Brady talked about that and so we are now benchmarking our computer codes to make sure they can predict this. The far picture that you see there, you're going to see that in the tour tomorrow. That's the test facility in which we started measuring BWRs to see where we were and to take advantage of that. No one understood where it was. What we are learning is the temperatures are much lower inside of the casks than we ever believed they were going to be.

Once we pulled the assemblies out of the sister rods for the cask, we came up with the test program. I don't expect to go through this. This is just an eye chart the way it looks here but we have shared this with the Board staff and others. This is a test program we hope to do and accomplish.

We have about 15 rods in Oak Ridge. We just sent 10 rods to PNNL to do some testing and about half of the rods to Argonne for them to do testing to try to pull it together. We felt like it was best to have the different labs participate in case there was a problem with one so we could understand and see where we are.

So, we're working on this to make sure that we can then validate and understand on irradiated materials, the behavior of the cladding and once it was stored in a cask and once it came out of the reactor. We're working on that.

This is what Mike Malone talked about in the ductile-brittle transition. We have looked at this. The biggest thing on this one that we are learning is that the temperatures are much lower inside of the cask and the pressures are much lower, the rod internal pressures are much lower than we originally envisioned when we started our research program. It has been very enlightening for us.

The temperature in which it loses significant ductility is much lower than we anticipated when we began the work. It is less ductile -- it is not brittle, just less ductile. That's an important distinction. So, what we're finding is that these materials not only are they important in storage but it's also important in transportation. Will it be sufficient ductile that they can accept the normal vibrations in the transportation process?

So, where we are today, the data we have today and the data we're showing now is that we believe that it is currently available, the data is available is believed that the cladding will remain intact and, its integrity, will not be challenged through extended storage or transportation.

We just believe that cladding is going to be sufficiently strong, sufficiently rigid, that everything we do in storage would be okay. We're just not seeing any problems so far. We then move forward to continue to do other things. The stress corrosion cracking as we call it, SCC. We need three things for it to occur on the canisters. We need an aggressive environment where chlorides are present. We need susceptible material, which we do have. Some of the stainless steels are susceptible to cracking as well as a tensile stress. We put together a test plan.

You see we had fabricated a large canister. Using the same procedures and processes that you would expect for a regular canister to be used. We took that. We took it apart. We analyzed it and did some very sophisticated things where we tried to measure the stresses of the tensile stresses in the canister after fabrication. We found we do have some tensile stresses. They do exist.

We are also looking at the aggressive environment. We know some of the utilities do have chlorides available, especially those close to the ocean with the prevailing winds off the ocean. So, we continue to do work there. The big issue that we're struggling with there is to understand the pit initiation as we would call it and then the crack initiation as it goes further and then the penetration -- how far into the canister will it go. There's a lot of opinions on this right now but we don't have definitive data to say and make profound conclusions yet. You can see on the bottom, this is an area where we have engaged a lot of Universities because there's a lot of expertise in a lot of these Universities to help us address this problem as we move forward.

Where we are right now for these conditions, we know we have some signs with the materials. We know we have a material susceptible to chlorides and we know that stress exists. So, we're continuing to do research on this. Currently some stress corrosion cracking exists but we don't know if it will go through the canister wall. Some say it will, some say it won't.

We have about five National Labs working on this issue and we have maybe a dozen opinions on how it's going to behave. These are issues we have to know and understand and do more research and we continue to work on it as we move forward.

The other issue is transportation, understanding how these things affect transportation, how it will affect the cladding. Just the normal shock and vibration as we ship. Canister integrity as well as the cask components. One of the things and comments I want to make as we move forward and Nick talked about the drop test we are looking at doing in Germany at the, in this case. We are not testing the structural rigidity or capability the casks itself. The Nuclear Waste Policy Act requires that we buy casks that have a Nuclear Regulation Certificate of Compliance and the NRC is in charge of regulating the cask manufacturers. The DOE has no intention of double regulating the cask manufacturers. We will buy casks that have that CoC. But the one thing that we are concerned about is how the fuel will behave in the cask as it is dropped.

We will talk about it a little bit later. When we talk about this, this was a fun project as you can imagine. What happened is the Spanish came to us. We wanted to do something like this any way. The Spanish came to us and proposed it. We were a little bit hesitant at first because it was a little bit early in our program. So, we had to pull some money.

The Spanish agreed to pay for a big portion of it, about two-thirds of the cost. So, with them paying two-thirds of the cost, we felt it was foolish to not take advantage of it. They had specific data they were looking at and wanted to gather so we agreed to move it up in our test program to do this. It turned out to be a much better program than I had hoped for. I'll just say it.

We monitored and measured all of the data as we crossed the country. We had about nine terabytes of data. As we crossed, and as Nick pointed out, I had Nick plot what we measured when we measured in the cycles and the strain, how did we measure and how did it compare to the failure curves.

This shows that we're about two orders of magnitude lower in strain and micro strains than what would be required for failure so we concluded we were in good shape in this case right now.

We didn't see anything that even came close to failure. The cyclic strain, this is the cyclic testing that Oak Ridge is doing. They doing some innovative activities there because we want to know how the rods behave that have been irradiated and go through the cyclic strain and cyclic test. Oak Ridge is doing this. They're doing it in the hot cell. We have two things. Sylvia presented this one.

If you look at the one with 1300 PSI. In the 1970s, I believe it was, there's a NUREG on this, I can't remember the number but they did a shipment from Nevada to Albuquerque and they measured the top of the bed of the truck. They didn't measure the fuel itself because no one could get on there to put strain gauges on the fuel itself. The maximum stress is about 1300PSI and we put it on a shaker table and that's what we did to understand how it would behave.

When we took the data from the multimodal transportation test, the maximum we saw during the test itself was about 850 PSI. That is adjusted for no rubber pads that we did from the shaker table test.

If we did it based on the highest we saw, that would be about a thousand PSI that we would see and we saw it during the coupling test, the TTCI. So, with this, we looked at that and again, that's at least an order of magnitude lower than what we expected, what we believed would be happening if we were to do more tests.

If we look at that, even in the end when you go over a million cycles, towards ten million cycles, those were taking so long we just had to stop the tests. There's no failure on those at the very end, the green ones. It was just, we were taking thirty days to run the tests. This with ten million cycles, we said it wasn't going to fail so we stopped. Even that many cycles at that strain, we didn't see any problem there.

So, we believe even with the cyclic test, we believe that the transportation, we believe that the shocks and vibration that we encountered during the entire length of the test would exceed that no shock and vibration would exceed the strength of the cladding either in the static or dynamic condition. This is under normal conditions of transport.

I would note this is also without what we call the S2043, the American Association of Railroads, AAR has a standard for 2043 for railcar for shipping of nuclear fuel. It is a very high-performance car. It has performance requirements very similar to a passenger car only it carries 200 tons.

So, what we found is even using a standard freight car, we saw nothing that gave us pause or concerns when we transported these materials on a standard freight car across the country back and forth.

So, we believe in transportation we're in good shape there.

As far as future work, we need to continue to do more work on the static test and more at Oak Ridge and PNNL. We continue to do more numerical modeling to make sure we understand very well how our materials are behaving to make sure we can duplicate those with our numerical models. The scaled drop test at BAM-- I apologize, I can't what BAM is, but it's a German organization. I don't know what it is but it's a facility. This is another one where the Spanish came to us. They had the cask and Elaina talked about it. They said, we would like to drop it and the Germans at their very nice facility said they would drop it and cover the costs. The cost we have involved in this. We understand the scale factors very well but at the same time, the amount it's costing us is very low in comparison because we can engage both the Germans and the Spanish on this to share the cost. So, we figured for that much money, it's smart to continue with this. But again it’s a 30-cm drop, about a foot. We're looking at the performance of the fuel inside of the cask, not the cask itself.

So, we'll continue to do that work. Drying and understanding more shipping conditions. We talked about it this morning. We need more data and we need to understand that a little bit better. We're in the sure it makes a big impact on our cask but we just need to understand it. We're going to do more research on stress erosion cracking. We will continue to work with EPRI on understanding how to do in-situ repair if we need to. They will continue to do that with us as we continue to work with them. So, cask failure, canister failure mechanisms and hypothetical loss if we do have a crack. We're doing consequence analysis to see if we need to do it or not.

We're still evaluating that and then we need to know more information about the conditions of salt and chlorides at the different utilities if we don't have any chlorides there, then we're in pretty good shape. So as far as transportation, we continue to do transportation-related work and things that we need to do to understand the stress limits as well as those on the rail car. We're looking at an 8-axle rail car, we’re looking at the design, the current one on the Atlas which is a twelve-axel car. It's very heavy and large and eight would be good so we are looking to evaluate that. We're working with other countries to get more about transportation and more transportation data. We continue to work with the Universities, mostly. We have allocated this piece of work, developing sensors and remote sensing to mostly universities, as opposed to National Labs. We believe the Universities have the skills and abilities and we want to get sensors inside of the casks to monitor and understand what is going on inside remotely and there's a lot of technical challenge in that one from the hostile environment within the cask and to getting through the wall of the cask and outside. And dealing with the battery. So those are the things we're currently looking at and things that are important to us. Like I said, well, I guess, any questions at this point? I have a great team to answer them.

>> BAHR: Thanks, Ned. we have about ten questions. I think there's a staff member who wants to ask questions. I'm going to defer to Bret first.

>> LESLIE: Board staff, it has to do with putting it all together. So, you and Brady and everyone else has been talking about storage and transportation. What are the real implications on the disposal side of having actually lower temperatures than expected? So, you know, for disposal you have limits that assume certain things. Well, the real temperature is going to be 20 percent lower. How has that kind of finding in the storage and transportation been brought into the thinking of the type of experiments that need to be for disposal. Now, I'm doing it between the two but I just wanted to know whether DOE has had that internal conversation yet.

>> NED: Having the temperature so low, we believe, I believe we can make the statement that pretty much all of the casks that have been loaded today, can be transported and stored indefinitely because of the temperatures. We don't believe hydride reorientation is occurring or degradation of cladding is

happening. At those temperatures, we believe they are behaving well.

So, under normal conditions of transport and storage, we believe for normal conditions of transport, there's no problem but we're going to look at issues dealing with emergency conditions under transport if you're with me. We're looking at doing some other work. The utilities have expressed an interest in loading it higher against the margin, the 400-degree C which is the NRC limit. We have some concerns about loading it that hot. I'll be candid. We are not enthusiastic about loading at that temperature but we continue to do a little bit more work we believe to make sure that we understand that, that we can answer that question.

Utilities believes it gives them some flexibility in their pool that they can load it hotter but at the same time, we believe it can create other problem, where we may see hydride reorientation. So, when Rose talked about possibly heating some of the rods to 400 degrees and testing them, we want to make sure we understand that. That's what -- so we're doing things like that so we can try to bound the issue entirely especially with our lab testing.

>> BOYLE: DOE. Another part of Bret’s question is, how does it relate to disposal? So, the first thing, and I would have to turn to Peter or maybe John Scaglione might know. Just because the utilities calculated their heat output one way doesn't mean

the repository people, pre or post closure, did. I don't know. For the sake of discussion, let's assume the repository people did as well, and it's actually cooler, and it's also cooler because we're taking longer to dispose of them.

But I'll put my tongue in my cheek a little bit. I can't believe I'm getting this question from the Board. Ernie Hardin is nodding his head. You guys ought to be aware there was many years with many meetings where the Board's point of view was, you ought to have a cooler for a repository. So yes, Karen is bobbing her head yes too.

So, the Board, all of those years ago is correct. The cooler packages shouldn't be a problem.

>> LESLIE: I would like to follow that up, do you need an HotBent experiment that is going to 200 degrees C. I was being practical, not flippant.

>> BOYLE: That's okay. But just because some of the spent fuel could be cooler because of analytical methods or just age, whenever we get to a repository, perhaps we can crank the temperature back up by bringing the drifts closer together. We really couldn't put the packages any closer together. So, there could conceivably be interest in 200-degree behavior of that any way.

>>NED: I get so focused on storage and transportation that I forgot about disposal.

>> PEDDICORD: Thank you very much! This is very interesting and very nice in pulling all of this together as well too! A footnote, if I recall correctly that normally four or five years ago, whatever was during the dark period when you were questioning all of this, I believe it's correct that the Board was one of the voices crying in the wilderness, urging you to go forward with this test because of the potential of getting data and information. And, I think, you really demonstrated tremendously the value in that, in the things that have come and the new perspectives and things we didn't know five years ago. So, nobody pats us on the back but afterwards, we'll get together and we'll pat ourselves on the back as part of it.

But the other part of this, while Mike Billone remains young and vibrant and all of the adjectives that you want to put to that, but you pointed to the multimodal test and BAM that you'll do with Germany and so on. Is there at least some thought process to be reaching out and engaging in these bigger endeavors that there could well be more surprises out there, good or bad, that might shed light on some of these things. I'm also thinking in the period, kind of after the demonstration test, you know, that is going to be open and so on. Looking at a broader or further horizon.

>>LARSON: That's a good point! The international community is moving along also and there's a lot of good ideas there. What we

find is the ESCP forum is a good forum to at least to hear things that are going on and then we pursue it when we hear it. For instance, we understood, that we found out that the Koreans were doing a similar test to what we were doing here. We didn't have the opportunity to join in it at this point but those were the type of things that we're learning through that process.

We have continue to feel like we have to continue to work with the international community. We see them twice a year. We work with them twice a year in the ESCP program. So, we feel we keep in touch. I would just say that the program doesn't change that fast. The research is tedious, slow and time consuming when we deal with hot cells and it's very expensive so we have got to keep after the international community to understand what they're doing.

>> PEDDICORD: One of the things is you kind of have a program of exchange of scientists, international exchange. You have some folks in -- I forget. I'm old. And I think you had some of your folks embedded in programs for a period of time overseas as well too. Is this something like that still functioning or something like that, that you can still implement to kind of have the very good dialogue in exchanging ideas?

>> LARSON: I don't know, we still deal with them. When we do some of the research programs with the Universities, we put together what we often call steering committees and we often have

international people participating in the steering committees with us so they understand and we're getting feedback. That is the one thing we continue to do but in our other research, we don't have a program like that. Maybe it's time to look at it again, I don't know. I don't know, Sylvia, do you?

>> SALTZSTEIN: We have some visiting scholars from Korea but we haven't been doing it as much as we used to. And a lot of it is because it's become much more difficult to do since 9/11 and other resulting changes at the national labs, it's been a lot more difficult to do those exchanges.

>> LARSON: We continue to work with them but like I say, it's, we're -- as a general statement, we're ahead mostly of the international community. There's some that are ahead of us in certain areas, for instance. The Germans are ahead to the molded cask and the metal seals and cask. They're ahead of us but some of the other stuff, we're a little bit ahead of them.

>> BAHR: I think this is the last question because then we eat into the break.

>> TURINSKY: This was asked earlier of someone else but I'll ask you because you're in charge of the program. What about the claddings that are coming out before we get to silicon carbide. At least we don't have to worry about hydrogen for that one. What, where do you see this program going? Westinghouse has new cladding materials. They are putting on all of these coatings

for the accident tolerant fuel, so how is this program going to address that or do you view that as the responsibility of the industry at this point?

>> LARSON: You're exactly right. There is new cladding coming out. Even all of the cladding that we have, we don't have every type of cladding that was in that transportation test. We don't have every type of cladding in the demo. What we believe is if we can get good at benchmarking our computer codes, that we can model it effectively and do a good job there. We're looking at some of our numerical models to help us move forward and help predict the behavior of some of those because we just don’t have the budget to physically test everything that exists today.

>> TURINSKY: You have heard this from me before. Really, the fuel performance modelers are not engaged in this program at this point.

>> LARSON: I understand.

>> BAHR: Thank you! (Applause)

So, we're scheduled to have a break until 2:55.

(A break was taken.)

(Standing by.)

(Silence.)

>> BAHR: So now we're going to move on to our final presentations of the afternoon, both of which are focused on this idea of what's involved in direct disposal of canisters of spent nuclear fuel and Tim Gunter from DOE is going to kick us off and then we'll have a presentation from Laura Price of Sandia National Labs and John Scaglione from Oak Ridge National Labs after that.

>> GUNTER: Good afternoon! I'm with the DOE office of spent fuel and waste science and technology. I'm the program manager for disposal-related R&D which includes direct disposal of DPCs, dual purpose canisters, which is what we are going to talk for the rest of the afternoon.

This presentation is a two-part presentation I am going to be followed by Dr. Ernest Hardin, from Sandia National Labs. They are going to give a general overview of our program, what we have ongoing, what we are looking to accomplish, and then Dr. Hardin and the other presenters will go into more details on the work activities. I am going to start out with some disclaimers which I am not going to spend much time on, but I did want to call your attention to the first one the underlined text here: <u>To the extent discussions or recommendations in this presentation conflict with the provisions of the Standard Contract, the Standard Contract provisions prevail.</u>

So basically, what that means is so we have standard contracts with the utilities for the disposal of spent fuel. Some of the work we have ongoing some of the recommendations that we might make may not necessarily be in line with the standard contract. We recognize there are contractual issues and policy issues that would have to change if these recommendations were to be implemented. But that is not the purpose of our work. We focus on the technical issues right now. Outline of the presentation talk a little bit about the background examples of DPCs in current use. Projected accumulations of the DPC inventories. What are the benefits from direct disposal? Or at least the potential benefit. Some history of the DOE R&D program for DPC direct disposal. And then Dr. Hardin will pick up and talk about some of our previous studies. Including screening of critically dose assessments on both low probably and low consequence. Also talk about independent expert review. What kind of results we got out of that. What we are going to do about it. Talk about some injectable fillers. And then summarize the ongoing work and planned R & D in the future. This is an important program for the utilities. As you know spent fuel inventory and the capacity is not enough for all of this spent fuel that is coming out of reactors, so they went several years ago to dry storage and dual-purpose canisters. So, DPCs are loaded in the pools and dewatered, welded shut and then transferred into a shielded storage cask. So, what is the DPC? Dual purpose canister. It's a canister that was designed and licensed for two purposes. One is for storage of spent fuel and then for transportation of spent fuel. And they actually make up a majority of the dry storage inventory, greater than 90 percent of our current inventory. So why are we interested in trying to directly dispose them if they were not designed initially for that purpose? And that's kind of the distinguishing feature of them. They were not designed, not loaded or licensed in consideration of the ultimate disposal in a geologic repository.

We'll talk on the next slide about some of the potential benefits for, if we're able to demonstrate that we can directly dispose of the DPCs. These are some of the features that we have to consider for direct disposal. Primarily the first one applies to all. We have to consider the safety of the workers and the safety of the public. Mostly in the safety of the workers in the operations and of course the public and the doses that can be received, through the performance assessment. Is there an impact of performance assessment based on this, based on direct disposal of DPCs?

So, I'm not going to spend much time on this because you have seen storage canisters, a warning in some form or another. This is a typical example. There's multiple vendors that supply DPCs and multiple configurations based on the fuel type and the amount of fuel you're trying to store. This is a typical, a NUHOMS design basically just a rolled cylinder, a right cylinder. Stainless steel. Some of the other components vary in materials but you have a bottom plate and a series of top plates. And then one design in this case and they're placed in horizontal concrete vaults and they do that using a transport, shielded transport cask that this slides into, then slides into the vault.

Another quick design. This is a MAGNASTOR, it is a vertical design. This is what you have seen sitting out on the pads at ISFSIs. It was put in the concrete over pack.

So, this is a graph that shows the increase in inventory of DPCs. The blue line is the amount of spent fuel in the spent pools. This is in MTUs, metric tons of uranium. And you can see where we're at about right now. And then the green line is the amount of fuel in dry storage and as we go out in time, more and more fuel is unloaded from the reactors and then moved into dry storage. You see the spent fuel inventory is continuously dropping and the dry storage inventory is continuously increasing. This is based on an assumed life extensions of 60 years for the existing fleet.

And then the red line is just the total. So, you can see that we will have, absent some change in getting a permanent disposal method, we'll have quite a few DPCs out in the future.

What are some of the potential benefits of directly disposing of the DPCs? In no particular order, you probably have less collective worker dose and these are kind of inter-related. Reduced handling of the fuel management operations. You don't have to cut open existing DPCs and repackage them into another canister and cask arrangement. You eliminate a lot of handling activities which in turn you could potentially minimize the dose to the workers in those operations.

You also have a reduced low-level waste inventory because you're not using those DPC holes. They would be disposed of, if they were directly disposed of. If they're not, you have to dispose of them somehow, typically low-level waste.

Also, going along with the reduced fuel handling is the reduced risk for fuel damage because of less handling and potentially significant financial savings. I mean, this is all adding up, savings in dollars and reduced handling and reduced equipment that you no longer need.

Okay, this shows some of our campaign activities we started back in 2013. Really, a modest level of funding initially and actually for the first few years and in '17 we have started increasing our funding and in '18, and then going into the next, we got pretty substantial funding levels. So, we have directed some of that to continuing with DPC analysis.

Our initial approach was technical feasibility and low probability screening. Some of the key, you know, key issues with direct disposal is criticality controls. So, there's a number of ways we can look at to show it would be acceptable. One is additional work in the screening on probability. We can do enhanced analysis for our criticality analysis. There's potential use for fillers that would be injected in the DPCs for criticality control. Basically, moderator exclusion and it could be also some kind of neutron poison that is part of the injection.

Another option is consequence analysis of criticality. We're just looking at it as an option. What if you're not able to screen it out on low probability, then there would be some consequence analysis that we could carry forward. As loaded, I'm basically getting as loaded data for the spent fuel, which would give us higher fidelity modeling on our criticality analysis.

So that's really, if you look here. This is our program going through time and we'll be talking about some of these in the following presentations such as engineering, handling these DPCs and in general can be a bit heavier and larger than other spent fuel canisters so we have to evaluate the physical size of them. Can you handle them in can you get them down in a repository? Thermal management, you know, what does that do to your thermal management strategy in terms of aging, back filling if you need backfilling in whatever geology you are looking at, and also, we look at different types of geology, some of which are more forgiving to higher heat loads.

So, then there's the calculations, if you're in a salt repository and you have water flooding into a DPC or any spent fuel package, well, that water is going to be brine so it will help you in your criticality calculations because of the neutron absorber properties. And then as I'm going along with what moderator exclusion, overpack reliability, how substantial and how much longevity those have that helps keep out the moderator.

And then we'll talk about these more. So, I'm going to stop there. And turn it over to Dr. Hardin.

>> BAHR: I apologize for not mentioning Dr. Hardin in my introduction.

>> HARDIN: Hi, good afternoon! I'm going to give you an overview as claimed here of the past work we have done starting around 2012, 2013 and really finishing up in 2015 with supplementary work done by Oak Ridge in 2017. I am going to present work that is really done by a team of people. The leads from the rest of the team are here, Laura and John Scaglione of Oak Ridge. But behind us, there's probably ten or twelve other people who I don't have time to name.

I'll give you the conclusions first. How is that? We were tasked to do a technical feasibility evaluation for the prospect of direct disposal, commercial spent fuel in DPCs so we said the objectives are safety, post-closure criticality control, thermal management, and engineering feasibility. The conclusion is disposal is possible with all of the geological settings we looked at. That would include hard rock, be it granite or tuff, but also salt and clay or shale but that statement needs qualifications.

The feasibility evaluation we did is done in the context of low probability screening. We were looking at whether there are indications of criticality would or would not occur. And further, that disposal is possible but we had concluded that not all DPCs would be disposable in all geologic media. That was largely due to the post-closure criticality question.

We made some other findings at a high level. This question of disposal over-pack reliability gets to what we call early failure waste packages. We're talking about manufacturer’s defects. If a waste package failed in an abject way, then we have a whole litany of different processes that kick in and criticality is one of those. So, we're looking for a package with a very low defect rate. We reviewed what Yucca Mountain did for their early failure abstractions and some experts at Sandia concluded it could be done better and we could update that.

We also recognized from some of the work that Oak Ridge did, that the degradation of the basket was going to be important for whether criticality will occur. That you needed a configuration in order to see the criticality process kick in. That all baskets are not created equal. There's a couple of dozen fundamentally different designs out there in the DPC world and some of them would degrade faster than others. So it amplifies the possibilities for the analysis of the degradation of canisters.

We made recommendations. We said, please continue the work on fillers which we're doing and investigate the screening on low consequence and we're going to talk about it more this afternoon. So just how would we dispose of large heavy, heat-generating waste packages? First off, I have to say and I'm fond of saying that the typical DPC waste package would not really weigh that much more than the 21 PWR waste packages proposed for the Yucca Mountain LA. So, the Yucca Mountain TAD loaded would weigh right around 48 metric tons. The MAGNASTOR that Tim showed you a slide on, weighs about 50 metric tons loaded. What's the difference in diameter? Right around 20 centimeters. That's a percentage. It should be about ten percent, maybe a little less.

So, when you add the disposal over pack to one of these things, you're adding about twenty or thirty more metric tons So, you're talking about something over all that weighs in 70 or 80 metric range and that's not shielded. If you want to deal with it, if you want to move it or store it, you need to put a shielded overpack on it, and that will add north of 40 or 50 tons. There are sources where you can look up these numbers. So they're not that big or heavy.

But they are big enough that we would be forced to use in-drift emplacement, this is where you transport them under ground and basically deposit them on the floor of the drift. There's no point in trying to up end them or to put them in a borehole. You're not going to get performance out of that and it's going to be an engineering hassle. The packages could be transported underground by shaft or ramp transport. I have a couple of slides on it. You will need some sort of aging or ventilation in situ in the repository. As you recall we were going to do that at the Yucca Mountain. That would be to remove heat and control the heating of the near-field and ultimately for most of these concepts, you are going to need to backfill and there's reasons for this. If you have a saturated zone of repository, you need the back fill in there to control the conduct of water through the opening. If, however, it's unsaturated, then you have other options. You don't necessarily need to backfill. Backfill can control the ultimate collapse of the underground opening and the formation of a larger and larger DRZ as the collapse occurs – the Disturbed Rock Zone.

So, these are some concepts. The spacings here could be quite large. Typically, of course, it's more economically effective to have a smaller spacing in the drift and a larger spacing between drifts from the point of view of total excavated volume, which is a cost parameter.

But the spaces could be really quite large. Ultimately, if you have let's say a 20-meter spacing between waste packages in the repository which is not unreasonable for thermal management, you have over 200 kilometers of underground fill for emplacing waste. That makes a fairly large-scale civil works project and so you have to ask the question, what are the performance requirements on those? How long do they have to stand up? What sort of ground control do we need? What are the possibilities for maintenance and ultimately, how are question going to backfill them as a radiological environment so it would have to be done robotically. This is all things we touched on in our deliberations and our final report.

As far as engineering challenges, we feel they can be met. These are problems at their heart, feasible but they're not necessarily cheap to solve. When you're talking about a project that costs 40 or 50 billion dollars, spending 100 million dollars to design a first of a kind shaft hoist may not be that insurmountable.

So, for handling and packages of these large canisters, of course, we would use the standard practice today. I'm not going to go into it. There would be some operational and worker safety issues that would have to be dealt with. For transport of the packages underground, there's several options.

We got a little help from Charles Fairhurst, who is a well-known professor and mining consultant, who surveyed worldwide practices in transporting and handling large heavy things, especially underground. It comes down to several technology options. The will Swedes use a spiral ramp, a rubber tired conveyance. You can have a fairly steep ramp that way. There's some safety issues if your transporter gets away from you.

The French are working on a funicular concept, which is also a steep ramp, for their CIGEO repository. That would have a payload capacity somewhere in the 25- or 30-ton range. We could use a shallow ramp, the Yucca Mountain LA has a plan for such a thing, and actually the north ramp and the south ramp at the ESF are shallow ramps in which we would transport waste packages on rail using a really quite heavy shielded transporter but the rail has a capacity for equipment that weighs like 250, 300 tons. So that's feasible.

Finally, a heavy shaft, you know, if you're mining a repository in a geological setting like a layered bedded salt, that can be stratigraphically and hydrogeologically quite complex and you may not want the exposed area of your access way to the various units you have to mine through to get down to the repository.

So, for purposes of stability and water inflow and water control, a shaft would be, you know, superior and that's what has been selected at WIPP and Gorleben. In fact, if you look at the salt mines they generally don't use ramps, they use shafts. Then you have to ask the question, how you are going to get this shielded transporter containing 70 or 90 ton waste package down a shaft. Then I went to BG Tech in Germany, which is affiliated with their government agency for repositories and they produced a conceptual design they have been working on since the '90s for a large shaft hoist with a capacity on the order of 175 metric tons. I'll say more about that.

The question of drift opening stability over time is also important, especially if you are thinking about ventilating in situ for the removal of heat.

The hard-welded tuffs at Yucca Mountain are no problem but the shale formation would be a different animal. You would have to come up with the right system and it would be site specific as far as how you're going to keep the drifts functional for the required time with minimal maintenance.

Okay, so about the heavy shaft hoist. It's important to note that it's friction type hoist. It's a friction winder as opposed to a single drum winder. When you were at WIPP, you may have taken a ride down the waste transport shaft. It is a Koepke friction wider shaft. At the time it was built, it was the largest shaft hoist in the world to my understanding, the largest payload. Its payload is about 40 metric tons. The German group did a conceptual design and tested the safety systems for a hoist of a capacity of 85 metric tons which was suitable for the Pollux cask and a particular disposal concept they were working on for Gorleben in salt. They then elaborated on that and said, well, what if we wanted to take a CASTOR-V cask, a PWR cask, self-shielded, and dispose of that directly, much as I described here. They said, this is the payload capacity we need and they drew up a conceptual design for that.

The long story short is that it turns out for waste transport, you need a slow hoist. You want one that doesn't require much power. Doesn't look like a mining hoist. It's slow and it has safety systems. It’s counterweighted and you actually have to do work to get your heavy package underground.

The cost of that would be surprisingly low according to that estimate. So, moving on to thermal management. This is a figure we developed to show the relationship between aging time or the time out of reactor until the fuel in a waste package can be back-filled and drift or repository panel can be closed. So that time is on the X axis. Over here we have the total power for a hypothetical 32 PPWR waste package which could be a DPC. So, the black curves plot the actual decay heat output for different degrees of burn-up. This is totally hypothetical figure and you would never see one canister with 32 assemblies that all have the same burn up according to the numbers we use here.

In any event, what this allows us to do is to compare the different disposal concepts. For thermal management purposes, I have cooked it down to the easiest thermal management strategy that you can come up with. That is, you can declare a thermal power emplacement limit for each package. It's very simple, when the package gets to so many kilowatts, you can then place it. That limit is plotted by these horizontal lines and where they intersect the black curves, you can then trace downward and find the aging time that's required. For the green and the blue, we're talking about hard rock unbackfilled and also salt. These are media with really high temperature tolerance. Salt can easily take 200 degrees C and that same value was used for the design at Yucca Mountain. However, if you have a system that uses a low permeability clay-based backfill or buffer material and you care about whether the peak temperature of that material in the very near-field around a waste package, you then have a severe constraint on the power output of that package, otherwise you blow that limit.

This is consistent with what the Swedes and the Swiss have been doing over the years. It turns out, even if we had an exceptional back fill that could sustain 200 degrees C peak temperature and still function, we would require significant aging on the X axis in order to get that in place in a repository. That's the thermal management story. It's doable for any disposal concept but some are going to be a lot easier than others.

As a supplement to that slide, you can ask the question, how long does it take to cool waste packages so that you can put it in the repository. So, choosing a limit of ten kilowatts, which is one of several studies that we did, and using the TSL-CALVIN logistics simulator which Elaina ran for us, we're able to simulate how many waste packages in the current fleet of DPCs and the projected inventory of DPCs which would be loaded in the future, how long does it take each one of those to cool to ten kilowatts? So, the green plot is the number of those cooled ten kilowatts per year. You can see it tails off. By 2130, we could potentially get all of those DPCs in the repository at the ten-kilowatt limit.

As an aside, the facility throughput you would need to do that is quite manageable at 1700 metric tons per year. That's quite a bit less than the facilities planned for Yucca Mountain, for example. That really is a full story on thermal management. I want to talk about post closure criticality control. So you have a waste package – the neutron absorbers are aluminum based, the package breaches -- ground water or moisture comes in and it begins to attack the aluminum. We know that aluminum doesn't survive that long in a moisture environment, like on the order of 100 or 200 years. It depends on the grade of aluminum and the thickness but we can't take much credit for it.

So, the aluminum has embedded particles of boron or has some boron associated with the aluminum. As soon as the aluminum corrodes, we no longer have a configuration that we can count on that contains those neutron absorbers. So hence we need to think about how we control post-closure criticality, absent neutron absorbers. So, the strategies for doing that are fairly straightforward. Moderator exclusion, keep the water out. So, if you have a defect-free disposal overpack and you have a disposal concept and a geological setting, that does not bring disruptive events that can break those or breach those waste packages. You could probably construct an argument that criticality is less likely than the probability screening threshold for the TSPA.

That at the moment seems like a stretch. We were kind of on the cusp there. Let's look at other options. Another one would be moderator displacement. So here we're talking about the primary purpose of the fillers that you can somehow get into the package and discuss in a couple of minutes is to keep the water from moderating a nuclear reaction and allowing criticality. That's an option. Those fillers can also be loaded with particulate matter that absorbs neutrons . That’s quite a possibility and we're continuing to work on that. And you could also put, you could cut the DPCs open and put control rods there. Typically we imagine Zirc-clad rods containing mainly B4C that would act as poison. It would take a lot of rods to do this. It's been studied by EPRI. It's an option. It's not one we're going to pursue at the moment because it starts with cutting open the DPCs, okay?

So, with all of this said, we need a criticality analysis methodology. So Oak Ridge provides that function for DOE at the moment. They are using burn up credit up to 29 different nuclides have been included so far. They're looking at the as loaded characteristic of the fuel including the map of where the assemblies go in each canister and they're evaluating some nominal cases and some stylized degradation cases.

As you can well imagine, it's difficult to characterize exactly how the basket and fuel would degrade in the repository, in the disposal setting and in that environment. There's going to be some variability. There's going to be some heterogeneity in how it degrades so we go to the stylized cases which have been around for decades to serve that purpose.

So finally, the figure shows that the reactivity of the fuel in a repository is not fixed in time. Due to radioactive decay and growth, it would tend --- it's on the decline now. We'll see a minimum, if I can make it work, in roughly 50 to 100 years and then it will increase and there's a maximum amount, somewhere in the neighborhood of 10,000 years, earlier or later. It depends on the characteristics of the fuel.

This particular plot is k-effective versus log time. There are three different burn up credit scenarios here and this is for a hypothetical 32PWR package containing fuel with the typical characteristics. So basically, it's interesting that the difference, the vertical distance between those curves gives you an idea of what you gained from considering burn up credit. It's very important for the strategy.

Okay, so moving right along here for Yucca Mountain the probability of post-closure criticality was deemed low enough that it could be excluded from TSPA. The analysis used to get to that conclusion was an event tree. Here is a similar event tree that we have recast for the DPC problem. You can see the pivotal events. Like, sufficient water does or does not go in the package. Without water, it can't go critical. The water does or does not contain neutron absorbers. We're talking about natural chlorine. Three-quarters of chlorine natural abundance is chlorine-35, which absorbs thermal neutrons very well.

Corrosion products, if they built up inside of the basket within the fuel, would act as moderator displacement. We're talking about corrosion products of aluminum, stainless steel and other materials available in the canister.

Then we get to sufficient fixed neutron absorbers. Well, for Yucca Mountain the strategy here was to adopt. Well, before about 2001, they were actually seriously considering a criticality consequence strategy. They went to a low probability strategy because they realized there were neutron absorbs materials available that could be built into the waste package and would go the distance, so 10, 20 thousand years. They specified in the TAD transport aging and disposal canister. For the LA, there was a spec developed, not a design. It says by the way, you should use 11-millimeter plates of borated stainless steel. That of course was reviewed by NRC and in that fresh water condensate environment, that strategy will work. If you take the same material and put it in a different chemistry in a different geologic setting, we're not sure it would work. That's where we stand right now with that approach.

And then of course, there's other questions that you can ask whether the basket is collapsed, whether the fuel is collapsed. Ultimately something's got to stop the leak, quench the criticality reaction, but that is an active area of investigation for us.

So, in two slides I would like to summarize the feasibility study we did in 2013, 2015. With respect to safety the disposal of DPCs directly would respond to the same characteristics of the system that make disposal of the same fuel in purpose-built canisters safe. I said that backwards but what it means is, the characteristic like diffusion limited transport in the near field and far field, works great for DPC just like any other canistered fuel.

So, we did find however, the performance assessment models need to be tweaked in order to discern the differences. We're talking about some pretty subtle differences in the degradation and the ultimate release of radionuclides and the transport to the accessible environment. So you need good models, and we also found that we might have to develop the capability to put cementitious material into our long-term performance models for the repository and that is because you might need them to handle the larger heavy packages with larger opening spans and so forth. That's a safety issue.

As far as engineering feasibility, if we're going to go to aging and that figure I showed you that was generated with the logistics simulator has some DPCs as old as 80 years out of the reactor when they were emplaced in the repository. If you are going to do that, you need to consider fuel condition as part of your thermal management strategy. Notwithstanding what Ned told us just now. That really, we might be able to store this fuel indefinitely in DPCs.

Clearly, we're going to need engineering work on transporter and emplacement vehicle system concepts. There's going to be materials used and site-specific corrosion problems. You need corrosion data. That data would need to be collected timely and we talked about a couple of the other things already. With respect to thermal management, we think there is work, R & D work going on in the program right now to look at backfill materials that can take 150- or 250-degree C. That work should continue. It may be useful. There's a possibility that large heavy packages in a salt repository could sink due to plastic creep. And I say that because in the last decade or so, there's been some new data developed supporting a new mechanism of low strain rate, low stress creep in salt. It's a possibility. DOE has a collaboration with a couple of German agencies are on that topic. Finally, if we're going to put DPCs in a repository in clay or shale, we're going to heat it up. We're going to heat a lot of clay and shale so we will need the best thermally driven process behavior model in that material. And so as far as criticality control goes, we will continue to analyze as-loaded DPCs. So that information continues to come in and what was not mentioned is the GC859 exercise doesn't necessarily include design information on the canister of the basket but you also need it to simulate post-closure criticality. We talked about degradation scenarios and how we'll represent the behavior.

We need to look at how the fuel and basket ultimately degrade. That's what turns criticality off. That's part of our overall program. For BWR fuel there's more work we can do to implement burn up credit and we can answer your questions on that. And we continue the fillers work. So those are the conclusions of that study.

And then this year we commissioned Halim Alsaed to do an independent review of the DPC R&D disposal program that has been raised and I wanted to touch on the recommendations of that. The first five we have already talked about. As far as 10CFR72.236, this is part of the storage rule that addresses canister design. It says you need to consider disposal and it's been handled in a rather cursory way up to this point in licensing of storage systems. That really is a DOE question. How much hay do you want to make on that? Right now, we don't have a firm fixed idea of what we need from the vendors and the utility industry by way of disposability on this. There is a connection there. It may or may not be exploited in the future.

So, we talked about overpack performance. There's a few items in Halim's list, a Cesium 133 burn up credit, a probabilistic k-effective approach, a burn up verification tool we're not ready to take on, or we think there isn’t a payback right now, so we're still having the discussions.

Cesium 133 is an abundant fission product not currently in the model but it wouldn't swing k-effective that much. Probabilistic k-effective is a whole new approach to modeling criticality maybe someday. A burn-up verification tool would be something to address misloads. Misloads are kind of the Achilles' heel of the low-probability strategy. You can analyze hundreds and hundreds of DPCs as-loaded to determine whether they'll go critical in the repository, then you have to look at human error in the loading. So, you can switch assemblies in the canister or bring an assembly in that wasn’t supposed to be in that canister. There's different modes of human error you can apply. The more modes, the greater probability of a misload and the misloads have to be considered conservatively. In any event, it tends to demolish your probabilistic model for screening out criticality.

So yeah, so the burn-up tool would be the actual physical tool, an instrument we deploy in the fuel tool. We're not sure it would be accurate enough but it's something we can talk about in the future.

Finally, I wanted to fill in some of the background on some of the other topics we're going to talk about. We have been talking about criticality and waste package for probably more than twenty years. This is a figure I stole from a report twenty years ago by John McClure. It is a RELAP-5 figure that shows power in the package, generated by a criticality event versus time in seconds. The interesting thing about this is, this is a phenomenology we're trying to understand better. Using updated tools but what is happening here is depending on what breached area you assume in the package; the water can egress at different rates. So, if the area is very small, then you the event runs longer before it ejects the water that makes the event possible.

So, the thing quenches itself but it takes longer if you have a small breached area. This is sort of the phenomenology we're looking at. I like to use these influence diagrams. The model I just showed you could be represented if you took all of the green bubbles and set the parameters of degradation as an initial conditions a priori, and then erased all of the dotted lines and you'll have more or less what McClure did, the mark zero type of model.

I think John can describe what Oak Ridge plans to do. They want to start with that and ultimately, we want better coupling between some of the thermal hydraulic processes and ultimately with degradation. Radiolysis can be a part of that as well.

So finally, two slides on fillers. This was raised in our meeting in August. First off, there's been R & D done internationally and for the Yucca Mountain program. Steel shot tested. We can pour a steel shot into the top of a fuel assembly and it will penetrate. That's not an issue. The AECL in Canada did similar stuff with the multi assembly CANDU waste packages. You can pour granular materials in there dry, not a problem. So, we really don't need to repeat that R & D. If we want to cut the lids off the DPCs and dry them out, we can fill them up with dry particle fillers but the only problem is we have to figure out how to get the lids back on. Once you put the filler, it can never be lit again because you'll never get the water out. That's the story of cutting the lids off. If you cut the lids off, there might be other things you can put in canisters besides particulate fillers.

So, we are looking at how you can improve the system as DPCs are loaded in the near future. Couldn't we modify the loading map and put the assemblies in places that favor sub criticality in the disposal environment rather than how they do it today which is thermal management and gamma dose outside of the package.

So, the alternative is the injectable fillers. We cut the covers off the vent and drain holes, pump in the liquid and solidify it under controlled conditions. We're talking molten metals, low temperature melt glasses, aqueous cement slurries. We're considering all of them. This is a list of attributes that we think a filler should have. They are fairly straightforward. I won't read them. Notice, we do want material that is safe. Imagine filling 10 thousand canisters up with molten lead. That probably isn't a good idea. It has to be a reasonable weight. We can't make the canisters too heavy and it needs to cost no more than the cost of repackaging. It needs to be economically effective.

Which brings me to the last slide, based on this, we proceed with these four work packages. The technical and programmatic solutions, getting people with industry experience to think outside of the box and quantify some of the propositions that will help DOE manage the disposition of fuel in DPCs over long term.

So, there's a little bit of feasibility, a little bit of cost estimation in there. The probabilistic criticality consequence analysis, Laura will talk about and the filler program continues. Los Alamos is looking at the degradation of aluminum based neutron absorber materials and exactly how it happens. There are decades of literature on it because the materials have degraded before in fuel pools but then finally, multiphysics simulation of criticality. Here the goal would be to simulate different styles of criticality events – steady state and transient. Look at the energy produced. Look at the repetition rate for episodic behavior and also look at the degradation of the fuel in the basket because that shuts it off.

Those are some of the outcomes. That's what I have, thank you!

I hope there's questions.

>> BAHR: According to our schedule, we have ten minutes for questions. Questions from the Board?

>> TURINSKY: Is there anything going on in other countries looking at direct disposal, dual purpose?

>> HARDIN: So, the only case I can give you is the direct concept of the BGE. They used be DV tech. But that's the one where the CASTOR-V was going to go into Gorleben and they needed a really heavy hoist to do it. So, in salt they had same conclusion that we did that criticality is probably not going to be a problem. John's analysis shows, if there's even moderate burn up, like on the order of 20 giga watt/day per ton then flooding with saturated salt brine and complete removal of the neutron absorbers would be still subcritical for a very wide range of the fuel.

So, the Germans have looked at this and they addressed criticality. All of the other programs use much smaller canisters and so you know, if you have four or fewer PWR assemblies or BWR equivalent, you pretty much don't have a criticality problem, especially if you use a burn up credit analysis.

>> BAHR: Are there questions from Board members, Sue?

>> BRANTLEY: I think you said you liked the diagram on page 23, I think you called it a consequence diagram. I didn't understand it. I would like to like it too. Can you explain it to me? I have never seen one before.

>> HARDIN: This is a simple one. So, we call them influence diagram and it gives you an idea of where to look for results from one process affecting another. The important thing here for

modelers is the directions of the arrows and then they can start filling in what they're feeding. The colors represent how it might be done using different codes coupled externally so the arrows now represent code to code hand offs. Some of them would have to be done with tiny steps because they're tightly coupled and some not because they're loosely coupled. You can imagine the direct feed in that you get from the criticality event, there's an arrow missing there by the way. It would have an immediate impact as you raise the temperature of the UO2 fuel and then you raise the temperature of the water in the immediate vicinity of the fuel rod and so forth. This can be elaborated between the criticality event and temp?

This can be elaborated with.

>> BRANTLEY: So, the missing arrow is between the criticality event and temp?

>> HARDIN: That's hard, because there's an immediate affect that doesn't require you to go through this larger part.

>> BRANTLEY: What's the T,H,N, and M?

>> HARDIN: So thermal hydraulics, neutronics and mechanical.

>> BRANTLEY: So, this is just a schematic of all of the models that you need to put together?

>> HARDIN: Exactly. And it's a little simplistic. John has the MARK5 version with even more levels and even more processes. Yeah, it's a tool. It's an exposition tool.

>> BAHR: Are there questions from the Board? Questions from staff?

>> LESLIE: Bret Leslie, Board staff. I may have misheard it but did you say the current GC859 doesn't include all of the information you need?

>> HARDIN: I kind of said that.

>> LESLIE: So, my question is: How do the information needs get fed into whoever at DOE that generates GC859 so the information you need to do your job is provided?

>> HARDIN: I wonder if I can punt that question to John. Can you take the microphone, John?

>> SCAGLIONE Okay, so we get a lot of the information on what the discharge inventories and the fuel assemblies and what's in the canisters and we're starting to get more information on how they're loaded from the GC859 process. The other data we need is information on the fuel assembly designs and then the canister design specifics.

So, we go through a number of other data bases such as the NRCs, Adam's website. We work directly with the fuel vendors and we work directly with the cask vendors to fill in our gaps so we can actually develop the models that we're looking to build and make our whole case.

>> BAHR: Any other questions from staff?

>> PABALAN: Pabalan, Board staff, on slide 17, you mentioned that one of the strategies for post closure criticality control specifically with respect to moderator exclusion, is over pack integrity. What do you think is a more difficult technical challenge? One, demonstrating the low probability of the early failure from manufacturing defects of the over pack or demonstrating the probability of a breach of the over pack due to a corrosion or lack of corrosion resistance?

>> HARDIN: Well, the question is which would be more difficult, low probability of manufacturing defect or low probability breach.

>> PABALAN: Low probably of breach.

>> HARDIN: Yeah, I think they are equally challenging. I think the low probability of breach, we're familiar with one concept. That would be pretty tough there. There you have seismic as one part of the scenario. We have disruption possible on any given day. So maybe a rock falls but there's other concepts out there where they would not see that. You can use back fill in a shale formation, low seismicity and disruptive events can be negligible, the volcanos and there, you would have hope if you could come up with the right material.

>> PABALAN: Another question is, with respect to the multiphysics simulation of the criticality, can you say more about what computer codes or models you're planning to use for this multiphysics simulation?

>> HARDIN: Yes. So, I'm going to let John address the tight coupling of neutronics and thermal hydraulics. I think he's still prepared to do that.

And then in parallel, I'm working on using a distinct element code to look at the disintegration of degrading fuel and basket material. So, with expressed purpose of applying a seismic ground motion boundary condition to that and shaking it and trying to produce failures that might give us transient criticality. A transient event could occur if you have some gross change in configuration all of a sudden.

>> BAHR: We have a question from Nigel?

>> MOTE: On slide 17, using DPCs only but in other places you referred to opening DPCs for different reasons, putting steel balls and that sort of a thing. How much have you looked at the operational consequences according to where that would be done? Are you expecting the sites to do that, the utility sites or on-site at the repository and what are the implications for the integrity of the canister after you have done some sort of modification which you could take the lid off and rewelding?

>> HARDIN: Good question! That is actually part of the planned work that we have in the technical solutions work package. And I'll drop a name. We have John Kessler working on that. He's a longtime proponent of DPC direct disposal. No, we're not proposing we build a new facility for loading DPCs in a different way going forward. Any change in the way that DPCs are loaded going forward would have to be done at the site to be considered realistic.

There is a hurdle. This is way beyond my pay grade to figure out how to convince utilities to load their DPCs differently. So as far as looking back at it, DPCs, those are already loaded and sealed, you probably would need a special facility with a pool or specialized hot cells for cutting the lids off.

>> MOTE: Okay, thank you.

>> BAHR: I think it is time to move on to our final set of presentations by Laura Price and John Scaglione. It looks like John is standing to start.

>> SCAGLIONE: I am John Scaglione from Oak Ridge National Laboratory. We have the last presentation today. We're going to talk a little bit about the just following on what Ernie and Tim have discussed and discuss what we have learned. I have the same disclaimer. Tim went over that. We covered that. I just wanted to kind of give a little bit of background or elaborate on some of the -- a lot of times when people talk about how much fuel is

stored, they talk about it in terms of metric tons. We wanted to illustrate here as of two weeks ago, we had almost three thousand dry storage systems already out there on nuclear sites across the nation. Storing 125 thousand fuel assemblies. That's a lot of fuel that is stored. This is an important effort we're looking into now because we have a lot of these canisters that are already loaded and we will need to be -- we want to understand what the options are available for handling them in the future. We have about 200 new canisters loaded per year on average. As Tim mentioned earlier, addressing criticality potential in our geological repository is still one of the remaining challenges in just identifying how we are going to do it and what makes sense and developing the technological justification to demonstrate why so it's understood.

So, we're focusing on three major initiatives now. They fall into the blue region there which is the probability side. Then the red region is the consequence side. So, we're doing higher fidelity modeling. What that means is we understand what they did at utilities. We understand what the vendors did for the licensing approach. Put the fuel into dry storage and how they plan on using that information to be able to meet their transportation regulations. And that fuel has already been loaded. We know what they did. Now we're looking at, okay, we're going to stick it into a repository somewhere. Can we do something different or sharpen the pencil in some areas to make it so we're not getting as much margin that they might have thought was acceptable because they met their need.

We're looking at using fillers to displace moderator. If we can prevent moderating material from getting between fuel rods in an assembly, then you don't have the potential for criticality and then moving on to the consequence analysis, and there's two primary types of consequences that we need to consider. One of them is considered, what we call a steady state or a quasi-steady state. Essentially that's a slow approach to criticality as things degrade, water comes into a package. Then you might reach the critical configuration but as soon as you hit criticality, you will start generating heat and water and stuff can evaporate so that could shut the criticality off. But then it could return.

So, it's going to be an ongoing process over many, many years and it could be going on for a long time. Then there's a transient consequence which is an event that can be started or initiated from let's say an earthquake. They have got like, some of my basket has degraded and I still have spacing between the fuel assemblies but the earthquake could cause a basket to collapse. That's a large reactivity insertion with a short pulse and that's another event we have to consider as well.

We're going to talk a little bit about the high-fidelity modeling first. So, this has been discussed a little bit earlier in some of the conversations we have had about bias and how people do their licensing approach versus reality. Everybody tries to do a simplified bounding approach at first and if that works, then you're done and good.

But maybe what is bounding and good for one application might not be the same thing that you need for something else. So, we're we have high level data that can be used to support a bounding analysis. That will get you around out here into the bull’s eye. Right here is where we consider reality. You always have some uncertainty that needs to be accounted for and depending on what you're doing or interested in calculating, for example, if I'm interested in making power and ordering new fuel assemblies and it's affecting my profit margins, I'm going to try to get in the yellow region as much as I can.

Now, for spent fuel, maybe I can live with being in the red region and currently the utilities seem to going through the default option of being in the blue region.

But that causes a lot of margin that might be available in some of these systems that have been loaded that we could recoup when we start looking at direct disposal.

So, one of the tools that we have developed and some of you are familiar with it. We have talked about it in the past. It's called the UNF-ST&DARDS tool. The analysis and resource and data system and this integrates a comprehensive data base on all of the fuel in the U.S. The information we need to develop our models, to support various licensing type analyses and we characterize all of the fuel and the canister systems they're in. So obviously there's a lag of what we have in there because part of it is driven by the GC859 collection effort and some it takes time to catch up with the utilities because they're constantly making and burning new fuel.

And we perform explicit analysis on each loaded DPC. That means that if there's a 3.5 enriched forty gigawatt, that's how we model it. In a typical licensing documents, they will go with either a fresh fuel assumption for storage applications or they will say if it's below 5 weight percent, it's good enough to be loaded. They don't worry about the burn-up or where it's located in the canister.

The way we have been developing the UNF-ST&DARDS, we can actually take credit for each loaded fuel assembly. We do implement full burn-up credit with the 29 actinides and fission products which is consistent with the burn-up credit methodology that was used in the Yucca Mountain license application and it's also consistent with the methodology that is used to support NRC's staff guidance that allows burn up credit with actinides and fission products for PWR assembly.

We take component credits where we can also. A lot of these canister systems that we have been looking at are from the shutdown sites. When they shut down the reactor, they clear the pool. So, a lot of them have old burnable absorber assemblies or old rod control assemblies that are lying around and they stick them in the fuel assembly and the whole fuel assembly would be discharge with the burnable absorber assembly goes in the cask or the canister. We can account for those at least from a moderator displacement perspective that rod is there in the guide tube region.

And then depending on which geological repository we're looking at, we will take credit for items or attributes that might be present that we believe to be justifiable.

For example, in a salt repository, we believe it's okay to take credit for the chlorine and the water that would enter the package. So, of the canister systems that we have loaded, we looked at 616 in explicit detail with 2 primary geometry configurations where we lost a neutron absorber. In some of the packages there's enough steel and hardware components that we believe there's still going to be some separation of assemblies even after the absorber has degraded just because the way all of the other hardware as it degrades will keep some space.

We have seen in some other systems where they don't have the same attributes and therefore, we bring all of the assemblies together in a close-packed cylinder configuration. These configurations are selected based on engineering judgment. There's a lot of ways it can be arranged and, in all likelihood, it won't be all nice and close-packed but you start separating them and letting them go in different orders and the system becomes less reactive. We're still conservative in how we are doing our modeling but we're not necessarily using the same bounding approach.

So out of these, just taking a look at these systems, it showed that if I use the design basis approach and lose my neutron absorbers, the system would be considered critical from a modeling standpoint. If I put in a little bit of, sharpen my pencil a little bit, put in the burnup credit and use the as-loaded analysis, or the loading map, you see we have 473 that pass the criteria for determining whether if it will critical or not. That's 76 percent of the analyzed configurations. That's where the top configuration over there, where there's still some spaces, we reduce that spacing to look at, okay, so let's say I don't get that configuration and I have to go with the tight packed one, what's the impact. It shows we have 68 percent that are passing the criticality criteria screening. So, all I did was come up with a slightly better analysis approach. Right there, we're showing that there's a lot of these DPCs that could be directly disposed and don't pose a criticality concern in the future.

However, 68 percent is not 100 percent which means we have to look at other options. That's where we started looking at the moderator displacement and using fillers. So, Ernie mentioned if we can cut the lids off, we have a lot of options available on how we can add stuff to the package. The way we're looking at it now is, can we use the same process that they're using at the existing facilities. So, when they load the dry storage canister, they are required to have a process to unload it. So, they load the canister, seal it up and put it on a pad and a year or two later, they need to do something to fix it or unload it. They're going to take it, put it back in the pool. The first thing they'll do is drill out the vent port. Those ports, they access the inside of the canister and they're going it fill it back up with water. We believe that that's a pretty simple process and if we can come up with the right material that you can actually inject into the system, that would solidify, that would provide the moderator displacement function.

So that's what the current research is looking at. Is that something that could be done and is it possible and what is the right material but that's one of the areas we're looking into. Currently we're focused on the low temperature metals and cement slurries. We have also developed a multi-physics modeling approach to understand how well this filling process will work. So essentially what we have done is made a simplified multiphysics model. It's actually a CFD model. We have an experimental setup. Right now, we have got it where we have a drainpipe. We have got the mouse holes in the bottom of what we consider the fuel assemblies. So, we have the little nooks and crannies that you need to understand. How is it going to fill? Nice and even? Is it going to be pushed to one side? We want a nice distribution of material as it fills up. So, part of this would be to demonstrate that our models understand how a large system would work. And right now, the first thing we did is to look at, building it simple and adding complexity to it but we're validating the model with the experiment as we add complexity to it.

Our hope is once we get the model set up with all of the extra features that we need to account for the thermodynamic gradients across the system and making sure things are not going to get cool in some region so we can actually fill it before it starts to solidify. Then we can let the model, we can run the crank and find out what are the right materials that are good candidates in there.

We have to look at the viscosity and there's a lot of different factors to consider in how that will come about.

We don't have the way to just keep on practicing and seeing what happens. We wanted to use a computer to do most of the work for us before we get to too many demonstrations.

I want to get to consequence analysis. This is the activity that is just getting started right now. This was basically started this summer, at least the initial parts of it. It's really kicking off now and getting under way. This is a slightly modified version of the bubble diagram that Ernie showed. What it's here to show you is that there's a lot of things that go on inside of a repository that are interdependent upon one another. Criticality is one event that can affect repository performance. It just so happens, if you have a critical event, it affects your radionuclide distribution and concentration and it can affect the heat. It can reflect how other processes degrade and change over time. Fundamentally, you have it -- because the nuclear fuel goes away over thousands of years. You have temperatures, chemistry, you know, water flow rates. These are all going to be dependent upon which host media you're in. And then we have the waste package. There's a lot of variability in waste package and materials and those are all going to have different ways on how they change and degrade and the spent fuel is also going to, have its own processes that it goes through and how it changes over time.

So, at the end of the day, our primary concern is how is the repository performing and then we need to understand, if I did have a DPC criticality event, how does it impact the repository?

So, the primary consequence metric is going to be dose to the accessible environment. Does it change the value. So, we're looking at the consequence of criticality and how they would impact our repository performance model. Our current activities are looking at developing a generic performance assessment modeling capability that can account for perturbations and parameters that are affected by a criticality event.

Predominantly you have the power from the critical event, the duration which translates to heat and pressure, the thermal hydraulics, the neutronics, and the mechanics.

And then everything doesn't have to be coupled. We can actually look at having a sub model which is the DPC criticality model and understand how what the magnitude of those perturbations can be and then make sure it's reflective in the overall assessment model and then from that, we can make a determination whether it should be included in the other all screening analysis for developing a future license.

Now, I'll hand it off to Laura Price.

>> PRICE: Thank you John.

So, I'll talk about the DPC criticality modeling work. So, the objective here is to develop an approach to modeling the consequence of criticality on a repository performance. Either we can develop an approach that would screen criticality from the PA on the basis of consequence or if you can't screen it out on the basis of consequence, it would be included in the PA. A couple of things to point out about this. First, we're assuming that the regulation that would apply to some future repository is similar to one that was used for Yucca mountain. What we mean by that is that the rules used to screen future events and processes, to include them in the PA or exclude them are similar. Both in the rules, in the scope, and the timing.

And that the performance measure would also be dose and the post closure time is 10,000 years, a million years would be something carried forward. I would also like to point out that for looking at developing an approach, we can't answer the question right now because that's a very site specific question on whether or not criticality is something we can screen in or screen out. It would have to be included in the PA. We can develop an approach but the final answer is site specific.

The last thing I would point out is criticality can only happen if water has entered the waste package. So, any criticality consequence we calculate are superimposed on any releases that have already occurred without a criticality occurring.

So, we're not looking at steady state criticality as John described. We're not talking about failing another waste package but what additional consequences are there from criticality after a waste package has already failed. That's an important point to make here.

So as John pointed out, we're just starting this. Right now, the current package has a two-phase approach. We're on the first phase since we just started. The first phase is scoping out what we're going to do. That's what we're talking about today. The second phase is execution. The first phase is scheduled for completion in January. We have a report due at the end of January.

And the approach we're taking is consistent with the DOE's topical report that came out in 2003 and when I say it's consistent, a couple of things this DOE topical report talked about. It talked about the two types of criticality that can be considered which John talked about, both the quasi study state and the transient. It also talked about how the most important effect is probably the increased inventory and we'll look at that and then the temperature effect and how the increase in temperature may affect the barrier system degradation, corrosion rates and that sort of a thing. We'll also look at that.

We're looking at the consequence. I'm not looking at the probability for this particular study or how a waste package fails or why it fails. We're assuming from the get go, from the start of the study that the waste package has failed and we have water entering the waste package.

We're also looking only in-package criticality. We're not looking at criticality outside of the waste package, the near field or far field. We will include uncertainty and variability as appropriate and as we can and as a first cut, we will have to adopt some bounding assumptions in some instances.

So, we're not reinventing the wheel here. The DOE did some studies on criticality consequence twenty some odd years ago. This is some of the root studies that were done that I cite here. The first one was for tuff and DOE owned waste, spent nuclear fuel and high-level waste. Only about one percent of the inventory increase in one percent over 10,000 years of criticality. They looked at all of the waste packages that had the spent nuclear fuel and they assumed a steady-state temperature just below the boiling point. What is important about some of these studies is that they pointed out how important it is to know how much water there is and the infiltration rate of water. That controls the power and duration of the criticality event.

The second study up here, they showed total Curies increased by the 24 percent but that's also a conservative calculations and that it's not implying a 24 percent increase in dose. They did not take it up to dose so I can't tell you what that would be. And this last one here was the transient event and they said there's no negligible inventory increase and there was no effect on other waste packages which would be the primary concern for a transient event. Will the power burst from a transient event affect the neighboring waste package.

Here are a couple more studies that were conducted years ago. This first one up here, Ernie showed the diagram for it. It showed how the pressure and the temperature and the power output changes as a function of the waste package egress area and the second one was done by the NRC.

They took it out to TSPA calculations and they showed steady state dose increased by factor of three and they calculated in a transient case, the dose increased by an order of magnitude, but the risk was small because they calculated the probability.

I think there's some ways in which we would like to go beyond what was done here and the ways that we're doing that are different. One is obviously we're looking at dual purpose canisters. These dual-purpose canisters did not exist when the studies were done. They looked at multipurpose canisters in 21PWRs. We're looking at John said, the as-loaded configuration and using burn up credits. Also, we will look at the solubility of radionuclides as a function of criticality. If there's a criticality event, create a chemical environment that affects radionuclide solubility in the waste package. We don't know the answer to that. We're looking at a saturated environment too. We would like to look at also unsaturated environment. I think a saturated would be interesting to look at because of the availability of water.

And then as Shawn and Ernie mentioned, we're trying to couple performance assessment codes and neutronic codes and as Ernie also mentioned, we will also be looking at what stops the criticality. At what point do you lose the configuration or is there some other thing that eventually stops the criticality? These studies that I cite here, they all assume that criticality went on for 10,000 years and then stopped, but that's the assumption that was made and we would like to investigate that further.

So, we're going to look at a saturated environment which would be shale and an unsaturated environment in alluvium. And for each of those, we're going to look at the quasi study state and the transient event. So, here's some of the parameters and phenomenon we want to include. Some will be included more explicitly than others and some more quickly than others and some may be side studies. As John mentioned, we'll look at the as loaded inventory and the corrosion products, their chemistry, the moderator exclusion effects and loss of configuration when you have corrosion and burn up credit as John mentioned and the shut off mechanism for criticality.

Obviously, temperature and pressure. Those are the two primary effects and then the fission and activation and radioactive product generation, radiolysis chemical effects and then from the transient event, looking at mechanical effects.

So, our strategy is to pick a DPC to be modeled. We’ll start with one DPC. We'll develop a conceptual model for how the criticality occurs and progresses over time. We will have a conceptual model for shale and alluvium, for saturated and unsaturated, and then when criticality occurs, these sorts of things. And then we'll put, employ a computational performance assessment model, brief conceptual model and right now, the model we're going to employ is called PFLOTRAN. Then we'll calculate the time dependent outputs for each model and you can see what they are as we discussed previously. Temperature, inventory, radiolysis and chemical effects and then mechanical damage. Then we'll run the model with a criticality event and without to see the difference. What would be the dose with criticality and without criticality. That is all I have.

>>BAHR: I have a question about your choice of host materials. I don’t know of anyone proposing building a repository in alluvium.

>> PRICE: Part of it has to do with our linking to the geological disposal safety assessments work. And they have models built in PFLOTRAN for that. So, we're trying to leverage off of existing models we have. That's why. It's mostly that what we want an unsaturated environment that we have a model for. We're not trying to reinvent the wheel there.

>>BAHR: Why not use tuff if you want a unsaturated environment that people have looked at?

>>PRICE: That's the hot potato and we don’t have a model for that in PFLOTRAN.

>> BAHR: Other questions from the Board? Questions from staff? Nigel?

>> MOTE: You were here this morning so you heard the conversations about being able to compare experimental results with models. What can you do to give some confidence that your modeling is anchored to, I would say the real world and I'm not trying to be provocative. I don’t know how you set up some practical experiments, for lack of a better word to get some physical results that can demonstrate that your models are reflective of the real world.

>> PRICE: That's a good point! The only physical reality we know of is the Oklo natural reactor which we think a disposal package criticality would emulate. So, it would be something like Oklo but right now, I don't have plans to do that. I'm not sure how we would but that's something we can think about.

>> NIGEL: Okay, so Oklo was a natural phenomenon. There was no packages or disposal regime with other materials around. So, I think to get to something which represents canisters with all of the internal structure in an enclosed rock environment, would be different from that which is exposed and was a very high

concentration uranium resource, naturally enriched. I would suggest it to be a very different case.

>> PRICE: Thank you!

>> LESLIE: Board staff. It’s probably been more than ten years since I looked at disposal criticality report. My recollection is it outlines screening out either probability or consequence yet in the rest of the performance assessment you can screen out on risk. In other words, you can take probability information and you can take consequence information and say it doesn't matter. Does that, NRC’s approval, allow you to basically marry the two approaches? In other words, you don't need a real sharp pencil for either probability or consequences if you approach it from a risk perspective.

>>SCAGLIONE: The answer to your question is yes. The topical report did have a pathway on how to screen on probability and consequence. The thing is, we never had to go the consequence route because we were able to meet the probability screening criterion therefore, it was kind of on the back burnered. We had a lot of flexibility. We were designing the waste package at the time so we could instill certain features that would help us screen in the base of low probability.

>> PRICE: I have to say the criticality report section 3.6 discusses screening on probability and section 3.7 talks about consequence but it is written in the context of including it in

the PA. So, they're talking about risk of what you would do in a PA.

>> FOUFOULA-GEORGIOU: Is that correct?

>> PRICE: -- With the influence diagram? We don't hope to model all of it this year.

>> FOUFOULA-GEORGIOU: Yeah, I understand. This is a common approach of putting down the processes. It links them with the expected interactions and again, my understanding is that you don't plan to use it as a predictive model right away but also, as model that can help you understand what models or couplings are the most important.

This is basically graphical expression of a coupled system of nonlinear equations and we do it all of the time. If the link of one versus another is stronger or weaker, then we want to see how it propagates to all of the other processes. So, is that something I understand correctly?

>> PRICE: Yes!

>> FOUFOULA-GEORGIOU: Okay, I agree with the approach. I would still consider it as the exploratory model which is very important before we get into the full scale, you know, predictive model.

>> BRANTLEY: I'm just curious and I know an earlier speaker talked about one of the biggest issues could be here is just human error in packing. So, you know, you talked about 68

percent of the packed DPCs out there, you think there's enough safety with it but what about the human error? Are there studies of the rates of errors of people packing these and how they're put in there? You also mentioned they put in moderators sometimes and I would think maybe that's not as important. Maybe that's basically like a safety thing so maybe that's not always noted correctly or something like that. I mean, how are you going to model that?

>> SCAGLIONE: Okay, so there's a couple of different factors that are going on with that. We do have a misload analysis model as part of our methodology. We understand how a misload can affect our results but the issue that we have is that this canister was loaded for storage. It's going to be certified for transportation, meaning that what is in the canister is known. Therefore, it's going to come to a repository and it's already gone through two NRC check points and all of the sudden, you know, us having to account for a misload is a little bit that's something we're going to need to discuss with the regulator in the future if it's something we need to consider.

Now, for the shutdown sites, they have gone through and emptied their pool and they didn't have a case where they went to go and pull a fuel and someone said, oh, we thought we already loaded that one. So, they know exactly what they loaded and where. So, the misload that we have considered in our model is okay, let's say they put the right assemblies in the canister but maybe it's in a different configuration. We have a methodology that allows us to understand what's the worst case, what's the worst impact on criticality potential if they did load it like that. That would be a strategic discussion in the future if you're going to go into licensing. Do we get credit for the certification of what was transported or do we have to include that as part of our overall methodology?

It impacts the number of what I have there, like, 68 percent or something like that. They dropped it to like 62 percent. We had it based on the 617 we ran.

>> BRANTLEY: So, let me see if I just understood what you told me. First of all, you said you have no evidence that assemblies have ever been put together incorrectly. Like, each reactor knows exactly what they put in there. There's never been a problem there. So, the problem is when the assembly goes into DPC, the question is do they always get that right? Do they always record exactly where they put them in the DPC? You have no evidence about that. And so, the way you're going to put it in your model, you're thinking about, putting it in is to say worst case scenario so you don't any data on what each assembly goes where -- so you’re just going to assume the worst thing, this could have happened and put it in terms of criticality calculations.

>> SCAGLIONE: That's how we have it implemented in our methodology, so we err on the side of conservatism. If there's error where we put it in the wrong spot so there is some industry issues but they have always identified it or corrected it at some point. They understood they had a mistake somewhere. It got captured.

>> TURINSKY: They're talking in a reactor, they go down with a camera and go over every assembly and read the serial number. Now, whether they do it when they're loading canisters, I don't know. That is what makes it -- procedure make it low that they did it correct the first time but the serial confirmation will catch errors.

>> BRANTLEY: But they still have to put it in the DPC.

>> TURINSKY: What I'm saying is --

>> TURINSKY: The DPC, maybe the people who have observed what went on can comment on it. Do they do the camera scanned in to look at the assembly serial number which identifies it? I don't know the answer.

>> You don't know that?

>>BRANTLEY: This says something about 68 to 62. I didn't understand that piece.

>> SCAGLIONE: I didn't show the results. We have looked at the possibility of misload and what the worst-case configuration is. Assuming that the right assemblies are in the canister but

they're arranged in a different configuration. So, they didn't technically load it according to their plan which is a misload. How that affects criticality potential, I have a number of 68 percent that were acceptable and they dropped the number a little bit. It wasn't a huge impact on the existing number.

>> ILLANGASEKARE: So to know a little bit more about the multiphysics simulator. The way you are looking at is, you can build to find it important or are you looking at a simulator like consult -- is it a package? Or does it have the ability for multiple processes?

>> SCAGLIONE: So, you're talking about the filler. We're using star CCM.

>> TURINSKY: That's a good question. Have you looked at the uncertainties due to the engineering uncertainties? You guys are really good at Oak Ridge about uncertainty. In a reactor, NRC9595, that's almost two sigma. In a reactor, that's about 1200 PCM. I have no idea what it is after the fuel has aged 10,000 years or anything else. Have you folks worked at that or are you planning to look at it?

>>SCAGLIONE: Currently, our methodology for the depletion portion and critical portion is following a couple of NUREGs out there that support a methodology that the NRC finds acceptable for burnup credit, that is the ISGA methodology.

>> TURINSKY: But that's short lifetime fuel, right?

>> SCAGLIONE: That's right. But we understand how the material changes over time. We do factor in the uncertainties. We know how that's going to impact our end number on k-effective. Those are all very, very small impacts. Now, that's provided we get our initial inventory correct the first time.

>> TURINSKY: So, it's much smaller than a reactor which is like 1.2 percent of two sigma.

>> SCAGLIONE: Right, it's smaller than the reactor.

>> BAHR: Are there questions from the Board? Other from the staff? Bret Leslie

>> LESLIE: Board staff. You have done 616 out of 2400 that are out there. That's based upon the information that you have been able to obtain so far, right? With the next round of GC859, is it going to be 50 percent rather than 25 percent? If you go to your bulls' eye, you're only doing the conservative in GC859. Any other thing, you need to get the proprietary information. As Mike pointed out previously, there's a lot of sensitivity about sharing the proprietary information.

What I'm trying to figure out is, how close to a one hundred percent can you figure out which you're going to gain?

>> SCAGLIONE: So, we have a couple of parts to answer that question. We received a lot of the loading maps last time. Approximately 1700 of them. Because it was the first time we asked for them through the GC859process, a lot of them came in.

They weren't actually usable for us to develop our detailed models because we said, please tell us how you loaded your package and any way we can think about how they did submitted it did. So, we gave more instructions on the format we're looking for this time. We believe it will be better. Once we get that, we believe we can double or triple it easily.

Then for the other part of your question, on the proprietary data and that type of information, we are working with some of the vendors in getting access to the detailed proprietary data so I can understand how much I'm losing by using my coarser data I get from the GC859 and if we find something really killing us, we're going to really need to apply it or right now, we are getting access to some of the detailed data so we can understand how off we are.

>> BAHR: Any other questions? Any questions from the audience? State your name.

>> AUDIENCE: Darryl Lacy from NYE County, Nevada, I want to thank for looking at the dual-purpose canisters which is good cost and exposure issues. Nye County had those as contentions for the Yucca Mountain licensing process. So, we think it's important to look at. We spent a lot of time looking at exclusion of water and moisture inside the canisters that you indicated one of your fillers might be a water-based type of

cementitious filler and I was wondering if -- polymer or other (inaudible).

>> HARDIN: Good question, Darryl! I can address the polymer, the organic part of the question. The gamma dose is similar, the filler material would receive over hundreds of years is somewhere in the neighborhood of 50 to 100 million greys per kilogram. It's a high dose. You can look up from a well-known vendor on the survival ability of certain materials and there's virtually no polymers that can go the distance. That's a little problem that we have.

The other question is about water and dewatering would be an important aspect of using an aqueous cement slurry.

>> Thank you!

>> BAHR: Anything else before we go to the public comment period? Okay, we have one person signed up. Leora Morgan from The Nuclear Issues Study Group.

>> AUDIENCE: Good afternoon, I just want today make a comment on the interim storage in New Mexico to let you know there was letters sent from the legislature and a comment made by governor Martinez that New Mexico wants these facilities and I'm here to say the communities in the local vicinity, other communities in the state do not care to have these facilities in New Mexico. We feel it's incredibly unsafe and it's not tested or a proven technology. There's also many communities that have passed

resolutions opposing the transport and myself, I'm a member of the Navajo Nation and we have a law against the transport of radioactive materials through our lands. Although the railroad would be people say, well, the federal government would stipulate there's -- that they have jurisdiction over the railroad and all over the country, we're worried about the state of the railroad and particularly the Navajo Nation. I would argue this is our land and it was stolen by the federal government so we make these laws to protect our people and we have already dealt with a lot of contamination created by the United States and the weapons program, mining uranium and milling. So we have many health impacts and contamination that have not been paid for by the federal government to clean up and so as a member of Navajo Nation, I was just letting you know this law was passed in 2012 and according to the recent passage of the Organization of American States Declaration on the rights of indigenous people, our Nation has the right to create and enforce our own laws so even if the federal government says it supersedes our jurisdiction, this is our land and it's already been contaminated so we don't these casks or the state of the aging reactors and how they handle their waste and I was just visiting San Onofre and I know about the incident there that could have led to criticality and so we know the facilities where they're out, they need to be shut down. They need to stop creating the waste because there's nowhere to put it.

And in New Mexico, there are a lot of people who are concerned about this. So, I'm speaking not just as the member of the Navajo Nation but also a resident of the State of New Mexico.

Thank you!

>> BAHR: Thank you for your comment. Any other public comments? Well, I thank you all for your attention. Thanks to the presenters. Please state your name and your affiliation.

>> AUDIENCE: My name is Eileen Shaughnessy. and I'm also with the Nuclear Issues Study Group. I just want to add to what was just said in terms of many people here in this state who are very opposed to the siting of a central interim storage facility in New Mexico.

Our state has been over burdened by environmental racism, and by the negative impact of almost the entire nuclear fuel chain since the beginning of the Manhattan Project

And I have a feeling one of the reasons you're meeting here in Albuquerque is because of the proposal but I just want it to be known there is a lot of resistance and a lot of concern regardless of these tests and these studies that you have spoken to today.

By in large, people are very concerned about transportation risks, about the risks involved with these casks and when it comes to spent nuclear fuel, what I want to be put on record is that we have to talk about 100 percent certainty and no one here can guarantee that for 10,000 years or more. You all have a very big task in front of you to be the Board that is looking at these issues and I commend you for doing that work and I hope that you would be able to do this work, keeping in mind the sanctity of our water, the sanctity of our soil and future generations and so, when we talk about transporting materials across the country past hospitals and elementary schools, on the existing rail system, there are so many concerns that I don't believe you can tell us there's one hundred percent certainty that something would not happen.

So, thank you!

>> BAHR: Thank you for your comment. Just for a clarification, the reason we're meeting here has nothing to do with the interim storage proposals. We're here because Sandia National Labs which has done a lot of the technical work that we have been listening to is a place with the Department of Energy. Again, state your name and affiliation.

>> AUDIENCE: My name is Karen and I'm a resident of Albuquerque and a member of CARD, Citizens for Alternatives to Radioactive Dumping. I'm not speaking on behalf of that affiliation. I'm very concerned about the lack of one hundred percent certainties when we're talking about human life. The fact that so many New

Mexicans, including the Navajo Nation and the pueblos have felt betrayed by the federal government because we feel we're a national nuclear sacrifice zone.

So, when the DOE went to see if someone would welcome the interim storage, somebody from Arizona said, what about New Mexico, no one cares about New Mexico. People back east would be very happy to have the nuclear waste somewhere else, out in the desert where there's no people. But there are people. We're here. We are very concerned. I'm especially concerned about sabotage.

There's so many crazy people who are willing to have their life blow up, their bodies to blow up in order to stage some action movie type thing. If it's going to be tested in a thirty-foot nine-meter rock, what about 100 feet? How about simulating a first sabotaged with IEDs in the nighttime before the train arrives, placed all along and triggered by the passage of the train? And the train plunges into a boulder filled bed. With or without water. The boulder is sticking up and not in a thunderstorm until the casks are crashing into these boulders, perhaps ricochets off the canyon walls. I don't see anyone imagining what I would consider a true worst-case scenario and in the age of terrorism, we have to consider. Thank you.

>> BAHR: Thank you. Well, I think that's it for other comments. Again, thank you for all of the participants. And I hope that

the rain has stopped outside, although I know New Mexico needs rain.

(The hearing concluded.)

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