Official Transcript of Proceedings

NUCLEAR REGULATORY COMMISSION

Title:	Advisory Committee on Reactor Safeguards Reactor Fuels Subcommittee - Open Session
Docket Number:	(not applicable)
Location:	Rockville, Maryland
Date:	Tuesday, September 30, 2003
Work Order No.:	NRC-1079 Pages 1-152/229-281

NEAL R. GROSS AND CO., INC.
Court Reporters and Transcribers
1323 Rhode Island Avenue, N.W.
Washington, D.C. 20005
(202) 234-4433

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

\+ + + + +

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

REACTOR FUELS SUBCOMMITTEE

\+ + + + +

TUESDAY, SEPTEMBER 30, 2003

\+ + + + +

OPEN SESSION

\+ + + + +

ROCKVILLE, MARYLAND

\+ + + + +

The subcommittee met at 8:30 a.m. at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, Dr. Dana A. Powers, Chairman, presiding.

SUBCOMMITTEE MEMBERS:

DANA A. POWERS	Chairman
F. PETER FORD	Member
THOMAS S. KRESS	Member
GRAHAM M. LEITCH	Member
VICTOR H. RANSOM	Member
JOHN D. SIEBER	Member

ACRS STAFF PRESENT:

RALPH CARUSO

ALSO PRESENT:

BO CHENG	EPRI
JEFF DESHON	EPRI
KURT EDSINGER	EPRI
ODELLI OZER	EPRI
ROSA YANG	EPRI

I-N-D-E-X

P-R-O-C-E-E-D-I-N-G-S

(8:30 a.m.)

CHAIRMAN POWERS: Back in session. This is the second day of the meeting of the Subcommittee on Reactor Fuels for the Advisory Committee on Reactor Safeguards. We have a much more robust subcommittee today to hear about the robust fuels program.

In attendance today are: Jack Sieber, Peter Ford, Tom Kress, Vic Ransom, and Graham Leitch. And I trust you gentlemen who are new today will stay with us for the duration and not go off to some silly license renewal or other sort of thing. It's clear that fuel is essential to the nucear performance, and we want to see what the reaction is today.

Do any of the members have an opening comment that they would care to make?

(No response.)

CHAIRMAN POWERS: Well, for the new attendees, I will comment yesteday we had an examination of about two-thirds of the NRC's fuels research program. And I invite you to take an examination of the transcript and the viewgraphs. I think you will find it will be quite interesting, if somewhat reminiscent of what we heard last year in a similar subcommittee meeting.

Today we are going to examine what is going on in the EPRI program in the form of the robust fuels program and a few other topics. And so without any more introduction, I will just turn it over to Rosa Yang, who will guide us through all of this.

10) ROBUST FUEL PROGRAM OVERVIEW

MS. YANG: I don't know about that.

Good morning. I am Rosa Yang, EPRI. I manage the robust fuel program. About a year ago, October 9th, the industry, EPRI, and the fuel suppliers and some utility folks were here at the invitation of NRR to discuss the topical report, the RIA topical report that we submitted a few months ago. And during that presentation, we were requested by this committee to come back and talk about the robust fuel program.

Specifically, I was asked to say, bring all your staff. So I did. And here they are. I am not going to take the time to introduce them because you will hear from every one of them.

The only comment I want to make, as you will see, what I will try to do is to give you an overview of the program, how it was formed, why it was formed, and how is it functioning now in the industry. I won't get into too much technical detail because most of them will be covered by EPRI staff later on. Feel free to interrupt any of us.

Because much of the work we do is proprietary, I'm very sorry. I hate to do this, but after my presentation and after the presentation which will be made by Odelli Ozer about the letter that we have sent to NRC -- he will make a brief presentation just by the content of the letter because it was discussed yesterday. After those two presentations, the rest of the presentation will be closed to the public. My apology for that.

If there is anything specific you would like to talk about and you are not able to participate in the rest of the presentation, we will be happy to talk to you about it. The reason we do it in this somewhat nonacademic fashion is because much of the work we do, we have special contracts with the fuel suppliers. And being fuel is a fairly competitive commercial environment, we need to honor those contracts.

One more thing I want to say is the handout you have, I'm going to follow in most of it until almost toward the end. Then I add a few slides about the RIA topical report that we submitted more than a year ago just to address some of the questions raised yesterday. So I intend to make a brief presentation of that subject. So you don't have that in the handout, but I will be happy to make those slides available later on because I just added them last night.

So let me start. The program was formed actually a couple of years before 1998 and officially kicked into work in 1998. There are two key reasons for the formation of this thing. One of them I will give you a little bit more detail, which is we are operating considerably differently today and maybe the last few years as comparing to, say, 10-15 years ago.

I think you heard a little bit from APS yesterday. The guy was talking about there is more boiling in his plant. So I will give you a few examples of how the environment has been more demanding.

The second reason is because the fuel is a fixed market and is very competitive. The price of the fuel has been going down steadily as a result of this competitive environment. So that doesn't leave enough money for the fuel suppliers to do R&D.

So you have a difficult situation. On one hand, you operate in a more demanding environment. And at the same time, we put on new products, which may not have been tested as it deserves. So sometimes, not all the time, sometimes, they don't perform as advertised.

So utility decided that they need to do something a bit proactive to form a program like this to make sure that they don't have operational surprises and they have regulatory stability and they have the flexibility in operating their plant because, like Dana said earlier, fuel is where the energy comes from.

Maybe after we solve all of those problems, we can look at burnup extension. So first I want to clarify that this is not just about high burnup. This program is about operational flexibility, about no surprises, about regulatory stability, and then the next step is burnup extension.

As I go through them, you will see most of the program content that we aimed at. Many of them are below 62,000, which is a current licensing limit.

MEMBER KRESS: That looks like the guy could probably high jump over that and --

MS. YANG: Oh, yes. I forgot to give due credit to one of our members, EdF, Nicolas Waeckel of EdF. He's looking at a PWR fuel assembly and this typical utility guy, trying to make a high jump. And, of course, he is armed with all of these robust fuel program reports, which you can't quite see the reading, but if you magnify enough, you can see.

And the first step is operational margins, which is what we keep talking about. We want no surprise. If you don't want surprise, you've got the margins. And after this first step, then the next step is burnup extension.

Of course, we are doing all of this with NRC's watchful eyes and all of the reg documents.

MEMBER KRESS: What's that thing up in the right-hand corner?

MS. YANG: This one?

MEMBER KRESS: Yes.

MS. YANG: Twenty-First Century, when we move into the future.

CHAIRMAN POWERS: Well, I'm surprised that you don't get fuel bowing when you use it that way.

MS. YANG: He's a science guy, electricity guy.

MEMBER KRESS: A good pole vaulting pole is very flexible.

MS. YANG: This is robust PWR stuff.

MEMBER SIEBER: Leave it to a utility guy to misuse the fuel.

CHAIRMAN POWERS: Yes. He's probably an accountant.

MS. YANG: Okay. So a bit about the changes to make the environment more demanding. We have increased discharge burnup. We have extended the cycle length. I have slides of those to show you. And the peaking certainly has been increased, mostly because of the low leakage core. The enrichment has steadily increased. And I have a slide to show you. And the newest thing is plant uprate.

You know, they are the small one to 2 percent ones, and then there are the 20 percent ones. And how that impacts fuel performance needs to be assessed.

Just overlay all of those to protect the plant. Many of the new water chemistry has been implemented. I listed the ones for PWR is elevated pH and lithium and zinc injection; for BWR, noble metal chemical addition, -- sorry for the acronym, noble metal chemical addition -- hydrogen water chemistry, and zinc injections. And, of course, there are design changes and new material introduced to satisfy the more demanding environment.

Not all of these things are adverse in terms of fuel performance. And, in fact, I would argue many of these are intended to improve the performance. It's just when you have new things and you make many changes at the same time, you could reduce a margin. And you certainly increase the unknown.

So let me show you --

CHAIRMAN POWERS: Rosa, how is the beginning-of-life boron changing in the plants nowadays?

MS. YANG: How many what?

CHAIRMAN POWERS: The beginning-of-life boron concentrations changing in the plants?

MS. YANG: It's higher. You know, as you go to higher, I think now the steady state boron is what, 1,800 or so, 18, 16. Yes. Eighteen hundred, yes. So it's increased considerably.

I think maybe ten years ago, you were looking at --

CHAIRMAN POWERS: Eight hundred.

MS. YANG: -- 900 or 1,000, yes.

CHAIRMAN POWERS: So we double the boron concentration.

MS. YANG: Yes.

MR. OZER: Longer cycle is required for boron.

MS. YANG: Here's the list of cycle line, in effective full-power days. As you can see, from 10 years ago or 12 years ago, we just steadily increased the cycle lines for both BWRs and PWRs. I think most of the BWRs are 24-month cycle now.

Look at the enrichment. Again, these are the average enrichment for the reload. It's steadily increased because of the mix of enrichment and all that. So we haven't quite hit the five percent limit, but it's pretty close there.

I told you about uprates. The different color shows different percent of uprate. As you can see, we have a considerable number of 20 percent uprate. So these are --

CHAIRMAN POWERS: No one is more familiar with the number of 20 percent uprates than this committee. You're killing us.

MS. YANG: Well, so far what we have looked into, we haven't really found a smoking gun in terms of uprate and how that impacts fuel performance. But it's just intuitive something you need to look at.

Okay. This is how our program functions. All the work or the majority, the engine is certainly with the working group. And there are four working groups. And you are going to hear from three of them today.

We left working group 2 out, which is dealing with RIA, LOCA, and burnup extension. The reason we left it out is because we know you were going to hear a lot about those issues yesterday from NRC research side, and we have a topical report that we submitted that we would discuss it separately with NRR and we made a presentation a year ago. So the original intent is not to spend too much time on that because we really have a lot of material to cover in other areas.

Given yesterday's presentation and discussion, I just thought we would have a brief one on RIA only to just refresh the committee members' memory.

Working group 1, working on the mostly PWR crud and water chemistry-related issues; and working group 3, working on hot cells and other high burnout properties; working group 4, working on the failure mechanisms. You will see the list of the utility chairmen and the EPRI project managers. As I said earlier, you will hear from each of the EPRI project managers.

So those are the work engine of the program. And the integration is really just the chairman and vice chairman of each of the working groups. And the executive committee and the senior reps, they are the management of the program. They are the ones deciding on program directions and budget. So it's a very important function.

And these are the executive committee members. You will see we also have nonvoting members from NEI and INPO. The current chairman is Joseph Sheppard of South Texas Project.

Here is our members. I just want to point out -- I guess I don't have a pointer, but I can use this. I just want to point out for the U.S., we have I think it's 20 utilities and about 80 units participating in the program.

I get criticized all the time when I talk about members because we have this interesting funding scheme. Part of our funding comes from membership. Part of it, the utility has to pay additional money. When I talk about members, I am talking about those who pay additional money. So these are the membership.

We have 103 units in this country. About 80 units fund the program supplementally. We have a considerable number of international participation.

There are nine groups here listed, but if you look at number of units, they are actually more than the U.S. And, of course, we work very closely with INPO, NEI. Ralph talked about NRC research yesterday. We work with all of the fuel suppliers: Global Nuclear, Westinghouse, and Framatome.

So, for the rest of it, what I would like to do is to give you a very quick overview. First, I would like to start with what are some of the issues that face the industry. And then I want to go into what are some of the areas that we work on.

The issues facing the industry are there is a large number of BWR failures I think many of you have alluded to yesterday. And the root cause so far is unknown. You will hear a little bit more detail. And I would just give you a really quick 30,000 feet overview of the situation.

NMCA and zinc injunction, many of the BWRs are using it. And the impact of that on fuel currently is being assessed. We have channel bow issues, which interfere with the control blade insertion as some of the BWRs, which probably reminds them of the PWR incomplete control rod insertion many years ago.

On the PWR side, the major failure root cause continues to be grid-rod fretting. I just want to say we're not doing much here because this is purely a design, fuel design, issue.

We have looked at the issue. We have held workshops. So we have a good understanding of the issue. And then we, the committee, have decided that this is a vendor problem and a vendor-specific. And we highlight the issue. And I think there are measures in place to address them.

To most parts, they are somewhat effective. The problem is still here, but they are being addressed. So we, the robust fuel program, are not addressing it.

Axial offset anomaly still exists, but they are current. The frequency and the severity are down significantly from a few years ago. We have a very active program addressing this issue. And you are going to hear from Jeff later on about it more.

The PWR RCCAs or the PWR control assemblies have a cracking and deformation problem. And we have a program initiated to address that. So if you just look at the 30,000 feet view, what are some of the key performance issues facing the industry? And these are them.

Just a sort of a bit of background on the things I already said, here is the number of defect assemblies as a function of time. You know, some of the earlier days, you'll see I have a breakdown of the failure mechanism in a minute. For the past ten years or so, things are beginning to improve, especially for the boiling water reactor. And you see a big increase of the boiling water reactor in the last couple of years.

Here is a trend plot. Again --

MEMBER LEITCH: On that previous one, I'm trying to understand the scale here.

MS. YANG: I know.

MEMBER LEITCH: It's one and a half? What is that? One and a half what?

MS. YANG: It's fuel assemblies, number of defective assemblies per gigawatt, per thousand megawatts. So it's roughly one and a half assemblies per unit, per large unit.

MEMBER LEITCH: Yes. Okay. Got you. Okay.

MS. YANG: Which means every reactor has one.

MEMBER LEITCH: Yes.

MS. YANG: Of course, it's not like that.

MEMBER LEITCH: Yes. On average, yes.

MS. YANG: Okay. So here is a trend plot

of the type of failures for BWR. So, as you can see, the last couple of years, we have the two dominating failures are corrosion-related failures and PCI or PCI-like types of failures. Bo will review for you in great detail later on.

For the PWRs, I think I already talked about it. The major failures are grid-to-rod fretting.

CHAIRMAN POWERS: Why is there a large number of unknown, not inspected?

MS. YANG: Why?

CHAIRMAN POWERS: What does that mean?

MS. YANG: Oh. It means that they don't inspect it. In many cases, they have it either in the discharge batch that they know they have a small failure. They just didn't inspect it or sometimes when you say, "unknown," they know they have a failure. They probably even know where it is, but they just don't know what the root cause is.

You see, the others, we actually classify them in terms of the root cause. Sometimes a visual inspection at the pool does not yield to identifying the actual failure root cause.

I think it is fair to say those are maybe ones, two and they are very small. I think whenever we have a large number of defects or there is something like the BWR failures that we don't understand, I think the industry has been pretty good in going after the root cause.

CHAIRMAN POWERS: The scale on this particular plot, of course, is different from the other one. Is this the integral number of failed assemblies in the fleet or is it on a per-reactor basis?

MS. YANG: In the fleet.

CHAIRMAN POWERS: In the fleet?

MS. YANG: Yes.

MEMBER KRESS: What is this debris, the yellow?

MS. YANG: Yes. That's the foreign material intruded in the system, either from steam generator repairs, or whatever foreign materials.

MEMBER LEITCH: You talked about PCI. About a dozen or more years ago, there were interim management recommendations for operating to avoid PCI. The plants are not operating under those bases any more, are they or are they? I don't know.

MS. YANG: No, no. In this particular case, these plants are using a type of barrier, which allow in principle should work to prevent the PCI failures.

MEMBER LEITCH: Okay. But what we're saying is, even with the barrier fuel, then, apparently there still are several PCI failures.

MS. YANG: We think it's PCI, but these we don't really know. I mean, I think we call it PCI suspect. We think it's PCI, but we haven't really confirmed it's PCI.

MEMBER LEITCH: Okay.

MS. YANG: Do you want to say anything, Matt? No? Sorry.

MR. EYRE: Sure.

MS. YANG: I don't want to put you on the spot. Sorry.

MR. EYRE: Matt Eyre, Exelon Nuclear.

A number of the I guess PCI suspect failures have been thought of as being due to, let's say, missing pellet surface, might have arisen out of a manufacturing process. And some of our operational guidelines don't account for that.

So when you operate the reactor, do a control rod maneuver, you may overpower that particular place and cause a PCI-like failure. And there you have it.

MEMBER LEITCH: Okay. Thanks, Matt.

MS. YANG: Okay. So that is kind of laying the background. Now I really want to say, what are we focusing on in our program? Basically the way I see our program is I divide it into the four working groups. They address different technical issues. But I group them somewhat differently here in three areas.

The first area I call solving the current problem, solving existing problems. So I list a few. The second area is proactively trying to find out where our margins are. So this is totally reactive. So the second area is more proactive. The third area is just as proactive, but it deals with regulatory issues. So keep that in mind.

That's how I group the three different areas. And the reason I kind of want to highlight it is because I don't want to just read through these words. I am going to inject some slides in between them. So you might feel a little bit not so clear later on.

So existing problems, there are three. And I think in my earlier about what is the industry problem, they are the same issues: failures, fuel failures, impact of NMCA, and AOA.

The failure root cause, I think you have seen some of the charts. We have planned two hot cell examinations later this year. Both of them are to occur before the end of the year. So we are all very nervous to make sure that they do occur. You know, they have to fit in between the utilities' window that they give us and in between the orange alerts. So we are hoping that will occur.

We also are being very active in going to utility sites to assist them as specific failures occur, particularly Bo Cheng there being our expert in that area.

This is often a very challenging situation, as many of you familiar with these types of issues know, because you could spend millions shipping fuel to the hot cell and not being able to identify the failures because not all of them are that easy to identify.

What I have listed here is to show the four hot cells that we have conducted within this program. These are not the only hot cells, but these are the hot cells exams aimed at identifying failure root cause.

I think we have been pretty lucky. So far we have been very successful in identifying the failure root cause. The chart has a lot of information on it. There is a time line on it. There is the different type of failures being listed on when the shipments occur, when the exams occur. I think Bo is going to address, in particular, these things that we already have performed the hot cell that we have identified the root cause. And these two new ones are to occur. The shipment will occur later this year. And hopefully we'll have the results early next year.

CHAIRMAN POWERS: I guess maybe I'm just a little confused. Are you trying to tell me that fuel that you think has a PCI failure gets shipped to KKL or that KKL thinks that all failures are PCI-like?

MS. YANG: Oh, I'm sorry. I see the confusion. No. These two are independent. This we have conducted, actually already conducted a hot cell of PCI-like type of failure of GE barrier fuel. And that occurred in the past. And we have confirmed that is a PCI failure. And in that particular case, it was because of the missing pellets.

You know, if you think about it, you have these cylindrical pellets with the cladding. And if you miss a piece of the surface due to manufacturing chipping or something, then you create a local high stress. This is kind of the opposite of the classic chip situation, but it is the same effect. So that is what we have found here.

Now, we have a new PCI-like failure of a different barrier design. And it's Framatome fuel. We don't know the failure root cause. It could be PCI. It could be any number of other things. We just suspect that it is PCI.

The Browns Ferry is corrosion failure and has been --

CHAIRMAN POWERS: And by "corrosion failure," you mean eaten through the cladding?

MS. YANG: Yes, lots of clad and, you know --

CHAIRMAN POWERS: Well, you seemed to distinguish between corrosion failure and crud-induced failures.

MS. YANG: They are some different nuances. And we're trying to understand what they are. They may be one of the same phenomena, just manifest somewhat differently, or may be different. But we are trying to identify those.

This thing is particularly puzzling because this particular case, as far as we know, pretty much follows all of the BWR water chemistry guidelines.

CHAIRMAN POWERS: We're very proud of that water chemistry there. I'll have to tell them that.

MEMBER FORD: Rosa, you've got four hot cell examinations, presumably all in the United States.

MS. YANG: No. This one is KKL in Europe.

MEMBER FORD: Oh. I rather assumed that since it was a GE fuel that was being examined at Vallecitos.

MS. YANG: No. It's at Europe.

MEMBER FORD: Okay.

MR. OZER: The LaSalle fuel is going to --

MS. YANG: To Studsvik, too, yes.

MEMBER FORD: Because my follow-on question was, what other hot cells are you using? My assumption was wrong. These are not just United States hot cells.

MS. YANG: No, no, no.

MEMBER FORD: If there any work, for instance, in Japan? I noticed you have got a lot of stations in Japan. Are there hot cell facilities being used there?

MS. YANG: They are hot cells. There is a number of very good hot cells. We haven't used the hot cell in Japan, mainly because of the shipment and because of the U.S.-origin materials. But hot cell is an issue.

If you look at the long-term infrastructure for our industry, we don't have a lot of hot cells. And we don't have experienced hot cell staff.

MEMBER LEITCH: In all cases, this work is done with failed pins, not with pins that are similar to failed pins and have the --

MS. YANG: We usually take both. We usually take a sound rod as reference. And if it's PCI, we try to take symmetrical location --

MEMBER LEITCH: I see.

MS. YANG: All of them, we actually usually have done a considerable number of pool-side inspections, discussions trying to look at the manufacturing record, the operation record. And so we don't take things to the hot cell lightly because it is a big expense, on the order of several million.

MEMBER LEITCH: Okay. Thank you.

MS. YANG: And the shipment itself is becoming more and more expensive and takes time. I think you will hear from Kurt later on that it's because of this and in order to improve the fuel performance, we are trying to develop a considerable number of nondestructive inspection techniques to be able to perform at the pool side. So we can reduce our needs for the hot cell.

Noble metal, which is a technology implemented, is a GE-patented technology. It was first implemented in Duane Arnold in 1996. And you can see how quickly the majority of the U.S. fleet has implemented noble metal. The 27 BWRs are mostly in the U.S. I think there are a couple overseas, but the majority of them are in the U.S.

What I want to just highlight, one point, is the utilities facing demands from different sides. For example, the radiation field reduction is a big driver for the utilities, especially from utilities' operation perspective.

So zinc injection up her could be helpful to control the dose rate. So utilities want to inject zinc. And, of course, I think many of you probably know the fuel is the heated surface within the primary system and it has a lot of surface area.

So most of the stuff you inject, be it zinc, be it noble metal, be it whatever you inject, tends to end up on the fuel surfaces. So we need to look at the fuel performance.

Another driving force, other than the radiation field, is the material degradation. This is especially important as we go to plant life extension.

In order to preserve the integrity of the components of the plant, noble metal is required. So quite often our work was needed to address the material degradation issue, to check how effective it is in addressing the plant material issues, the radiation field, and the fuel performance.

So quite often our program worked very closely with the chemistry department and with the material department. And we have this triangle trying to balance all of the issues and jointly develop guidelines to the utilities on the plant chemistry.

MEMBER LEITCH: Pushing in the opposite direction, I guess, is hydrogen water chemistry, is it not? In other words, it tends to elevate the radiation field significantly.

MS. YANG: Yes. But that's why you use noble metal. If you use noble metal, then the amount of hydrogen that's required would be considerably reduced. So that would reduce the radiation field.

MEMBER LEITCH: I see.

MS. YANG: So noble metal tends to have that benefit as well.

Okay. So those are the problem areas. And you will hear a lot about them later on. Now I'm moving into the next area, which as an area I said earlier is proactively trying to ensure margins and under high-duty conditions. I highlighted margin. I highlighted high-duty.

As you will remember earlier, we talk about we operate fuel considerably differently today as comparing to years ago. So we don't really have any hot cell data available of these modern high-duty fuels. And I will show you what they are in a minute.

In order to get a sense of the health, if you may, of these kinds of high-duty fuels, we need to conduct both pool-side and, more importantly, the hot cell examinations. And because of the expense and because of the time and resources required, I think we cannot afford to do a large number of them. So we're very, very careful in choosing what rods to take to the hot cell.

I would call our program probably as minimum in terms of number of hot cell examinations and scope. We try to be as comprehensive as possible, but they are certainly not overly broad. I think we try to be conscious about what we need and just get what we need. Those data are extremely essential.

I said earlier our main purpose is for margins. And the same type of data can also be used for burnup extension. I will show you why. This I think I already pretty much talked about that.

Another thing I may not have mentioned but you probably guessed it already, we want not only to choose the right type of fuel with the high-duty. We try to go to plants that have the type of chemistry, water chemistry conditions that we're interested in or concerned about. And for the PWRs, we try to go to the high-temperature plants. As you can see, what we are trying to do since we are doing the minimum number, we try to look at the bouncing case.

Another fairly busy chart, but we're intending to summarize the number of hot cell exams that we do, in this case, again, the time line in the middle; the BWR, the boiling water reactor, hot cell examinations listed on the bottom.

There are two major ones, both from Limerick. The first one is actually the one at 52 assembly average. It's the same one that Mike Billone talked about yesterday at 56 or 57. He's talking about the rod burnup.

And we just made another shipment of the next higher level burnup. And the highest burnup there is 65. Again, it's the assembly average. We look at different processing material. And also it's the first time we really look at high-duty fuel with noble metal chemical addition. So those are the boiling water reactor ones for the pressurized water reactor.

Again, we already made the shipment of Zircaloy-4 from HB Robinson. And it's the same material that went to Argonne for the LOCA program that Mike talked about yesterday. We made another shipment last year of the ZIRLO cladding at 70,000. Here is why I talk about the same data.

It's the first time we look at ZIRLO under high-duty operating conditions, at least in this country. I think it's probably the highest burnup of ZIRLO ever looked at.

MEMBER LEITCH: So just so I understand, these are not failures? These are high --

MS. YANG: No, no. I want to differentiate. These are the hot cells --

MEMBER LEITCH: High burnup.

MS. YANG: -- to get to high burnup to get a margin. None of them failed.

MEMBER LEITCH: And then what do you do, examine the cladding or the fuel itself?

MS. YANG: We look at the fuel. We look at the cladding. I think I deleted the scope slide. Yes. I deleted it. Well, we mostly look at the fission gas release. We look at the cladding dimensional change as a result of irradiation. We look at the fuel morphology or microstructure change as a result of irradiation. Mainly we look at the cladding mechanical properties, ductilities, corrosions, hydrogen levels, mostly checking how the fuel would behave.

MEMBER LEITCH: Okay.

MS. YANG: And these data are quite useful, of course, to check the health of the material. It could be useful burnup extension licensing. It could be used for some of the RIA tests that we talked about yesterday, provided the mechanical property for the --

MEMBER LEITCH: These exposures are for that particular pin, not the average or --

MS. YANG: Seventy, I believe, is assembly burnup, is it not, Jeff? I mean, Kurt. I'm sorry.

MR. EDSINGER: Yes.

MS. YANG: Yes, assembly. So the pin may be ten percent higher.

MEMBER LEITCH: Yes. Okay. Thank you.

MS. YANG: So, as I said, it's the first time we look at it. We could do it at 62 first, then at 70, but we choose to look at 70. So there are some misunderstandings about this is entirely for burnup extension. It is not. It is the first time we look at ZIRLO.

Same thing here, same story here, a couple of years behind, North Anna. The burnup is about the equivalent. It's the first time we look at M5. For those of you who know, these are the two types of advanced alloys used in this country, ZIRLO and M5. And there are still some utilities using Zircaloy-4.

Here are the five hot cell exams, sound rod for margins. They could be used for burnup extension as well.

Very quickly, just one of the techniques that we developed within this program to sort of as an interim measure solve the AOA and the PWR crud issue is by using ultrasonically to clean the fuel.

The first application is at Callaway, which is the plant most plagued with AOA issues. It was a big success. And they are cleaning real low quantities of fuel right now. And it has since been used at South Texas units 1 and 2 and again fairly successfully.

It was intended to be used later on at Vogtle unit. So this is a technique developed by the program, and it probably will be fairly widely used.

The interesting point here is not only the technique is useful, but the technique actually can be transferred to solve BWR issues. And Kurt will talk to you about how we are right now as we speak qualifying the same technique for the BWRs. And there their interest is for those rate reductions.

MEMBER KRESS: You take these fuel assemblies out of the reactors and sit them on the --

MS. YANG: In the pool.

MEMBER KRESS: In the spent fuel pool?

MS. YANG: In the spent fuel pool.

MEMBER KRESS: And then you have an ultrasonic system?

MS. YANG: To clean it.

MEMBER KRESS: Clean it in the pool itself?

MS. YANG: Yes.

MEMBER KRESS: Where does the crud go?

MS. YANG: It is a closed system. It has its own filtration. And it collects the crud. Do you have charts of those laid out, either of you?

MEMBER KRESS: It's the tank that goes down in the pool and the tank is a closed system?

MS. YANG: Right.

MEMBER KRESS: So you collect the crud in

--

MS. YANG: Yes.

MEMBER KRESS: Okay. That was my concern.

MR. EDSINGER: I do have some slides.

MS. YANG: Okay. The second area for margin, the third area --

MEMBER FORD: Excuse me. Rosa, before you go on to a new topic, just to come back to your slide on the hot cell projects to quantify margins, you made there that this is the first examination of ZIRLO and M5 in this country.

MS. YANG: Hot cell examinations for such high duty and high burnup.

MEMBER FORD: And you also pointed out that there won't be much of this data because of the expense of getting the data. And you also made the case that you want to move into a proactive phase. If you're going to go into a proactive phase, then you'd better have a good model of the various degradation mechanisms. And you use this sparse data presumably to benchmark that model.

Step back one further step. How solid do you feel about the models of material degradation, fuel degradation?

MS. YANG: Just during normal --

MEMBER FORD: Because that is going to be the skeleton upon which you are going to build your proactive management philosophy.

MS. YANG: Peter, you always ask tough questions. I think if I could answer your question a little bit differently, we have a fuel performance code, which is FALCON, which is a steady state and a transient code.

So we use the same code for normal operation to predict the fission gas release, the deformation, and those kinds of things. And we also use the same code for RIA and LOCA. That's a different subject. I think you are talking about the steady state here.

MEMBER FORD: I'm talking about them all, but, yes, take a steady state or transient. It doesn't really matter. You're going to have to have a model, a predictive model, on which you are going to base your proactive management philosophy.

MS. YANG: Yes.

MEMBER FORD: And you are going to use that data to benchmark the model.

MS. YANG: Yes.

MEMBER FORD: My essential question is, how solid do you feel?

MS. YANG: About our model?

MEMBER FORD: About your model because you are not going to have too much data to benchmark it.

MS. YANG: No. In fact, you're quite right. These data, we do look at them and feed back into the model. But I guess I am not quite sure how to address your question properly because the way I see it is for steady state operation, for normal operation, we are looking at margins.

We never look at failures. You never design the fuel for failures. So the code would never predict the rod would have failed. And, of course, the rods do fail.

I am thinking back at some of the failures that we encountered that usually is somewhat of a local phenomenon that the typical fuel performance model cannot last by itself. You always need to run some detailed either neutronics model or thermal hydraulics model or whatever model.

MEMBER FORD: Graham asked a question about PCI, --

MS. YANG: Right.

MEMBER FORD: -- which is an old problem. I suspect that the current PCI problems you talk about are not quite the same mechanism.

MS. YANG: No, no.

MEMBER FORD: The actual splitting is not necessarily what we used to call PCI-before-barrier fuel.

MS. YANG: Right.

MEMBER FORD: So how good do you feel about being able to predict this new PCI problem or being able to predict the potentiality of zinc or NMCA to give you excessive corrosion on BWR fuel or these --

MS. YANG: Our model does address some of the chemistry additive. And I think they are in general good, but the reason, what I am trying to get to is that the reason, you have failure usually is because there is a local perturbation.

So if you input that, you probably will get the answer that you are looking for, but in most cases, I think failure occurs because we haven't tested the condition properly.

MEMBER FORD: Okay.

MS. YANG: So I would like to answer your question slightly differently. We have a document that Kurt will address later on. It's called "technical requirement document," --

MEMBER FORD: Right.

MS. YANG: -- which is almost like a specification that utilities say, "I need the fuel to perform this way" so that it won't fail. So that document was more focused on the type of performance we need and the type of testing that we would require if there is any change in the material. So you can see that document itself would require knowledge and the feedback.

So what we are actually hoping is after we finish these things, in each of the cases, we don't just have a data report, we actually have a report that assesses the margin that's there.

MEMBER FORD: Okay.

MS. YANG: And we are going to take that and feed back into the technical requirement document to say, "Do we have the right criteria? Do we have the right knowledge to say how can we do things differently in the future so we don't get into the same problem that we have today?"

MEMBER FORD: Okay.

MS. YANG: So let me just finish. So my thought is for transient condition, you look at failures because you have to go there in order to see that, but for the steady state, you usually don't design to failure.

So you predict, for example, fission gas release. So we do check if the code will predict fission gas release. In some cases, we're right on; others, we may be not predicting it well. So we have work to do there. We look at corrosion, and we look at the cladding dimensional change.

So maybe I am answering your question. We do look at these. And we check our code to see if we are predicting that. So this is kind of important to benchmark it.

One thing I want to point out is that these data are quite different from the Halden data. They are almost like two different populations. And any code in order to predict the light water reactor has to look at both.

MEMBER FORD: Okay.

MS. YANG: Matt wanted to say something.

MR. EYRE: Matt Eyre, Exelon Nuclear, again.

As Rosa pointed out, these are just basically a noting of the hot cell projects. One of the things that not only does Exelon Nuclear but a number of the other utilities have done or plan to do is also look at their fuel pool-side as well. So it will be a number of benchmarks to the hot cell and also the pool side and a number of different operating environments.

For example, we are going to be looking at our Three Mile Island unit 1 fuel next spring, which is M5. And it has gone to about 70 gigawatt days per metric ton, different operating environment than the North Anna fuel, similar burnup. And so we will be able to compare that with the North Anna fuel. So I will see somewhat sparse data up there.

There is a considerable body of pool-side, nondestructive examination that we will be able to compare with the North Anna work.

MS. YANG: Yes. Thanks, Matt. I think that is a really good point. Maybe in our trying to cover the hot cell, I didn't quite mention the pool-side. In fact, that is the majority of our data. These are just trying to confirm or get additional data that we can't get at the hot cell. But pool side is really important.

MEMBER RANSOM: Has any thought been given to including probablistic effects into your model so that you would predict failure rates? Is that feasible, I guess, or --

MS. YANG: It's feasible. And I remember years ago there was a code, actually, like that. I forgot the name, Entropy, something. I forgot. Thus, I think it's feasible. I'm not quite sure we want to go there because fuel performance, fuel behavior is fairly complex in the --

MEMBER RANSOM: There is a move towards probablistic methods in risk analysis and safety --

MS. YANG: You could. You could. We haven't done that.

MEMBER RANSOM: It would seem like a good way to go.

MS. YANG: We haven't done that.

MEMBER KRESS: It's not particularly a risk issue, I wouldn't think.

MS. YANG: Not particularly.

MEMBER KRESS: I don't know why you would want a probablistic model to predict fuel failure.

MS. YANG: Yes. It's usually pretty low in a 10^{-6}.

MEMBER KRESS: Low in risk base. It's an operational issue, I think.

MS. YANG: Yes. Thank you. I want to emphasize this is not a safety issue. This is a simple economic issue.

MEMBER LEITCH: You talked about ultrasonic cleaning in a couple of the future slides that you already covered. I guess you went back to it.

MS. YANG: Yes.

MEMBER LEITCH: I guess I am wondering. Apparently at Paks in Hungary, they were chemically cleaning, I guess. I don't know all of the details of that, but I guess they had a chemical cleaning process that went amiss.

MS. YANG: Yes.

MEMBER LEITCH: I am just wondering. Is anyone in the United States thinking about chemically cleaning?

MS. YANG: Not that I am aware of. Of course, this technique is totally different.

MEMBER LEITCH: Yes.

MS. YANG: And we won't leave it alone.

DR. MEYER: This is Ralph Meyer at NRC.

I gave a short presentation on this yesterday. I just wanted to make one point. The chemical process was completed and successful. This was just an overnight cooling of a clean fuel problem.

MEMBER KRESS: It was a loss of cooling. And I presume you can have a loss of cooling with these things, too.

MS. YANG: You could except that this is

--

DR. MEYER: I was unaware of that. Thank you.

MS. YANG: This is not just this system. You have to be there. And it takes about one minute or so to clean each assembly. So it is a continuous process. You don't just leave it there.

Okay. I want to talk about a part of our program which deals with regulatory issues very briefly. The uniqueness of our role in this is we are the industry interface with NRC on fuel-related regulatory issues. And they are kind of a two-pronged interaction.

With NRR, we interact through NEI any regulatory and licensing issues. And many of these, it's really industry interaction. It's not just EPRI. It's EPRI representing the utilities and the fuel suppliers. So it's really industry through NEI, interact with NRR.

I think this approach has worked reasonably well on research issues. There is a memorandum of understanding between EPRI and NRC research. And there are many collaborations areas. Fuel is just one of them and I want to say has worked very well. Thank you.

And we have these twice-a-year management types of discussions between EPRI and research, NRC research. And usually we get reasonably positive feedback to and from our management.

And specifically we have this LOCA program that you heard a lot about yesterday. And, of course, there is the Cabri water loop project that we collaborate together.

MEMBER KRESS: Is the iodine spiking phenomena part of this program? Are you looking ahead or has it gone away with the new fuels?

MS. YANG: Iodine spiking?

MEMBER KRESS: Yes, during transients or shutdown.

MS. YANG: During shutdown.

MEMBER KRESS: Yes.

MS. YANG: Yes. It's still there. I don't think we look at it. Maybe I'm not --

MEMBER KRESS: It's not part of your program?

MS. YANG: It's not part of our program, although we do know that if you have failed fuel when you shut down --

MEMBER KRESS: Yes. It seems to be related to failed fuel.

MS. YANG: It is, yes. And we have a handbook that kind of tells a utility what to do in case of fuel failures. And they talk about the iodine spike in there.

CHAIRMAN POWERS: Let me ask one question. Presumably, the tests being done at Argonne are being done to confirm regulatory decisions on the ability of fuels of the current types of satisfy the requirements of 10 CFR 50. And presumably once somebody concludes that question has been answered, NRC's funding for that work will come to an end.

MS. YANG: We hope so.

CHAIRMAN POWERS: What happens then? If the industry is moving to M5 clads, fuels of different types, and things like that, you still have the problem of showing that you comply with the requirements of Appendix K. So, I mean, what happens?

MS. YANG: I think I am going to answer your question. The way we work, for example, through the robust fuel program working group 2 to address your question is we would lay the groundwork, the road map, if you may, how to resolve any regulatory issues, be it RIA or LOCA, by submitting a topical.

Now, if, for example, the topical for RIA that has been submitted is for Zircaloy-4 but if NRC approves that topical, it basically says, "Here is a criteria. Here is what you need to do to show compliance." And although the criteria is Zircaloy-4, it is fairly straightforward for the vendors to take that approach and add to their data for M5 and to produce their criteria. That's one approach. And that's probably a more suitable approach for LOCA than RIA.

For RIA, because the advance alloys perform so much better than the Zircaloy-4 that we have used to develop the data, they could either develop a new criteria or they could say, "Well, this is a conservative criteria for M5. I'm happy with it the way it is. So they can take either approach.

Does that answer your question? We don't take it from the beginning to the end, but I think we do the work that is more efficiently done by the industry.

MEMBER KRESS: It sounds to me like you're presuming the phenomenology remains the same from one fuel type to the next.

MS. YANG: For RIA, I think it is. And I am going to get into a little bit technical discussion because RIA, the cladding force comes from the fuel. And then you just look at the cladding response.

And there are differences between one alloy to the other, but you can address that separately. Some may not be the same. But that's why I said for LOCA, it might be more different. In that case, the cladding may play a different role.

Do, Jerry or Ryan, you want to say anything?

MEMBER FORD: My question was somewhat along the same thought process. Just to clarify that when you are talking about the memorandum of understanding between EPRI and RES on the LOCA tests in ANL, there are no other tests being sponsored primarily by EPRI which address these issues. So those tests we heard about yesterday are the only tests going on in this country.

MS. YANG: I want to --

MEMBER FORD: Let me follow on for a bit.

MS. YANG: Sorry.

MEMBER FORD: We heard yesterday and you heard all the questions we brought up about those tests and the concerns about how would the results change if you had changes in composition of the materials.

The answer I heard you give Dana just now is those specific questions, which relate to different allows, will be the responsibility of the vendor.

MS. YANG: Of the fuel suppliers, yes.

MEMBER FORD: And EPRI would not be funding any outstanding questions associated with that alloy-specific item.

MS. YANG: That's correct. That's correct. Thank you for the clarification. I also --

DR. MEYER: Excuse me. Could I --

MS. YANG: You are probably talking about what you are going to talk about anyway. Let me also differentiate one thing. I guess when I was answering Dana's question, I was more thinking about regulatory approaches. Okay? That was specifically more for the regulatory approaches.

But when, Peter, you are talking about these memorandums of understanding between EPRI and research, we are talking about research programs. We collaborate to do the test, just because the test itself is so hard to do. But we are totally independent in terms of analysis of the test results.

Is that what you wanted to add?

DR. MEYER: This is Ralph Meyer from NRC research.

No. What I wanted to comment on was Dana's original question about the programs for the new alloys. In our recently issued program plan, we have, in fact, planned and budgeted for continuation of the program at Argonne on M5 and ZIRLO alloys.

And, in addition to the LOCA tests, the mechanical property tests, which are applicable to the RIA accident, we also have testing specifically for dry cask storage and transportation of spent fuel.

Now, the big uncertainty is the form and extent of the cooperation between the NRC and industry in the future testing of irradiated M5 and ZIRLO because we do not have that material on hand at the present time.

CHAIRMAN POWERS: Let me ask another question. You have indicated down below at the bottom of that slide that you are a participant in the Cabri program. What do you anticipate getting out of the Cabri program?

MS. YANG: Well, I was one of the persons on record saying we don't need the Cabri water loop project. So personally I don't think we really need more of the failure/no failure type of tests. I am just about to get into the presentation that you will see that come across probably even more clearly.

I think we have a good understanding of the situation. We know how fuels behave under RIA condition. We have a good model. And the model predicts the results. So I don't think we need more of the water loop type of project.

However, if fuel can fail in the water loop, which is kind of a challenge right now, you know, they can't fail the rod in the water loop, if it does fail, then we can look at fuel-coolant interaction. That's the only thing that we may get.

CHAIRMAN POWERS: Yesterday there seemed to be some controversy over the width of the appropriate pulse to use in testing. Were you planning to elaborate on that?

MS. YANG: I'll try. Okay. We have three major regulatory focus areas. RIA, we submitted a topical; -- we talked about that -- a LOCA; and industry guide is for burnup extension. So let me go to RIA, which is most of these are not in your package because I added them last night, be happy to make them available to you, but I believe all of them really came from or most of them -- I'm sorry, not all of them -- most of them came from the presentation when we were here a year ago.

This is what we proposed, two separate curves: one for coolability, one for fuel failure. I want to make a few comments related to yesterday.

And I will try to use some slides to illustrate the comment that I have here.

I think one of the key comments is the fuel behavior under RIA transient; well, fuel behavior in general anyway. And fuel behavior under RIA transient, in particular, is nonlinear, is very nonlinear, and is nonlinear with temperature for the fuel and for the cladding.

We know temperature is an important parameter. And the response is nonlinear with temperature and is nonlinear with another important parameter, which is the hydrogen.

The effect of hydrogen on the cladding ductility, it would strongyl depend on hydrogen concentration but not just concentration by itself. The distribution of the hydrogen is extremely important in cladding ductility. I think there are ample data to support that.

What is very, very clear -- I don't think there is too much ambiguity here -- is spallation. If you do get into spall the situation, the spallation severely degrades cladding ductility. And I will show those in a minute.

The next point, given it is such a nonlinear behavior or phenomenon, you need to model it. And you need a good transient fuel, thermal mechanical model to model the phenomenon. And FALCON is just one of the possible codes. That is our code.

And in our code, we have incorporated the high burnup properties up to the burnup level that we are looking at these phenomena in terms of thermal conductivity as a result of high burnup, the rim effect, and the power depression for high burnup. All of these are extremely important in terms of temperature distribution.

And also, as all of you know, the fuel performance code, I think John showed a very complex chart. It is complex. And you can write some fundamental equations for most of the phenomena.

But at the end of the day, there are these fudge factors that there is no way around it. You have to tune them. And you have to tune them with good, representative data from Halden because that is the only place that you actually measure temperature, you measure the gas release. So you need those. And at the end of the day, we are dealing with light water reactors. So you have to have the light water reactor data.

So you need a good code that has the good model in it, good property in it, and should be benchmarked properly. And another thing is you need a good understanding of the mechanism. And that can only be provided by these RIA simulation tests that Cabri and NSRR and the SPIRT CDC tested earlier.

And there are more than 100 of such elaborate and highly sophisticated tests. Some are more sophisticated than others. There are more than 100 of those available.

I am going to get into you also need a cladding mechanical property because that is the one really responding to the drive. And there are some separate effects tests if you want to answer when the fission gas release occurred and things like that.

What we have done, the industry has submitted last May in a topical report is separate cladding failure and core coolability limit. I think in those cases, we have incorporated the key controlling parameters.

For the failure, we think high corrosion and hydriding are the dominating factors. And I will try to define that. For the coolability, we will look at U02, the incipient melting of U02, as a function of burnup. And we look at the burnup effect as well. We believe those are the controlling parameters.

Both limits are supported by either

integral data or separate effects data, I mean separate effects. And this separation of coolability and failures is consistent with the current regulatory criteria to separate the two.

We don't see any justification for combining the two limit because they are totally different phenomena and by combining them is unnecessarily conservative, is way conservative.

The coolability limit proposed by the industry is conservative by itself because I am going to show you it's a belt-and-suspenders type of approach. And it is supported by data. So those are just outlines of what I would like to show.

The first thing, just to illustrate the nonlinear feature of this phenomena, as you all know, in an RIA transient, the energies are mostly deposited at the rim.

This is the temperature versus the radial position. This is the center of the fuel. This is the pellet rim right here. And the cladding is here.

So let's look at early in the pulse. Let's look at the 87 milliseconds. You see the temperature is very high at the pellet rim. And that is where all the actions occur.

And for this solid line, the cladding

temperature is the solid line. I'm sorry. I should have said in the beginning, this is the cladding temperature, is black. And if the pulse goes into the black, the cladding temperature goes like that. And at the end --

CHAIRMAN POWERS: That was too remarkable for me. You put a pulse in. And so the temperature and the clad go down?

MR. OZER: No, no. The temperature and the cladding are way at the bottom, way down, way down.

MS. YANG: Yes. Sorry. Here, this one. This is the beginning-of-life temperature. This is the end-of-life temperature. Sorry. This is beginning. This is the end.

This is 87 milliseconds. As you go to the end of the pulse, the temperature, the heat is beginning to conduct through the center of the pellet.

So the pulse, the temperature shape kind of smoothes out, becoming this curve. And the cladding temperature increased from this solid line to that line. And, of course, after a long time, the temperature goes to two seconds later, goes through your typical parabolic distribution.

So the nonlinearness is the temperature

keeps changing as you go through the pulse. And it progressively moves to the center of the pellets. At the same time, the cladding temperature increased.

CHAIRMAN POWERS: I guess you just totally confused me.

MS. YANG: Okay.

CHAIRMAN POWERS: The slide says we have a 9.5-millisecond pulse.

MS. YANG: Right.

CHAIRMAN POWERS: Okay. So now you are looking at 87 milliseconds. That must surely be a long time after the pulse. So we're not going through the pulse. We're just enjoying the response of the system to the pulse.

MS. YANG: Say that again.

CHAIRMAN POWERS: We're not going through the pulse. We're sitting here looking at the aftermath of the pulse.

MS. YANG: The temperature response in the fuel has a delay from the pulse. We have a temperature chart of the energy, of the pulse and the energy. I will find it for you later at the break.

MR. OZER: Rosa, the origin of the pulse is not at zero. The pulse starts later on. Early in the pulse, 74.5 milliseconds is only the beginning of the pulse. At the peak of the pulse, you are at 87 milliseconds.

MS. YANG: This one?

MR. OZER: Yes. So the counter starts before the pulse. And then the pulse is introduced around 70 or so. And 87 is the peak of the pulse. And then it starts to decrease after that.

CHAIRMAN POWERS: That is certainly not --

MS. YANG: No, no. There is the energy input. There is a delay in the pulse. You don't heat up the fuel, by the way. I'll show you. I think, in fact, we have a nice energy input versus when you cross --

CHAIRMAN POWERS: I can safely say I am totally confused, but --

MS. YANG: All right. The timing, we can clarify that, but the key point in trying to make assuming if you will just follow the zero time here, this is at 87 milliseconds, then the pulse keeps going inside.

MEMBER SIEBER: It would be interesting to show a plot --

MS. YANG: I will show you. It's on the computer.

MEMBER SIEBER: -- of the energy creation especially --

MS. YANG: Yes, exactly.

MEMBER SIEBER: -- and then look at that versus what the temperature results.

MS. YANG: Versus temperature, yes.

MEMBER SIEBER: Then I think it becomes clear.

MS. YANG: I can see why you are confused, but let's forget about the time zero. We will define that later. In an RIA pulse, you deposit energy at the rim.

MEMBER SIEBER: Right.

MS. YANG: So this is the temperature distribution at some point. And as time goes along, this temperature shape widens and eventually goes to the parabolic.

CHAIRMAN POWERS: I assume that all you're telling me is that there is conduction.

MEMBER SIEBER: Yes.

MS. YANG: There is conduction, right.

CHAIRMAN POWERS: Now, conduction is something I believe in.

MS. YANG: And that conduction --

CHAIRMAN POWERS: What that has to do with 9.5-millisecond pulses versus 30-millisecond pulses, I'm not entirely certain about.

MS. YANG: Okay.

CHAIRMAN POWERS: I have no idea what this slide is trying to tell me other than that there assuredly must be conduction.

MS. YANG: Okay. There are two points on the slides I want to make, and maybe I'm not making it clearly. The first one is the most of the energy is at the edge. And the narrower the pulse, the narrower --

MEMBER SIEBER: The sharper the peaks.

MS. YANG: The sharper. This is the peak. And that affects the temperature in the cladding, which changes with time. So that's one of the first nonlinear situations.

Now I want to go into the cladding failure because that's one of the criteria we propose. Then I will go to core coolability. Most of the simulation, RIA simulation, tests deal with cladding failure.

There are over 100 of them. And, just by studying them, I think this is kind of the conclusion. There are two different phenomena for high burnup and for low burnup.

For the lower burnup ones, it's mostly a high-temperature failure. It's called post-DNB operation. And we won't discuss too much of that here because most of what we are concerned about is high burnup. And for beyond 30 or 40 thousand, the failure is by pellet-cladding mechanical interaction. And that can be modeled very successfully.

So far the experiment has not produced any data that suggests otherwise. So pellet cladding mechanical interaction is the failure mechanism. And the driving force for that is from the fuel thermal expansion and the fuel extension as a result of fission gas swelling.

For those of you who I think are less familiar with pellet-cladding mechanical interaction, the cladding ductility is the key parameter in determining if the test failed or not.

This is also the conclusion of the PWR RIA PIRT that Ralph chaired, which says cladding ductility is a key parameter. Now, as you remember, now we're looking at the current regulatory criteria at high burnup.

So this point is trying to say burnup per se is not an important parameter, but the result of the burnup, the cladding corrosion and hydriding as a result of irradiation and burnup is really the key.

You could have very low burnup, a very high corrosion, and you could fail. The cladding could become very brittle or, conversely, you can have very, very high burnup but very good cladding, has very low corrosion. The cladding would have survived. And many of the Cabri tests have demonstrated that.

MEMBER SIEBER: But the fact is that it does become less ductile as burnup increases absent the corrosion effect, right?

MS. YANG: You mean the irradiation damage?

MEMBER SIEBER: Yes.

MS. YANG: Yes. And that doesn't seem to be as important as the hydrogen in the cladding. In the Cabri test, they have conducted two very high burnup rods with advance alloy and with very low corrosion levels, like 20 microns or so. And they have survived the highest energy input at 70,000 burnup.

So that is a good demonstration, if you may, that the hydrogen, when you have low hydrogen, even with the irradiation damage, it could survive this type of transient.

MEMBER SIEBER: And zinc makes the hydrogen low?

MS. YANG: Zinc?

MEMBER SIEBER: Yes. What chemical addition can you reduce the hydrogen from? Any?

MS. YANG: I don't know.

MR. BILLONE: I'm sorry. What was the question?

MS. YANG: What chemical in the coolant, addition in the coolant, would make hydrogen low --

MEMBER SIEBER: Lower.

MS. YANG: -- lower in the cladding?

MR. BILLONE: I don't think you can do it that way because basically you have water. And water is going to break up. And some of that hydrogen is going to go through the oxide, which is not protective, and will get to the cladding bare metal. It happens locally.

MS. YANG: In this particular advanced cladding, just corrosion is very low. And this hydrogen was also very low. If the rod is spalled, as I said, once a spalled, it severely degrades its ductility.

In the Cabri database, there are no failures up to 64 and now up to 70 thousand for both Zircaloy-4 and advanced alloy. As long as it doesn't spall, you can have fairly high corrosion, up to 80, 90, 100 microns. But there are no failures if the rod did not spall. So spallation does create a special challenge to the ductility.

In my slides, you are going to see I sort of favor the Cabri data in the sodium loop. We only have two new data points in the water loop. And one is not even quite confirmed.

So all the data I'm talking about here are in the old program in the sodium loop. And the reason I favor that I think is the same reason that Ralph Meyer talked about yesterday. These are the most representative simulation tests. They have the right temperature and the right pulse.

CHAIRMAN POWERS: I thought one of Ralph's major points was that the temperature was a bit high.

MS. YANG: For this?

CHAIRMAN POWERS: For the Cabri data. I mean, he went through the whole exercise with us on REP-Na10. And he made an adjustment for the higher temperature.

DR. MEYER: Yes. This is Ralph Meyer. The temperature was high as a consequence of the pulse width. The initial test temperature is the right -- it's the same as the hot standby temperature.

MS. YANG: So you are adjusting the --

DR. MEYER: The high temperature that I was talking about, Dana, was a consequence of the artificially broadened pulses that they're using. And these are the temperatures of the cladding at approximately the time that the failure took place.

MS. YANG: So you are really adjusting the pulse widths?

DR. MEYER: Yes. For Cabri, the adjustment was just for pulse width.

MS. YANG: For the pulse width.

DR. MEYER: For the NSRR data, the adjustment was for both pulse width and test temperature, the initial temperature of the test in --

MS. YANG: In that case, the temperature is very low. The pulse width is very narrow. So you adjust upwards both cases?

DR. MEYER: Yes.

MS. YANG: Mr. Chairman, you talked yesterday about something new in the topical. I assume you are referring to the failure model talked about here. This strain energy density approach has been used elsewhere. This is not something really invented by Anatech. What it does is it really just integrates under the stress and strain curve that you get from the mechanical property measurement.

What is unique, I would say, is the CSED, which defines the failure. You know, you look at the stress-strain curve. And you look at the total strain here. And then you integrate between the stress-strain curve. And that is the failure CSED.

The reason we take this approach is because it takes into consideration the strain rate, the temperature, and the stress biaxiality effect. I'm not the expert on this.

So I can't really defend this too much. But I think we need this because if you want to take a mechanistic approach of the RIA, instead of just looking at the data empirically, you need something as a gauge to say what is the measure of the adverse effect on the rod so that it fails or not fails. And this is our gauge.

You can look a number of cases. You can use strain. You can use stress. But we decided to integrate under the stress-strain curve as our gauge for failure.

So when I look at the response of any RIA simulation tests, I can calculate the SED for that particular test and then depending upon if it's M5, if it's ZIRLO, if it's Zircaloy-4 and how much hydrogen in the cladding, then I create a CSED curve, which is clearly mechanical property. There's nothing more than mechanical property. And if the mechanical property is good enough, then the rod will not fail. And if the mechanical property is not good enough, then the rod will fail.

So it is a concept of taking advantages of the mechanical property data. And I think because of this, you can actually -- I think Tom yesterday talked about extrapolating into advanced alloys. Because of this way of making use of the basic mechanical property data, you can extrapolate it or extend it to advanced alloys.

Now, one of the concerns or the criticisms is how good are the data. And here is our database. There are hundreds of data. They are not all created equal. They are burst tests, axial tension tests, ring tension tests. And there are different ways of doing ring tension tests. I would argue that axial tension test is probably not that relevant because we are looking at the hoop direction here.

I would also argue some of these very early or very old data are probably not as good as some of those on the lower end curve. But you can see the database is pretty extensive. They cover different fuel type, different burnup level, fluence, oxide, temperature, strain rate. The fact it is very comprehensive probably resulted in the scatter of the data. And here is the data. There is considerable scatter. I would agree.

I just want to use this curve to make a few points. A) do we want better data? Yes. You can always get better data. And we have many programs in place. The one at Argonne is one and some of our own programs.

We can always refine it, but the one thing I want to point out here is different shapes. Well, some of the scatter is due to temperature. As you know, temperature is an important parameter. And what I want to point out is these solid symbols. They are from the material that has spalled. The open symbols are, at least from the metallography, that they are not spalled. And this blue line is the best estimate fit of all the data. And this is the best estimate of all the spalled.

So I think it is clear that they are two different populations.

CHAIRMAN POWERS: I'm tempted to quibble --

MS. YANG: With that?

CHAIRMAN POWERS: With a lot of stuff

about this plot. For instance, let us take your spalled solid data point out of the plot. And let's look at the residual open spot. Why wouldn't I fit that with a constant?

MS. YANG: Well, one of the reasons we don't want to fit it with the constant, Dana, is because we do know there is a relationship here. What you are plotting is the strain energy density as a function of oxide to cladding thickness ratio. It's a flange parameter, but we use it. You can just look at it as oxide. The reason we divided by the cladding thickness is because, if you remember, our data had 14 by 14, 15 by 15, 16 by 16, 17 by 17. So this is just a way to normalize the data point.

Basically, you are saying that the critical strain energy density is reducing as you increase the hydrogen content. I think it does have a slope to it, especially if you look at individual data sets.

CHAIRMAN POWERS: You can have a religious belief there is a slope there, but I fail to see it in the data.

MS. YANG: One of the scatters, the reason is because we plot all the data that may not belong to the same curve. But if you look at individual sets, like this ring tension test, it does have a slope to it.

And if you look at burst tests, which is typically believed to be more demanding than an RIA or PCMI type of test. It has a very sharp slope to it, which is consistent with the mechanistic understanding of the mechanical properties.

I do agree as we improve, I think for many of the people here, they know that there are challenges for these either burst tests or the ring tension tests. You basically get different results if you do ring tension tests or burst tests. And depending on the gauge lines, depending upon how much friction you have and all of that, you get scatter in the data. And they can be improved, and they are being improved.

What we have done here -- and I will show you what we have done -- is we look at different ways of using the best estimate. We think for RIA types of transient, it is not a good idea to just take the lower bound given the type. Being risk-informed approach, you should look at the best estimate but trying to quantify the uncertainties or the scatter in the data. And we have done that in the report.

CHAIRMAN POWERS: It's a very peculiar

characterization of the uncertainty in that report. If you were to put the confidence bounds around that line, about where would they show up? You can take any percentage confidence you want, 50 percent, 80 percent, 90 percent.

MS. YANG: Yes.

CHAIRMAN POWERS: Where would they show up on that curve?

MS. YANG: On the curve?

CHAIRMAN POWERS: Yes.

MS. YANG: I'll show you. What we did, this is the best estimate curve. Then we said we will only look at the burst data. We will look at a different set. We said we will only look at the burst data, which is the green curve. Then we said we will look at the lower bound of all the data, which is a red curve. So you do get different curves.

The bottom line, these are the three curves. I'm sorry. The red become black here. These are the three curves. If we take the lower bound of all the mechanical property data we have, we will have predicted those tests failed and they didn't. And so we decided consistent with the risk involved, we should take a best estimate approach.

But if you really want to, if you take a list of the burst tests, which we know is more conservative than the PCMI type of scores, if you take this, you still would show this rod fail.

So we kind of lack data. We don't have a good justification. The right curve to be totally probably should be something like that, but we don't really have a good basis to draw a line here.

And then in the report, we did estimates of taking any of the curves. How does that affect the criteria that we have proposed? And we quantify that in the report. The bottom line is very, very small, basically because these curves are so flat and we use a very conservative corrosion correlation, just to refresh your memory, because these curves are basically flat here.

So if we take the lowest, lowest bound, we probably end up with a curve like that. So it is going to be lower than what we proposed but not by very much.

So basically when everything is sort of put together in terms of the failure curve, it is not that different from what we proposed. And these are discussed in great detail in the topical report.

So yes, I agree there is considerable scatter in the data. And we have looked at ways to quantify the scatter and the uncertainty. And we have quantified that. And the bottom line is that doesn't really change the final criteria.

CHAIRMAN POWERS: If you model that database as a constant, which it certainly looks like, then it says there's no dependence on strain energy density and it's a failed concept. That's the problem.

You have said, "Look, this thing depends on strain energy density." You draw a curve through it to get yourself a slope. You show how it depends, but you may be fooling yourself because the data is so scattered there may be no dependence on strain energy density. It may have nothing to do with it.

MS. YANG: If that's the case, then this curve you could plot. I don't think strain energy density is very unique here. You can plot the total elongation here, for example.

CHAIRMAN POWERS: I can plot the tides of the moon on this plot and do a calculation and get a curve through it. It doesn't mean there is one. You see, the data is so scattered that it is very, very difficult to convince yourself that the slope you have there is distinguishable from zero.

MR. CARUSO: Why is it a curve? Why isn't it a straight line?

MS. YANG: It's a best fit curve.

MR. CARUSO: But when you do a fit, you have to fit to an equation and decide what the shape of the data is. Is it linear or it a quadratic or is it a cubic? How did you decide that it was that shape?

MS. YANG: It's a statistical fit of the data. I mean, that's the shape that comes out of the fit.

MR. CARUSO: To what? I mean, you have to decide that it varies as the square or as the log or as the? Where does that come from? If you do a fit to a particular type of curve --

MS. YANG: The fitting routine gives you what is the best fit form.

MEMBER SIEBER: If you do a multiple regression, it will come out either a straight line or some kind of a curve.

CHAIRMAN POWERS: If you do a multiple regression in any real program, it will also tell you whether the coefficients are distinguishable from zero or not.

MEMBER SIEBER: Right.

CHAIRMAN POWERS: And here I just can't

imagine it would come back with any of the terms having a coefficient that was distinguishable from zero.

MS. YANG: Oh, I think it does.

MEMBER SIEBER: You may be right.

MS. YANG: Dana, one other thing I want to point out is probably our fault. We plot all of the data we have just because they're in that not damaged mechanical property data. But what I would argue is some of these axial tension tests are not that relevant for the PCMI type of driving force that we are talking about here.

So if you take the blue square and the green or the upside-down triangle, which is a lot of the scatter right here, I think that the slope you are looking for may be more clear.

MEMBER SIEBER: I think you are making your argument based on what you are observing there, but it would be interesting just to take the sets of data and plot the curves and see what they actually do look like, rather than have a single curve --

MS. YANG: Yes.

MEMBER SIEBER: -- trying to represent all of the data.

MS. YANG: And I believe they are in the report, the different sets. Like I said --

MEMBER SIEBER: Well, I don't know that.

MS. YANG: If you look at different sets, I think the dependence is more clear. It's just when you overlay the different sets of data, they appear to be more scattered.

CHAIRMAN POWERS: Now, when I look at the individual sets, what I see is a high degree of sensitivity to individual data points, that, in fact, if I take one data point out of each set away and say there was something wrong with that data, I end up with curves having zero slope.

MS. YANG: I'm not sure I understand. Say it again.

CHAIRMAN POWERS: Well, what I understand is we are going to run out of time on this session. So let us wrap up as quickly as you can.

MS. YANG: Okay. This is what was proposed. And these are the REP-Na tests, which have the right temperature condition and with different pulse widths. You can see the curve bounded the data quite well, even maybe somewhat more conservative for that one.

Coolability. We have shown here is all of the data, the RIA simulation data, which had fuel dispersal, some dispersal. And what we have plotted here is the amount of energy deposition after the failure as a function of the pulse width.

All of those on this side of the curve have fuel dispersal. All of those on the other side of the curve have failed. And some of them have considerable energy deposition after they fail, but they are no fuel dispersals.

So this is an empirical approach. And we have drawn a line here, and the line actually should be drawn between these two sets. Basically we observe you only have fuel dispersal when the pulse width is less than 10 milliseconds.

There is a chart which in the interest of time I don't want to get into that explains why the narrow pulse would have resulted in dispersal because the temperature distributions are considerably different for the narrow pulse and the wider pulse. So because we think the typical PWRs or BWRs that the pulse width is greater than 30 milliseconds.

So it is not possible to expect fuel dispersal with the prototypical pulse width. However, if you want to assume dispersal in case you have a very high energy input, then we look at the data in the industry that shows how that affects the rod geometry and how the fuel and coolant would interact. And there are these references.

But the bottom line is really only molten fuel would result in high energy fuel-coolant interaction, as illustrated in this experiment here. Is the measured energy conversion ratio as a function of the mean diameter of the particle?

The molten for the molten fuel dispersed that the energy conversion is high. But for the same diameter that if you don't have molten fuel, the conversion ratio is significantly reduced because of it.

So what we had proposed here is to limit fuel melting. We want to limit any amount of fuel melting, even if it's incipient melting, just local melting.

If you recall, the temperature distribution for the RIA pulse is peaked at the rim, which happened to be the highest burnup area, which they are fuel particles there due to the so-called rim effect.

So what we have done is to propose the criteria that limit any amount of fuel melting right here. We have used very conservative thermal conductivity data.

We have used fuel melting as a function of burnup. So we didn't just use the fuel melting temperature for fresh fuel. We have used a correlation from experimental data that reduced the fuel melting temperature as a function of burnup. So we used that to develop the fuel coolability curve. That's why you see this going down as you go to higher burnup. And they are these data, these Japanese data, that we have found that sort of support the curve that we propose.

So, to summarize, we have proposed a fuel failure limit, which is based on PCMI and the mechanical property. And we have proposed a coolability limit, which is based on preventing fuel melting. And both are supported, which we're using an analytical approach, mechanistic approach, trying to represent the phenomena and take the lower bound of many of the values that we have used, especially corrosion, which we use the bounding case of corrosion. So this is a curve that was produced and supported by the Cabri experimental data for the coolability.

We don't think there will be fuel dispersal if you have representative pulse width. However, if you do want to calculate dispersal, we have limited the fuel temperature to melting because of this more mechanistic type of approach that can be extended to advanced alloys.

So that I think, Dana, you asked the question yesterday. When you extend the methodology to advanced alloys, do you still need it to run any RIA tests? And I think the answer is yes, but you don't need as many so that you can get a good statistical distribution.

What you need are one or two of these RIA types of tests to confirm what you know, but what you need once you develop a new criteria for advanced alloys, you need good mechanical property data for the advanced alloy because we have found from the RIA simulation tests up to 73,000 burnup, which is the licensing burnup extension limit that we are looking for that PCMI is still the driving force. There are some mechanisms being hypothesized earlier, but we haven't observed that at this burnup level.

So we think what we proposed is conservative and considering the risk of the rod ejection accident, we think there is no basis to merge the two curves because in one case, you have cladding failure. And in many cases, they are the PCMI types of cracking. They certainly were not the same as causing a core coolability concern. So that's the added slides for the RIA.

For the LOCA program, coming back, closing to the end of my presentation on the LOCA, I am not going to talk about the Argonne program, which was being talked a lot yesterday. In addition to that program, we have a collaborative program with EdF, which looked at the separate effects of the LOCA face, which is quite different from some of the other experiments.

Basically you look at the ballooning and the burst phase. And then you look at the oxidation and the quenching phase. Just look at the cladding. You use fresh cladding, and you use hydrided cladding to simulate the hydrogen.

The intent here is assuming that the irradiation effect will be annealed out for the LOCA type of condition. Of course, that needs to be demonstrated to be the case. And I think the Argonne program will provide us some insight to that. And some of these French tests will provide that.

We are also monitoring the Japanese ALPS program and the Halden program, which is using real fuel rods to run LOCA tests in a test reactor. The purpose of this slide is just to let you know that we are actually approaching LOCA very similar to what we are approaching RIA.

We are actually going to model all the experimental results so that we have a better understanding of how much margins we have. I think the stylized LOCA phrase was used. And we need to know how much margin we have between the real LOCA, real hypothetical LOCA, and the stylized LOCA.

Very quickly, we have an effort for industry guide for burnup extension. This is to sort of lay out the framework or the road map for getting to burnup extension. This is being mandated by NRC to, instead of different fuel suppliers coming in for different topicals that the NRC mandate to save the resources to be more efficient use of the resources, we should do it on an industry-wide basis. And the target of this effort is 75,000 for PWRs and 70,000 for BWRs.

What we have done is we have looked at the current regulatory criteria described in SRP 4.2 and trying to look at each criteria to see how the burnup impacts that particular requirement and then decide on if new criteria should be proposed.

We have a fairly rigorous approach in this and have been discussing with NRR in the past. We're continuing with this. We are pretty much done on this except the LOCA section. I think you can understand why, but we'll complete it and submit it to NRC for review sometime next year.

So, to summarize the program, as I said earlier, we have many international participants. And they play a very active role. They don't just come to get data in many cases. They provide the data and the operating experience to the program. And we have addressed many of the key issues, like AOA and noble metal. And we have completed the RIA topical report. We are in the midst of hot cell examinations, trying to obtain data to ensure margins to avoid big surprises.

So a little bit of the PR here. We are aimed at reliable and efficient fuel operations. And we have industry-wide collaboration, trying to resolve safety and regulatory issues in an integrated and effective manner. Although I said it's PR, that is certainly what we are striving at and just to show some of the interactions and interfaces that I have talked about before.

So that's the end.

CHAIRMAN POWERS: Any questions of the speaker?

(No response.)

CHAIRMAN POWERS: Seeing none, thank you very much.

MS. YANG: Okay.

CHAIRMAN POWERS: We will recess until 10 of the hour.

(Whereupon, the foregoing matter went off the record at 10:38 a.m. and went back on the record at 11:00 a.m.)

CHAIRMAN POWERS: Let's get back to work here before people try to chase us out of the room. They won't succeed. We're comfortable here. We're enjoying this. This is a salubrious environment.

Let's get back to talking about -- what were we talking about now? Oh, LOCA tests. Odelli, it's yours.

MR. OZER: Thank you. Thank you, Mr. Chairman.

As Rosa mentioned, our robust fuel program is a collaborative program. We work very closely with our sponsoring utilities and the fuel vendors. This collaboration is probably the strongest in working group 2. We work very closely with our vendors.

And at the last working group 2 meeting, we had a considerable amount of discussion of the ANL

LOCA program. There was a considerable amount of concerns that were expressed. And the decision was made to express those concerns, to transmit them to NRC in the form of a letter. So it's the basis of this letter, the contents of this letter, that I was asked to talk about.

I think they concerned the tests that are being done to confirm the LOCA criteria. I think yesterday we got a very good presentation from Ralph, Ralph Meyer, on the basis of the LOCA criteria. They were established for the 1973 ECCS rulemaking hearings. At that time, there was very little data that was available on the types of forces that fuel would experience during a LOCA event.

So ductility was considered to be sort of a proxy, if you wish, a surrogate, to ensure adequacy of the fuel. And two limits were established. There was generally good agreement among the industrial participants that 17 percent was a good oxidation limit. And the 2,200 temperature limit was established to a certain extent to stay away from the region where reactions become autocatalytic.

Also, there was some evidence that oxidation at higher temperatures was more harmful than oxidation at the lower temperatures. So those were really the bases of the criteria as far as we're concerned.

Now, as I said, the 73 criteria were based on very limited information. In the ten years that followed, there was a considerable amount of information, experiments that were conducted by the commission's own assessment.

$1.5 billion were expended on trying to understand the behavior of reactors during a LOCA event. Seven hundred million dollars -- this is 1980 dollars -- were NRC's own LOCA programs. The remaining half was DOE and international participants in the industry in general.

As far as the two licensing criteria that we are most concerned about, the most relevant ones we feel are the experiments conducted at Argonne by Chung and Kassner consisting primarily of subjecting rods to a LOCA-like scenario, quenching them. And rods that survived the quench were subjected to impact tests with 0.3 joule hammers. And then it was determined whether they survived that or not.

Now, following these tests, in the mid 1980s, the NRC staff reviewed the large amount of data that was generated. They concluded, first of all, that now there was a sufficient amount of data to estimate the extent of conservatism in the criteria that were established in 1973. This is summarized in NUREG-1230.

Following the review, the 10 CFR 46 was revised, 10 CFR 50.46 was revised, based on NUREG-1230. The criteria were left the same at 2,200 and 17 percent. However, the commission allowed the use of best estimate models as long as the uncertainties involved in the overall modeling process were accounted for appropriately. This allowed the use of corrosion correlations that are more relevant, such as Cathcart-Pawel, as opposed to Baker-Just.

At the same time, the commission felt that there was sufficient conservatism in these criteria. And they de-emphasized at that point the use of ring compression tests or ductility in general in favor of the impact tests. The impact tests provide sufficient margin.

Here we are plotting all of the impact tests that failed, but we are plotting on the ordinance. The x-axis is the inverse of the temperature. So this is low temperature. As the temperature goes higher and higher, we progress in this direction. And we come to the limit, 2,200 Fahrenheit or 1,200 degrees Centigrade.

I am only plotting here the rods that failed and the level at which they failed. The y-axis represents the time that each rod spent in the oxidizing environment, the amount of oxidation, if you wish, different temperatures. So at this temperature, it would take this much time to oxidize the rods to a certain level.

Now, where does the 17 percent stand with regards to these failure levels? If you use the original Baker-Just correlation, you get this line. This is the 17 percent line. And there was a considerable amount of discussion about Baker-Just versus Cathcart-Pawel.

This is the Cathcart-Pawel oxidation limit. Yes, it decreases somewhat the margin, but still there is a considerable amount of margin.

Now, what I should stress is that some of these rods would have failed the ring compression tests. Depending on where you take your sample for ring compression, if you take it from the hydrided rim, they probably could have failed. Nevertheless, the NRC felt that this was sufficient evidence of conservatism and went on.

Now, we feel that the current regulations are based on the 1988 assessment, which relied on quench and impact tests. We feel that impact tests on the irradiated fuel are necessary to provide this linkage to the 1988 reference.

What we are trying to establish is, first of all, this reference between irradiated versus unirradiated impact tests. Eventually what we would like, of course -- I think this is consistent with what ANL and Ralph would like to do -- is to determine whether we can establish a correlation between the irradiated material response and the response of unirradiated but pre-hydrided material so that we can estimate the response of materials sooner without having to go through the post-irradiation process.

We feel that impact tests are more representative of expected post-LOCA loads. We feel that reliance on ring compression tests is problematic.

Mainly the results are subject to interpretation. They're qualitative. There is no clear way to relate the ring compression tests to the type of data that was used in 1988; in other words, the impact tests.

We acknowledge that yes, they have some limited use and they were used recently to inter-compare different materials to the response, to compare the response of the advanced cladding, for example, to that of Zircaloy-4 and to compare, for example, E110, confirm that the E110 behavior is there. But that's a rather limited use.

We are very concerned about the uncertainties that will be introduced by defueling of high-burnup fuel rings for doing these tests. The response of fuel in a post-LOCA seismic event will be to a great extent controlled by the rigidity of the rods. And the rigidity will be affected by the fuel that is inside those rods.

Yesterday Mike Billone showed dramatically different responses when you leave rather rigid pellets inside a four-point bend test. Now, if we are doing these tests on defueled samples, the impact of this effect will be lost. We will not know how to use it, really.

And we think that it has questionable relevance to post-LOCA load. For that reason, we are proposing to hold a stakeholders' meeting to ensure that appropriate input is considered. This would be a meeting of experts in mechanical properties and testing and so on, which I'm not.

So this is really a very brief summary of why we submitted that letter and what we are concerned with.

MR. BILLONE: Mike Billone from Argonne. Just for clarification, in Hee Chung's work, they did a lot of impact tests. They did a lot axial tensile tests. And they did a lot of ring compression tests. The research included all the three testing types. It wasn't just impact tests.

MR. OZER: That's correct. Nevertheless, I mean, some of those ring compression tests, as I said, would have shown not enough ductility or would have shown that material would be failing around 17 percent of the ring was taken from the hydrided rim region.

MR. BILLONE: Right. Just for clarification, because I have to translate this into a testing program in a way, there are different ways of running impact tests. And I'm wondering if you are asking for a screening test, a .3 joules screening test, or whether you do the traditional impact where you impact the failure and you look at energy absorbed in the failure.

MR. OZER: I think this would be best left for the stakeholders' meeting. But as a minimum, I think we would like to see the 0.3 joules test to provide the reference to what was done in the 1980s.

Again, that is a subject for debating.

MEMBER FORD: Would you mind putting up your graph, please? Just for my information, the data points which were done under thermal shock conditions or high strain rate conditions, what was the burnup, the highest burnup?

MR. OZER: They were zero burnup.

MEMBER FORD: Zero burnup?

MR. OZER: Zero or very low burnup.

MEMBER FORD: So you would expect those lines to move down towards the current mix with burnup?

MR. OZER: Yes.

MEMBER FORD: The question is how far the --

MR. OZER: The question is how much.

MEMBER FORD: How much?

MR. OZER: Yes, exactly.

MEMBER FORD: Okay.

DR. MEYER: Mr. Chairman, may I make a comment?

CHAIRMAN POWERS: Sure.

DR. MEYER: This is Ralph Meyer from research. We, in fact, have responded to the letter. We are happy to arrange the stakeholders' meeting and to discuss this possibility.

I do want to comment on the impact test in the light that I see them. Up until the time of the impact tests, which were done around 1980, both at Argonne and at the JAERI Laboratory at Tokai -- well, they didn't do impact tests, but both of them were exploring the mechanical behavior of a balloon specimen.

At the time the rule was made, the data, the mechanical data, ring compression testing, was all done on under-formed specimens. And an algorithm was cooked up so that you would apply that information to the ballooned region.

So the impact testing that was done at Argonne was some of the first testing that actually showed that the recipe worked because the intention was to have some ductility left after you survived quench.

And so the impact testing then showed that you had at least some strength left after you survived the quench. So I don't see any conflict between the impact testing and the ring testing.

In the current program, we have plans for four-point bend tests, rather than impact tests. We considered impact tests a couple of years ago, and we will reconsider impact tests in discussing this with stakeholders. But the current plans were based on doing four-point bend tests to explore the integral effect with fuel inside on the ballooned region.

So I think the current test plan is that it does cover the difficult areas, all of the difficult areas that Odelli mentioned, using the four-point bend tests. And we can explore the possibility of adding impact tests in the stakeholders' meeting.

CHAIRMAN POWERS: Maybe for my own edification, suppose one comes back and says, "Gee, this particular device survived one magnitude of tests and didn't survive the next one." What do I do with that data?

DR. MEYER: Could you repeat the question?

CHAIRMAN POWERS: Okay. Well, let's just take one from the plot up here. It says here is one that survived the .15 joules impact but failed under a .3 joules impact. Now, what do I conclude from that? As long as I have guys with real tiny hammers inside the core, it's okay?

MR. BILLONE: To get out of that, you would run the impact test of all of the failure, which is traditional. And then you would get an absorbed energy associated with that failure. And then you generated a database that is not arbitrary.

You have a screening test. You are always going to be criticized by what you pick, .03, .15. And the point is that with those, your material could be really brittle. And it can survive a low-impact test.

This is out of my league. A lot depends on how much confidence you have in the loads you are calculating in reactor by picking a screening test.

CHAIRMAN POWERS: And I guess I agreed with Ralph when he said yesterday that they lacked confidence in their ability to predict the loads under LOCA conditions, but maybe I'm wrong about that.

MR. BILLONE: That one is out of my league.

MR. OZER: The loads during the LOCA or after the seismic loads that would occur?

CHAIRMAN POWERS: I guess the answer is yes to that question.

MR. OZER: Both? Okay. The loads during a LOCA, the ability to survive the loads during a LOCA, is demonstrated by surviving the quench thermal shock. If a rod survives a quench thermal shock, then it survives it. That's proof.

MR. BILLONE: No. But I think the point was there might be additional loads beyond that. That was the issue, are there additional loads beyond just simple thermal stress in the cladding? I thought that was the issue of the unknown or the gray area.

DR. MEYER: Well, in addition to that is the uncertainty as to whether the test where you are doing the thermal quench is adequately representing the constraints that would exist in the fuel assembly undergoing a LOCA.

This is where JAERI has come in with lots of constrain tests of varying constraints and where one ultimately has to come down to some assessment of what the loads are. Either they think it usually comes down to some tensile load, axial tensile load, on the cladding, arriving either from an axial force related to thermal contraction or a lateral loading that causes damage in swinging of fuel assemblies.

MR. OZER: I think that yes, the constraint due to quenching is a consideration. I think the Japanese did probably the most extreme thing imaginable.

In a PWR assembly, 17 by 17, you have some 260 some rods, fuel rods. The constraints will be controlled by the fuel rods, not the 24 guide tubes.

There are 24 or 25, including the instrumentation tube. I mean, any forces that will be coming from the contraction of the fuel rods will overwhelm any forces that may be coming from the guide tubes.

As far as swinging of the rods and disbursing fuel pellets, there is no clearance between adjacent fuel rods sufficient to allow pellets to fall out. I mean, you would have to have a whole guillotine-type break, and there just isn't room for that. It's just not realistic.

But I really think that these kinds of issues should be discussed at the stakeholders' meeting. I don't think this is the place for it.

MEMBER KRESS: Why are we worried about aftershocks? Are we presuming that the LOCA was caused by an earthquake?

MR. BILLONE: At one time, we did.

MEMBER KRESS: This seems a little like going a little too far in regulatory space.

DR. MEYER: I don't think this is the crux of the discussion. The heart of the matter seems to be that the ductility criterion was invoked so that you didn't have to consider the loads. And if you now have changed to a strength criterion, then you have to ask what loads might be imposed.

MEMBER KRESS: I think that's a reasonable --

DR. MEYER: I don't think it's an essential part of the discussion.

MEMBER KRESS: Yes. That's a reasonable question to ask, but I certainly wouldn't add aftershock earthquake loads into that equation.

MR. CARUSO: I think that is part of the stylized nature of this accident. It's just a postulated sequence of events.

MEMBER KRESS: I think I would rule that out on --

CHAIRMAN POWERS: I don't think it matters because I don't think that you can make a persuasive case to me that you could calculate the loads in a real LOCA. It would be exceptionally hard to do that.

Now, that doesn't obviate the question here of what is the easy way to get things. I mean, one solution clearly is to say there is some ductility. The problem comes in as soon as you say there has to be sufficient ductility. Then you get into the same problem you've got here.

So maybe you have come to the right answer, to have the group of experts get together and kick it around, then come back and talk some more because I don't think we are going to solve it here.

MR. BILLONE: You're not going to solve it here, but the group of experts had better define the tests and tell you what you've learned from these tests.

The fundamental thing which has to be resolved is, are you going to move away from a ductility criteria to a strength criteria? That group of testing experts and material scientists probably can't answer that question.

CHAIRMAN POWERS: Yes. The only comment I think I can intelligently make now is that I am very suspect of coming in and saying, "Let's run .15 or .3 joules hammer tests" and walk away from it at that point because I just don't think that tells me anything useful except make sure any of the little guys that climb through the core during cooling have real small hammers. That's the only thing it tells me.

Any other questions?

(No response.)

CHAIRMAN POWERS: Thank you, sir.

Rosa, where are we now?

MS. YANG: We will present -- go ahead, Jeff. We are going to talk about AOA and rods, PWR rods.

MR. CARUSO: But this will be closed, Rosa?

MS. YANG: I think it's okay.

MR. DESHON: Yes.

MR. CARUSO: So this will be open.

11) PWR CRUD
AXIAL OFFSET ANOMALIES

MR. DESHON: Well, good morning. My name is Jeff Deshon. I am kind of a lone wolf at our fuel group at EPRI. I am not a physicist or nuclear engineer or material science guy.

I actually worked at one of these power plants for 16 years as either a chemist or a chemistry manager. And I came to EPRI in 2000 to work for Rosa. So that's not necessarily a bad thing, but I'm not one of those people because --

CHAIRMAN POWERS: Working for Rosa isn't a bad thing.

MR. DESHON: Because the issue that I came to EPRI for was to work on this phenomenon that's referred to as the axial offset anomaly. And in attacking this problem for the industry, it's really as much of a chemistry issue as it is a fuel issue.

So this presentation is broken down. I

thought I would kind of get everyone on this equal footing from the very beginning and go through, number one, what our objectives are from our particular working group in the robust fuel program, give you some background information, what is AOA, the history and implications of the phenomenon to our operating plants, and then here discuss with you some information that you probably haven't seen before in the area of crud -- and this is work that we have been conducting for the last several years -- and then finally review with you some strategies that we are pursuing in trying to resolve AOA.

Okay. Our objective for working group 1 of the RFP is indeed to minimize the threat of PWR axial offset anomaly. And the focus here is on doing that through managing core crud deposits.

Axial offset is normally defined as the integrated power in the top half of your core minus the bottom half over the sum of the two. Axial offset anomaly is a significant deviation from those, the numerator and denominator. It's typically observed as a negative offset.

Now, if you are of combustion engineering vintage, they express it essentially in the reverse order. And they refer to it as the axial shape index, or ASI. And it's merely the negative axial offset if you were to use the other definition.

Well, every core design has with it an associated predicted axial offset or AFD it's commonly referred to, axial flux difference, curve. And these are done using the design codes, either ANC from Westinghouse or SIMULATE. And they predict the axial offset during the cycle at various burnups during that period.

On a monthly and more frequent basis, our power plants will go ahead and conduct flux maps. And what they will do is compare the power data from those flux maps to the predicted code.

A few utilities have fixed in-core detectors. These are kind of nice features to have at a power plant because it gives you real-time data. It's constant data generation. The only problem with them, their resolution isn't quite as great as the movable in core detectors.

So getting back to the anomaly, as I mentioned, it's a significant deviation between the measured power data and the predicted. Now, the deviation results from a phenomenon where boron concentrates within crud and as crud is occurring in the upper strands of your core, where you have significant subcooled nucleate boiling taking place.

Once there is a critical or a sufficient boron that has accumulated within that crud deposit, then you will start observing flux depressions in the top of the core. For the core to maintain power, it will physically shift the neutron flux downward, actually, towards the bottom half of the core. And it results in this more negative axial offset than the prediction.

Now, historically a plant that has encountered AOA has been defined as a deviation of greater than, say, -3 percent. And that is to incorporate all of the uncertainties that might be present in the nuclear code design.

Here is a curve of what the power plants are trending during the operating cycle. This y-axis here is percent AO or it could be AFD, axial flux difference.

The x-axis is in burnup. And the characteristic features of this is typically axial offsets anomaly will start rearing its ugly head at a burnup of between maybe four and eight gigawatt-days per metric ton. And you will typically start seeing a dramatic shift in the slope of that line.

In this case here, this is a fairly

dramatic case of axial offset anomaly, where the deviation is quite far from the predicted line, here on the order of maybe -8 percent.

Now you will see it gets at its worst at about maybe 10 to 14 gigawatt-days per metric ton but starts taking a very positive, steep change towards the end of the cycle.

What is happening there is basically you are later in cycle, you have less boron in your active coolant system, you're burning up the low portion of your core, and the flux is starting to shift upward towards the end of the cycle. And, hence, you get this very steep slope coming out on the back end.

Here is a flux map of an individual assembly. And it's actually taking over the course of the operating cycle. What I wanted you to observe from this, from an individual assembly, is what we have here is this is a normalized power distribution for this assembly. And on the x-axis is the axial position in feet.

So this is your pretty typical 12-foot length assembly. And this aqua color here was the first flux map that was taken of this assembly. You can see it's very symmetrical. It was taken at a burnup of only 208 megawatt-days per metric ton. So it was taken up very shortly after the plant achieved full power during the cycle.

But if you take a look at the dark blue line here, taking close to 10,000 burnup or 10 gigawatt-day burnup, you can see that you've got a significant neutron flux taking place in the bottom half of the core.

Anything from this side to from six feet down below is the bottom portion of your core. And to the right of that is your top portion. You can see you've got more power being generated in the bottom half than you do on this, the top half. And this has persisted, even through a burnup of about -- what is that? -- 16,500 with this green line, but towards the end of the cycle, at a burnup of around 21,000, you can see this red line. You've got a more symmetrical shape distribution for this particular assembly.

So this is the kind of information that the operators or reactor engineers are looking at at the plant. The first documented case of AOA took place actually back in the early '70s at the Obrigheim unit in Germany. Their reactor coolant chemistry control wasn't particularly strong. PH wasn't controlled very well. And, most importantly, they had very little hydrogen over-pressure on the reactor coolant system. So they actually had oxygen and oxidizing conditions in the reactor coolant system.

What this led to was excessive corrosion of their ex-core materials and the corrosion products deposited in the crud. And what that did was it provided that foundation or that substrate for boron to concentrate and deposit.

The first domestic case occurred in the late '70s. And what was speculated in that case was that they had during a specific period during the cycle a significant amount of oxygen intrusion into the coolant. That in itself created additional corrosion products to circulate and deposit in the core. Again, it was enough corrosion product that deposited such that the boron could concentrate in deposits there to cause the flux depressions.

So AOA wasn't fashionable until the '90s, if you will. And why did that happen? Well, we've got good chemistry control. We've got good hydrogen control. You know, we learned from those earlier experiences in the '70s.

So what really transpired or initiated this problem was that the utilities were going to longer operating cycles. That was based pretty much on an economics position.

So to improve their fuel cycle economy with these longer operating cycles, they had to go to higher assembly powers, higher enrichment in the rods, higher thermal duty. And in some cases, to further improve their economics position, they were loading fewer assemblies. Therefore, they needed more energy out of the assemblies that they were actually loading, so higher thermal duty.

So if we connect the y to the root cause of AOA, accompanying these transitions to a higher-duty fuel, it created higher subcooled nucleate boiling taking place in these new assemblies that were being loaded.

And the subcooled boiling was taking place in those upper spans, typically in a Westinghouse assembly, spans 5 and 6; in a combustion engineering assembly, maybe in spans 7 and 8.

Well, having higher subcooled nucleate boiling does result in an enhanced deposition rate of corrosion products. Now, corrosion products are going to and always have deposited on fuel assemblies, regardless of fuel duty. That's just the relative thermal dynamics and chemistry behind frozen product release and deposition. Where you have significant subcooled nucleate boiling, it does act maybe as a magnet to these corrosion products.

Once a sufficient crud thickness has been established, the local conditions within that crud, be it the temperature, morphology, -- there are a number of them -- they create the environment that boron will concentrate and will either deposit through a precipitation process or, what we have also learned through our studies, through an absorption process as well.

To place it all in Venn diagram form, we believe you need three components, constituents for AOA to manifest. The first thing, you need the subcooled nucleate boiling component because before we started really pushing these assemblies with their fuel duty, the subcooled nucleate boiling taking place was present but not at such a high rate.

You need corrosion products that are circulating in your core because it's the crud that deposits in these areas of subcooled nucleate boiling that creates the environment for boron to deposit. And, of course, the boron rate here is in the green, designated in the green circle. So root cause is the boron deposit in crud in these high steaming assemblies.

So what? What if a PWR encounters AOA?

Now, I told you I was a chemist at the beginning. I'm not an expert in the physics business, but that's why I have actually put more description in here to help me out.

Steady state power operation for the most part, you know, AOA is not going to have that much of an impact on you. And that certainly might depend on the severity of AOA that you're encountering.

An example of where it could create a problem for particularly one with severe AOA is towards end of cycle because with each one of these axial offset or AOD prediction curves, there are also limits associated with those as well.

So towards the end of the cycle, I showed you on slide number 8 there. Towards the end of the cycle, there was a very rapid escalating AO shift for that particular plant. And that condition can approach your tech spec limit for AFB.

Now, additionally we've got a lot of utilities out there that undergo coastdowns at end of cycle. And, as you see here in this last sentence, the coastdown end of cycle may make the AOA situation worse because as the temperature is dropping, you are adding more reactivity into the top of that core. So the AO becomes more positive.

So that was during steady state conditions. During power transient conditions, again, it depends on the severity of the AOA. But during a downpower maneuver, what you will find is just try to visualize the boron and the crud are on those upper spans of those fuel assemblies because of the subcooled nucleate boiling.

When you downpower, your boiling actually ceased to exist for the most part. So what happens is that boron that has precipitated or absorbed there actually goes back into solution.

So the result of that, you have a little bit more boron in your reactor coolant, maybe a ppm or two, but you also have a more reactive upper portion of the core.

So then you're talking about rod worth. You actually reduce the rod worth of those control rods that you're inserting because you have a more reactive upper portion of your core.

So in severe cases, the control rod insertion limits may be insufficient to perform or to permit operation within the AFD control vents.

Another thing that takes place is just the reverse. When the power escalates back up to 100 percent and you start getting into your subcooled boiling taking place in those upper spans, then the boron goes back into the crud.

What happens is you have got actually pressure boron being deposited because prior to that downpower, you had boron there, but it was actually being depleted over the course of the operating cycle. So when you actually downpower and then ascend back up, that infusion of fresh boron or less depleted boron actually makes your axial offset or your flux depression more severe.

Here is an example of it here. Your y-axis is percent AO. Your burnup here is in megawatt-days per metric ton. And if this is a controlled shutdown here. And you can see there was a slight change in axial offset resulting from that.

But if we concentrate on this power transient here, where they went down to 30 percent reactor power, you can see a very dramatic change in the AO of that particular core from about maybe -4 down to close to -8 right here. The cause of that is largely because of this infusion of fresh, undepleted boron going back into the crud.

This graphic here, these are actually computer simulations. It's an example of a power maneuver for a plant without AOA. And, just to give you some reference here, this y-axis here is percent power. And it's also the control rod insertion percentage.

So you've got to kind of look at it in reverse order. The power starting out is 100 percent. And the control rod insertion is 100 percent full out.

This dream curve represents boron. And this dotted line is your rod insertion limit here. And down below, this is your axial flux difference or AO y-axis. And your x-axis on both is time.

So in this case, they're middle-of-cycle conditions. And the power plant is reduced to 28 percent at 8 percent per hour. They hold at ten hours. And then they increase at eight percent on the way back up.

The control rod insertion limits and AFD operating bands are shown. And you can see that they don't encroach on the RIL limits, and they certainly don't encroach on the AFD limits here.

In the next example, we do have a plant with AOA. You can see starting from the get-go, they already have rods partially inserted into the core at mid cycle to keep power normalized while at full power.

The blue line again is the power rampdown, hold at 28 percent and go back up. But in this case, you can see they're having to insert rods earlier, from the very beginning but earlier, in the downpower. And they come very close to the limits.

At the same time, you can see there they really have to borate at a much steeper rate and at a higher concentration as well in order to achieve the power levels they're seeking.

Down here you can see the AFD curve likewise is having some dramatic effect in that they're starting off at a lower AFE, -10, and as the downpower starts taking place, you've got all of this new reactivity taking place in the top of the core. So it's causing your AFD curve to go vertical. So they come close to the AFD limits as well.

So this is the sort of thing that challenges operators that have a case of AOA. Granted, this is more of a severe case of AOA, but it's something the operators have to be very mindful.

Then you also hear about AOA having an effect on your shutdown margins. I am really not an expert here on this. So I provided as much information as I can here, but the bottom line is you're burning out a great deal of your reactivity over the course of the cycle in the bottom half. And when you shut down your reactor, you have got all of this excess reactivity in the top half. It causes your rod work to be minimized quite a bit.

So the net effect is some degree of shutdown margin is lost following reactor trip as compared to operation without AOA.

MEMBER SIEBER: Does that affect the temperature coefficient?

MR. DESHON: MTC? I can't answer that. MTC is certainly a factor in your AO for sure.

MEMBER SIEBER: I would think so.

MR. DESHON: So there must be some interrelationship there. Jeff, would you happen to -- does the AOA affect MTC?

MR. SCHMIDT: It could slightly because you're getting your power distribution. So you're getting a slightly different weighting of your moderator temperature coefficient, but I don't think it would be a big effect.

MR. DESHON: Well, a couple of other implications that have been observed at our power plants. Yovan Lukic mentioned a couple of fuel failures that have occurred during the 1990s. There have been three that crud has played a role in those failures. And in the three cases, AOA was also present.

I did want to make mention, though, that AOA in itself does not necessarily lead to fuel failures. And we have had about 40 operating cycles in the United States that have encountered AOA, some of them very severe.

Some of you might be aware of the Callaway Cycle 9 case. It's the worst recorded AOA case that we have had in the United States. And it actually forced them to de-rate down to 70 percent power. And they gradually recaptured some of that during the cycle. But even in that severe case, they didn't encounter any excessive corrosion on their fuel assemblies.

Another issue that is very important to the plant, the fact that with these mounting crud levels that can occur in these plants, when they shut down for their refueling outages, this crud can be released, sometimes in an uncontrolled manner. And it leads to very high dose rates in the reactor coolant system.

So not only do we have this AOA operational issue, but we have got a couple of other issues that are quite important to the utilities.

MEMBER SIEBER: I thought all the PWR

operators had practice of borating the plant to a very high level while it was still hot to sort of cause a crud burst so that they would reduce the outage. Is that the case?

MR. DESHON: That is true. The whole purpose of shutdown chemistry is to induce a crud burst in a controlled manner. And part of that is indeed to borate early, to create an acid environment very early that will help you as you cool down dissolve that material so it can be removed efficiently by your demineralizers.

However, we have encountered when during either the rampdown for refueling outage or after all the rods have been dropped in, we see this release of corrosion products. And they're particulates. And they go out there, and they stick on the system surfaces. They're just very difficult to remove under our standard control parameters.

MEMBER SIEBER: Okay.

MR. DESHON: So our business here or focus has been on trying to address this component of the Venn diagram. Let's see if we can control deposits. We're not going to do anything in the near term about taking boron out of our systems unless we go to a Navy reactor design.

The subcooled boiling, it's the subcooled boiling portion of this Venn diagram that utilities have had to back off on or have addressed AOA by. And what they have done is instead of loading, say, 80 assemblies per reload, they have loaded, say, 86 or 90 or more assemblies to actually reduce the overall boiling duty of these courses. So our focus here is, what can we do about these corrosion products to mitigate AOA?

So I wanted to discuss this aspect of our program. We have got a number of aspects, research activities, that we have got going. But this might be of interest to you.

We devoted quite a bit of resources and time into understanding corrosion products, what is circulating in the reactor coolant as well as what is depositing on these fuel assemblies.

With this learned knowledge, we hope to be able to improve our modeling capability of the phenomenon as well as help us in our pursuit of mitigation strategies.

To this end, we have installed high-temperature samplers. That's two PWRs. Those are at Catawba and Diablo Canyon. Why did we do that? The sample systems for our PWRs are really quite poor in terms of being able to use them to characterize corrosion products.

You're taking reactor coolant that's 500, 600 degrees and cooling it down to ambient temperature at the sample sink. And during that cooldown process and then after you actually sample it, it's exposed to air, you bring it back to the laboratory, it's just not very conducive for corrosion products because they undergo certain kinetics and thermodynamics that don't afford you to collect the actual specimen that is in the reactor coolant.

So we have installed high-temperature samplers at these two units. And the next slide will describe those a little bit. Additionally, we have performed a number of crud scrapes at these units and cycles over the last several years.

I've actually put two of the Vogtle cycles in green here because they actually paid for those, but they are providing that information to our program.

Okay. We have installed the high-temperature samplers. I told you pretty much the purpose behind those samplers. One of the aspects that we are trying to look at is, is there any difference between circulating corrosion products at the beginning of cycle versus the way they are at mid cycle or end of cycle?

MEMBER SIEBER: How many zinc injection PWRs are there?

MR. DESHON: Let's see. We have got the two Farley, two Diablo, Palisades, two Sequoyah units, and Beaver Valley, and Callaway right now. Those are the ones I am aware of.

MEMBER SIEBER: And why are they doing it?

MR. DESHON: Well, the first units that did it, Farley and Diablo Canyon did it largely for PWSCC mitigation. The other units that I mentioned have done it for dose rate reduction, which it has been very effective at doing that, reducing radiation fuels.

MEMBER SIEBER: Well, the amount of corrosion products, is the amount that is circulating around in the coolant system less or is it just in different places?

MR. DESHON: In the zinc plants?

MEMBER SIEBER: Yes.

MR. DESHON: I think I touch on that in a later slide.

MEMBER SIEBER: All right. I'll wait.

MR. DESHON: If I don't, hit me up on it

again.

MEMBER SIEBER: I will.

MR. DESHON: Okay. Here are the attributes of the high-temperature samplers that we've got. We have probably all had an opportunity to read this. So I won't go into it any further.

I guess the key point here is we are collecting these samples at between 430 and 445 degrees versus 25 degrees. The sample lines installed at these stations are on continuous flow. So there is never any interruption.

So in corrosion product sampling, you can't just go out there, open a valve, and expect to get a good sample five minutes later.

MEMBER SIEBER: Right.

MR. DESHON: You need to have continuous flow through these things for days, if not weeks, before you can feel fairly certain you're collecting a representative sample. Well, that didn't come out very well, did it?

Here is a particulate sample that we have collected. It was actually the Catawba station. And the sample is collected at the beginning of the cycle. Basically, it looks like garbage, circulating garbage in the reactor coolant system. You have few well-formed crystals. And these crystal sizes vary rather dramatically.

Here is an elemental mapping of the same sample. This didn't come out very well either. What you have here is you have got various elements down here. You've got manganese, chromium, nickel. The ones to concentrate on are iron, nickel, chrome, and zirconium.

So what we have found in these high-temperature samplers was that: number one, you've got metallic nickel. Now, this is a single nickel atom that is not in the +1 or +2 state. So it's actually a particulate.

You've got chromium, when present, as part of the iron oxide phase. And something kind of interesting that we found was that we've got quite a bit of zirconium, zirc oxide particles, circulating in these cores, particularly at the beginning of cycle.

What you will see a little bit later in the actual crud samples is you have got a fairly high fraction of zirc oxide in those.

MEMBER SIEBER: Is that due to the manufacturing or is that some corrosion product?

MR. DESHON: Well, it's from the fuel. We're not certain how or why it's coming from the fuel, but most --

MEMBER SIEBER: It sounds like it's some kind of residue left over from the manufacturing.

MR. DESHON: Perhaps that or maybe on a reload or reinsert fuel that has had maybe some spallation take place, corrosion taking place. It's being released into the coolant just due to the start-up effects. So thermal and hydraulic --

MEMBER SIEBER: Got it.

CHAIRMAN POWERS: But don't claim there's any spallation. Rosa says that's a terrible field, right?

MR. DESHON: Okay.

CHAIRMAN POWERS: Let me ask you. You showed some EDX maps. The problem with EDX is you can't detect boron. When you try to assess the chemical form of these species from EDX, do you know they're not borates?

MR. DESHON: Well, we don't anticipate boron to be part of any of the circulating corrosion products because boron is very soluble. And it will only be part of a corrosion product if it's on an assembly where there is subcooled nucleate boiling taking place and that mechanism has forced it to deposit within that crud structure.

CHAIRMAN POWERS: Boron in the form of boric acid or an alkaline metal borate is soluble, but some of these borates, like iron chromium and things like that, they can precipitate at fairly low concentrations.

MR. DESHON: With boron?

CHAIRMAN POWERS: Yes.

MR. DESHON: Well, we haven't seen it.

CHAIRMAN POWERS: You never will with EDX.

MR. DESHON: Okay. What you will see here is that we don't rely on a single analysis for these data. Here is an example of that. This is an XRD graphic here. And this isn't as good as one down a few more pages.

This blue here is a silver peak. The silver is there purely because these samples are collected on a silver-impregnated membrane. So that's why there's silver. It's not that we have circulating silver in our reactor coolant system.

MEMBER SIEBER: It's not clear to me that you don't, though, because the control rods are filtered. And so if you have a bunch of cracked control rods, you are likely to have silver in some small quantity in the coolant.

MR. DESHON: Yes. I'll grant you that

possibility exists, yes. These silver peaks are very high, though. The red line iron oxide are magnetites. The aqua blue is trevorite or nickel ferrite. And the black lines are zirc oxide.

This doesn't show up here very well, but the point here is that what has been extracted from the data is that we have got a highly substituted nickel ferrite as a major phase. And by "highly substituted nickel ferrite," I mean that NiFe2O4 component has more nickel present than a traditional corrosion product you might see, say, on a fix to a steam generator surface. And, again, zirc oxide is a major phase in circulating crud.

Now, again, this was from Catawba station, which is a non-zinc plant. Here is an end-of-cycle sample that you can see and is a pretty dramatic difference between the beginning of cycle.

You know, the beginning-of-cycle sample looked like, again, garbage. Here you've got very well-defined crystalline structures, octahedral in shape. It's still a nickel ferrite, but the nickel fraction of that ferrite is very low.

The XRD scan that you'll see I think next detected nickel ferrite almost exclusively. And the SEM suggested minor zirconia and metallic nickel in chromium-rich oxide.

Look at these very nicely shaped crystal structures. Here is the elemental map showing the same sample with -- you can see this pretty much confirms that it's a near magnetite sort of corrosion product. The nickel fraction is very low here. And here is a sole zirconia particle right there.

This arrow is pointing to what was believed to be nickel metal right here.

MEMBER SIEBER: Did you say that was one atom?

MR. DESHON: No, not one atom. What I mean, it's --

MEMBER SIEBER: I was going to say that was a pretty good picture.

MR. DESHON: Yes, high magnification. It's essentially a nickel particle comprised solely of nickel. So from the Catawba sampling, trying to just generalize here, you have high nickel in ferrites and more zirconia at beginning of cycle. You've got the nickel ferrites again at end of cycle but low in nickel concentration. And you don't have nearly as much zirconia circulating in your coolant.

Here is a zinc sample from Diablo Canyon. This one was taken at middle of cycle. You can see here -- and this was something of a common theme, not only with the corrosion product samplers but also the crud that we have sampled from Diablo Canyon, as that the crystalline structures aren't nearly as well-defined and -- I hate using this term -- almost amorphous in its visual appearance.

Here is an XRD map of that particular sample. You can see we have got nickel ferrite or trevorite circulating in there. We have got zirconia, nickel metal. You see a very significant peak of carbon, which kind of intrigued us, but the zinc addition taking place at the applicant is in the form of zinc acetate. So chances are it's from a reduced form of the acetate.

Once again, looking at this scan, carbon is a constituent of circulating material. You can see these corrosion products really don't have defined edges to them like they do in the other plant.

MEMBER FORD: Where did the zinc go?

MR. DESHON: That's an excellent question. We don't expect to see a whole lot of zinc in these particulate samples because zinc is very soluble in the reactor coolant.

So, in fact, its solubility is somewhere between 100 and 200 parts per billion. These plants are only adding. Diablo, in this case, I think we're adding somewhere between 15 and 20 PBB. So chances are you are not going to see a whole lot of zinc in these particle samples.

MEMBER FORD: In the BWR, zinc is incorporated, I remember, into the magnetite.

MR. DESHON: I'm not a BWR guy, but we have got plenty of BWR experts back there that certainly could confirm that.

CHAIRMAN POWERS: You're a much higher class than BWR people.

MR. DESHON: The boilers couldn't give you information on these circulating corrosion particles like we have. All right. Excuse me. I digress.

So here are some general observations of our high-temperature corrosion product sampling thus far. And I don't want to read all of these to you, but something of interest to us here is the fact that we haven't seen any nickel oxide in these samples. And you will see in a few later slides nickel oxide is a component of crud from these high-duty plants that have experienced AOA.

Another interesting part here is that metallic nickel particles are an important part of reactor coolant system circulating corrosion products.

And we don't analyze a whole lot of nickel in crud samples. The reason for that is that nickel is very soluble under the shutdown chemistry process that we employ at these PWRs.

So we have been able to confirm that it is quite important during the operating cycle, and we know, therefore, it's present in the actual crud that we measure after the plants have shut down.

And the other aspect here is what we just didn't appreciate is that zirc oxide is a common circulating corrosion product.

CHAIRMAN POWERS: I'm surprised you don't see more manganese.

MR. DESHON: Yes. It's not a large component in the structural materials. And it perhaps could be due to how well manganese will diffuse through that corrosion film and reside in the soluble form or particulate form in the coolant.

MEMBER SIEBER: This is probably a stupid question, and maybe you didn't do it, but have you compared the high-temperature samples that you would draw as a liquid versus the fuel scrapings?

I would expect a lot of different constituents because you could have something that is soluble in the coolant and not be on the fuel and vice versa.

MR. DESHON: Yes.

MEMBER SIEBER: Could you say a word or two about that if you did it?

MR. DESHON: We are in the process of doing that right now. One of our major deliverables for next year is we are going to publish a report that collects all of this data that we have obtained through crud scraping.

Westinghouse, we have been working on this with Westinghouse a great deal. They are going to introduce crud scrape data that they have done previously.

And we are going to look at not only the crud scrapes. We are going to look at the primary water chemistry during those cycles, how much nickel and iron was removed during the shutdown chemistry process at end of cycle. And we are also going to be looking at these high-temperature corrosion product data.

The goal here is to try to piece everything together. And so, to answer your question, we don't have any definitive response to that right now, but we are in the process of analyzing it.

MEMBER SIEBER: I think that would be

interesting when you arrive at conclusions from that work.

MR. DESHON: That's right. That's right. Exactly.

MEMBER SIEBER: I'd love to read it.

MR. DESHON: Okay. We're going to move into the crud samples.

What time are we going to stop for lunch, 12:30?

CHAIRMAN POWERS: We're going to stop when you're done, but we have targeted 1:00 o'clock.

MR. DESHON: 1:00 o'clock, wow. I'll be done before then, I think.

CHAIRMAN POWERS: I just have a time listed down for your presentation.

MEMBER SIEBER: There will be another meeting going on in here.

CHAIRMAN POWERS: Not a chance.

MR. DESHON: We're going to move into the crud scrape data now. Just to kick this off, I showed you on a previous slide we have collected crud scrapes at a number of plants. What we have tried to target are higher-duty units that have experienced AOA and some that have not experienced AOA and tried to compare the cruds from those two camps of PWRs.

Now, here is an example here. This y-axis here, the unit is it's mass evaporation rate, pounds mass generation per hour foot squared. It's a unit of measure that we use to measure the duty of a particular assembly.

And this particular assembly here did experience severe AOA during the operating cycle. You can see that. And what these different colors represent is the calculated mass evaporation rate at different burnups during the operating cycle.

So the way that these designed these cores, these assemblies is that they will have higher duty at different points during the operating cycle. So that's what these different colors reflect.

Here is an assembly, a high-duty unit as well, but this particular assembly did not experience AOA. Now, you can see the duty of this particular assembly here was certainly less than this, but I happen to also know that this assembly came from a central core location. And it was surrounded by already burned assembly.

So those also influence crud deposition as well. Yovan Lukic mentioned that yesterday during his speech with his ring-of-fire discussion or Saturn pattern-type loading patterns versus checkerboard.

So here is a kind of a topical look at crud from the assembly that did not experience AOA. And it looks kind of like a moon structure here. Each one of these holes represents a boiling chimney taking place.

You hear about crud. Crud is bad. Well, not all crud is bad because having a little bit of crud on your fuel surface actually enhances heat transfer. It gets your subcooled boiling taking place a little bit better. Subcooled nucleate boiling is a much more effective heat transfer mechanism than forced convection.

So having fairly thin crud with these boiling chimneys isn't necessarily bad. This crud is comprised of particles. It has boiling chimneys, the thickness generally less than 20 microns. There were a few samples here that were upwards to 20 microns. But on average, the crud thickness from this particular sample was less than ten.

Now, something that we can point out here is that I want to point out that the nickel-to-iron ratio in this sample here was on the order of .57. Now, that's a fairly traditional nickel-to-iron ratio that we have found in the historic crud database, anywhere from, say, .4 to .6.

Here is crud from the other assembly that encountered severe AOA. Again, this did not show up very well. However, this was crud that we were able to measure flake thicknesses from on the order of 125 microns. And this is the clad interface right here. And this is the coolant interface up here. And you can see these very large voids in the crud, representing these boiling chimneys.

An additional feature to this crud -- and it doesn't show up real well here. I do have another slide that I should have brought along as well.

There is a region around here where this is almost exclusively zirconium in this particular crud flake. Just going down here -- well, I mentioned this here already. Zirconia layer approximately 25 microns from the clad interface.

And another observation is that it's a very high nickel-to-iron ratio, on the order of 1.5 to 2.5, certainly well beyond the stoichiometric nickel ferrite that you see in traditional crud samples.

It looks a lot better on my laptop. Wow, look at that. Have you ever seen crud look like that? You contrast the previous sample of the non-AOA crud. It was comprised of particles. Well, this stuff here, this rod-like or needle structure, was something that we had never seen before.

Now, this was, again, I wanted to point out, from a very severe case of AOA. But what you are looking at here from this vantage point down to here is just a side view of that flake.

So what did we find in that crud flake? Well, we found a new material called bonaccordite. Now, getting back to Dana's comment earlier, bonaccordite, just advance here one, does indeed have a boron constituent to it. But the only other place bonaccordite has ever been seen is at a meteor site in South Africa. So that's kind of a strange occurrence that it is taking place inside our reactors here.

So this particulate crud flake was made up of four primary components: bonaccordite, trevorite. That's this particle here that's attached by a number of these needle-like structures. We had nickel oxide, and that's this kind of matte-looking material here. And this brain-looking structure here is monoclinic zirconia here.

So what is unique about this is: number one, we see this bonaccordite structure. We actually believe we have seen this now in two reactors. Both units were experiencing severe AOA on the assemblies that were scraped. And the crud flake profile looked very, very similar between the two.

The other unit, we had some -- actually, it wasn't us. It was Westinghouse had done some less sophisticated analyses on that particular crud scrape campaign. So we weren't able to specifically identify this bonaccordite structure, but it sure looks like it because it, too, had needles in its composition.

So what's unique, again, is the bonaccordite and the presence of this nickel oxide because the nickel oxide isn't typically seen in thinner or from rods from lower duty. So if we take a look at that observation, bonaccordite comprised a significant fraction of the weight, 50 weight percent. And this material was found insoluble. You just can't beat it up.

Now, the only thing that they applied that would make any impact on it was hydrochloric acid. It's just not going anywhere through your normal shutdown chemistry program.

Zirconia found mostly at the -- let's see here -- at a depth -- let's see. How did I word this? At 20 to 50 percent of the clad depth from the clad surface. And it comprised about 30 weight percent, fairly large fraction.

Nickel oxide and nickel ferrite rounded

out the composition, both having about ten percent. Now, the fact that the nickel ferrite was only ten weight percent of the structure is unique in itself because typically nickel ferrite is the predominant crud oxide.

So if we take a look at some observations --

CHAIRMAN POWERS: If I take the nickel ferrite amount in the heavy deposit and compare it to the nickel ferrite in a unit that is not having an AOA, --

MR. DESHON: Yes.

CHAIRMAN POWERS: -- is it the same amount?

MR. DESHON: It could be. It could be, yes, because that particular flake of 125, the other one was only maybe 10 micron. So after looking at some assemblies with less severe AOA, say modest, moderate AOA or very low, mild AOA. We come up with some additional findings.

The amount of crud is proportional to boiling. That I guess is rather intuitive. But the inventory of crud is greater, five times greater, in AOA plants than in non-AOA plants. And the crud is still iron-based for the most part, but the nickel-to-iron ratio in these moderate AOA plants is on the order of .7 to .8.

Again, the historical crud, the lower-duty stuff, may be .4 to .6. We're transitioning here with a modest AOA, .7 to .8 maybe, .9. And then when you get into the severe AOA cruds, you're approaching up as high as a ratio of two to one.

So from these campaigns, we can at least come to these following points at this time, again, crud mass proportional to subcooled nucleate boiling.

Since nucleate boiling doesn't start occurring until typically spans 5 and 6, crud is typically heavier in those spans. Now, if you will look at these crud deposits from non-zinc injection plants, you see very little or no crud in spans 1 through 3, start seeing a little bit in span 4, and then more crud in spans 5, 6, and a little in 7.

Nickel-to-iron ratio increases with rod power and boiling duties. And something else that we have seen which is important to us from a crud modeling standpoint is that the porosities are lower in the crud flakes closer to the clad than they are to the coolant.

Nickel metal is typically absent in the thicker cruds. Nickel oxide is more prevalent in those deposits undergoing high SNB subcooled nucleate boiling. Thin crud has more chromium than thicker crud. Why is that? I'll get to that in the next slide.

Thick crud from rods undergoing significant SNB have fully substituted nickel ferrite. That's probably more information than you are interested in, but it still is important to our studies.

CHAIRMAN POWERS: Everything seems to be pointing toward differences in the oxygen potential during boiling.

MR. DESHON: You're a wise man. That's right because it could be that and we think it is probably a combination of these conditions taking place. Number one, we could be seeing an elevated pH taking place within those deposits because we know through our plant experience. And one of the boron deposit theories is that we're precipitating a boron-lithium compound.

Just to give you a sketch here, during a power reduction very commonly -- and a characteristic of AOA plants is that you're not only releasing boron, but you also see an increase in lithium taking place in your reactor coolant system.

After that power transient has stopped and upon ascension to power, you see lithium hideout take place. So you actually see a reduction in your coolant lithium concentration. So we know that there is an increased concentration of lithium above the bulk coolant in these crud deposits that could be resulting in an elevated pH.

Now, to touch on what you were saying is as you develop these thicker deposits through a boiling process, you have hydrogen present in these crud deposits. But the hydrogen because of its volatility is being stripped out from the deposit at a high rate.

And we also are aware very locally next to that clad location that we are producing radiolytic species, such as free radicals, hydrogen peroxide. And these are changing electrical chemical potential within that deposit from a reducing environment to a more oxidizing environment.

And, for that very reason, you're right. You know, seeing observations such as you're seeing less chromium in these thicker deposits, you're seeing a change in iron state going from .2 to .3 all lend itself to that sort of process taking place.

Nickel oxide, for example, we're not

seeing it in our coolant samples, but we're seeing it in these thick deposits. So the generation of nickel oxide very well could be this mutation from nickel metal to nickel oxide.

MEMBER FORD: So you're almost going to a BWR.

MR. DESHON: I wouldn't say that.

MEMBER FORD: All of those things that you mentioned are --

MR. DESHON: Right. And these things are happening very locally, right at the clad surface, within just a few microns.

All right. Well, I've discussed some of our activities and intelligence on crud now and corrosion products. The next several slides will just demonstrate a few examples of what we are doing in terms of minimizing crud and hoping to avoid AOA.

Rosa mentioned here we've got ultrasonic fuel cleaning technology available now. And I've got a few slides on that. We are pursuing an elevated and constant pH program.

Our EPRI primary water chemistry guidelines now encourage utilities to stop employing the traditional modified pH program that has been very common since the late 1980s and transition to a higher and constant pH within the limits allowed through your fuel vendor and your materials evaluation for, for example, your steam generators.

So what the guidelines suggest is see if you can approach a constant pH of 7.1 or 7.2 so that you have a constant pH throughout the whole cycle.

What we have done is taken it a step further in that we do have a demonstration taking place at one of our utilities that has increased the pH to 7.3 throughout the whole cycle and ultimately will go to 7.4 if everything pans out well. The other strategy that we are pursuing is zinc addition and, finally, boric acid, enriched boric acid.

Ultrasonic fuel cleaning. And these just don't show up that well here, but we have installed the ultrasonic fuel cleaners, as Rosa mentioned, at Callaway, South Texas project, and this coming week Vogtle will be applying it at one of their units.

The original prototype was installed at Callaway. And it was a single channel ultrasonic cleaning unit. And we demonstrated it during, I think it was, cycle 10 on 16 reload assemblies. And we had very good success with those assemblies during the operating cycle. So they went ahead and did a full reinsert cleaning prior to cycle 12 and cycle 13.

So what this diagram is showing is this is actually a more advanced unit, one that, let's say, for example, is employed at South Texas project, but you have got two chambers here that can accommodate a fuel assembly for fuel cleaning.

Up here you have a pump skid and filtration unit. And the pump will actually draw water from the bottom of the ultrasonic fuel cleaner down here and will draw it up through the filters and back into the pool.

What we have in these units in the South Texas project unit are 12 ultrasonic transducers that are vertically mounted inside the cleaning canister. And when the assembly is inserted into the canister, you apply the power to it. It takes probably three to four minutes in order for the material to be fully removed or removed to their satisfaction.

The way that they monitor when they are done cleaning a particular assembly is that they have radiation monitors on the hose and on the filter banks. So they will see initially a very rapid rise in dose rate or gamma activity on the hose. And once that subsides, they know at that time that they are pretty much done.

Now, I mentioned to you earlier that that bonaccordite was essentially impervious to any shutdown chemistry technique. If a PWR has bonaccordite, really, the only way to get rid of it -- and we have been able to demonstrate it -- is through this ultrasonic fuel cleaning. And we were able to successfully do that at Callaway during a cleaning efficacy project that we had demonstrated there.

This cleaning technology removes at least 85 percent of the fuel deposit on these assemblies and in most of the cases will remove above 90 percent. So I have kind of expanded on that, only because I thought you might be interested in that information.

Evidence suggests crud from reload fuel can redeposit in boiling regions of feed fuel assembly. We have evidence that that transpires. What happens when you reinsert fuel, depending on the relative power of that fuel in a second cycle of operation, if it's low-duty, then if you've got material on the rods, it's going to dissolve or it's going to be released through thermal hydraulic forces or hydraulic forces going, fluid shear going up the assembly. And that's because there's nothing really keeping it there.

The subcooled boiling isn't taking place on that assembly any longer. So the crud, we have been able to show through our crud scrape campaigns, actually reduces in that second cycle of operation. It releases the crud and makes it available to come back around and deposit on the new feed fuel, where you do have a lot of subcooled boiling taking place.

Early-cycle boron hideout promotes early onset of AOA and greater chance to degrade. That is just talking about if you don't have the crud substrate there early in the cycle, then you are less likely to encounter AOA on any reinsert fuel also.

We have had one case. This was following Callaway's most severe AOA cycle, cycle 9, that the reinsert fuel also exhibited AOA in that subsequent cycle, kind of a unique thing, but because they had such heavy crud loading that it still promoted the boiling taking place.

Some other aspects that utilities might want to use ultrasonic fuel cleaning, if you look at the Callaway case, they have had a progressive reduction in dose rates occurring at their plant on ex-core surfaces since employing this ultrasonic fuel cleaning technology.

Now, that data is somewhat muddled by the fact that they have also reduced the duty of their core there as well, but you are able to remove a whole lot of crud on these assemblies when you're doing this ultrasonic fuel cleaning.

Strategy, elevating constant pH. Crud management is the focus of this. The expected benefits from an AOA avoidance perspective is a reduction in the source term, transport and deposition of corrosion products at the fuel clad.

We do believe that if you are able to raise the pH in your system, say up to 7.3 or 7.4, that is a better regime to be in than, say, at 6.9 or 7.0. And that's based solely on a solubility and thermodynamic standpoint. If proven successful, it may pave the way for similar applications at other PWRs.

Here is the plant that is participating in this demonstration. These are the pH regimes that were used in previous cycles here. You can see -- let's see -- this is pH. So they started out here in this red and blue line early on at 7.0, came up and held constant at 7.2 through the balance of the cycle. And in this current cycle, they started out at 7.3. And they're maintaining that throughout the entire cycle.

We will be performing visual and lift-off measurements of their fuel coming up this fall in their outage.

CHAIRMAN POWERS: When you cite this pH, is that pH that is actually measured in the plant or is that what is measured by sampling and then subsequently corrected to room temperature?

MR. DESHON: These are calculated pH's at temperature.

CHAIRMAN POWERS: At temperature?

MR. DESHON: At temperature, right. These aren't 25 degrees. These are at temperature. Actually, we use TF.

CHAIRMAN POWERS: Okay.

MR. DESHON: Okay. We're looking at zinc addition and how does zinc figure into our AOA prevention strategy. Well, laboratory studies that took place before zinc was added at the boilers or in the PWRs were found to actually reduce the corrosion rate of your ex-core materials, stainless steel and inconel, and to make them more stable so you have less corrosion taking place, less corrosion product released from those materials.

So the end result here is a reduction in corrosion rates; transport; and, therefore, material that is available to deposit on your fuel assemblies.

Something else that we are intrigued by

based on the fuel observations during our crud scrape campaigns, oxide measurements is that the crud deposition pattern in these zinc plants is somewhat different than they are in your traditional chemistry programs, insofar as the corrosion products deposit along the entire length of the fuel assembly and not restricted to just those spans where you have subcooled nucleate boiling taking place.

So what that means to us is perhaps through zinc addition, you don't reach that critical thickness that is necessary to deposit boron in those upper spans.

So we are kind of intrigued from this potential aspect from zinc addition. So why are we demonstrating it at one of our high-duty units? Bottom line is there have been no high-duty units that have added zinc, firstly. So adding significant subcooled nucleate boiling into the equation makes us a little uneasy because we don't know how zinc is going to be affected under that environment.

Additionally, when you add zinc, especially in that first cycle, it will be rapidly consumed by system surfaces. And as it does that, it displaces other transition metals, namely nickel and iron, from those surfaces. And it gets them into reactor coolant. That makes them available then to deposit on the fuel assemblies.

So we take a very cautious approach to zinc injection for this demonstration plant. It's a step increase. Number one, they didn't start injecting zinc until six months into the cycle. Secondly, they added at a very low rate. And once they started detecting zinc in the coolant, they stepped the rate up a little bit. But they're doing it in incremental components so that they don't encounter any problems.

I think I have covered everything with the zinc addition. And my last two slides here, something that we are looking at that is not a demonstration yet, it is still in the research phase, is enriched boric acid.

We have got a program taking place right now at the Halden reactor project. And I have heard that facility mentioned here several times yesterday and today. The project right now is shut down due to some cracking problems in one of their primary pipes. So this activity is not taking place at this moment.

The objectives behind this particular project -- and we have received some DOE co-funding. Glenn is still here. I thought you left, Glenn. We have received co-funding from DOE for this particular project.

The initial objectives for us were: first, we're not even injecting enriched boric acid at the point. Halden needs to be able to demonstrate that they can actually exhibit the symptoms of AOA.

So to do that, they need to show us that they can deposit crud on their fuel rods under prototypical PWR conditions that we have prescribed for them. And, secondly, they need to be able to exhibit those symptoms of AOA.

We are giving them two tries to be able to address those two issues for us in phase I. Test one was not successful. So we have made some program changes for the next test. We're basically throwing everything but a Chevrolet into the reactor coolant system to deposit crud.

If we are successful in phase I, then the phase II objective will be to indeed determine whether or not EBA is a viable chemistry alternative to avoid AOA.

So why are we looking at EBA? A couple of reasons: number one, EBA allows us to optimize our reactor coolant pH so that we don't have to increase lithium above, say, the current three and a half ppm limit that we apply to our utilities.

Now, a utility that has, say, inconel 600 ml anneal tubing might be interested in EBA for that very reason, because they want to optimize their pH. But they are a little concerned that raising the lithium could enhance PWSCC susceptibility. So they can optimize their pH.

The second component here is directed towards the actual deposit mechanism. Because AOA, the boron component to it, is through a concentration process, you have to actually achieve very high concentration factors in order for the boron to actually deposit within the crud. So if you are able to reduce the concentration of your coolant boron, then you're reducing the concentration for the same fuel duty at that clad surface. So you can perhaps delay the deposition process taking place or avoid it altogether. So these are the two reasons why we're looking at EBA.

Because EBA is such an expensive proposition for any utility to undertake because of the capital costs associated, we wanted to verify it through a research reactor before presenting it as a potential strategy for our utilities. So I think that's it.

CHAIRMAN POWERS: One question I should have asked before. Have we characterized the double-layer potential on these fuel rods in the crudded and uncrudded states at all?

MR. DESHON: No. We approached Halden with that too late into getting into this EBA project because they would be best suited to do that. However, it was too late in the game to put in some sort of probe or anything like that, electrode. It's an interesting thought and something that we have considered and may or may not be able to do down the road.

There are some data. Studsvik has generated some data that we're looking at potential as well but not in a crudded environment, just looking solely at what is the potential right there at the rod under standard or prototypical PWR conditions.

CHAIRMAN POWERS: If you happen to have a reference to that, I would like to see it. It would be interesting.

MR. DESHON: Okay.

CHAIRMAN POWERS: The idea is to get rid of this, not to study it, but --

MR. DESHON: Yes. Yes, I know. Yes, I know.

CHAIRMAN POWERS: Any other questions for the speaker?

(No response.)

CHAIRMAN POWERS: Could I propose that we break for lunch and reassemble here at 1:30? So we're recessed until 1:30.

(Whereupon, at 12:48 p.m., the foregoing matter was recessed for lunch, to reconvene at 1:37 p.m. the same day in Closed Session.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

(3:21 p.m.)

CHAIRMAN POWERS: We're back into session. And we're going to hear the sad tale of fuel failing. We've got to improve our schools for fuel. You know, it's because they don't study prior to the test. That's why it is. It's lack of parental involvement. We have sibling rods. There must be parents someplace, right?

MEMBER KRESS: Must be.

CHAIRMAN POWERS: Go ahead, sir.

13) FUEL FAILURES

MR. CHENG: Okay. Thank you. My name is Bo Cheng.

I would like to share with you the fuel failure experiences in the U.S. light water reactors and our root cause investigations into those issues.

I will share with you first the industry fuel failure trend and talk about failure root causes. And I would like to focus on the investigation of crud-induced cladding corrosion failure because that seems to be the current issue.

I will also discuss somewhat the challenges we are facing today. And the last topic will be the BWR water chemistry changes, NMCA, and the fuel performance issues associated with it.

As you know, most of the fuel failure in the U.S. imposes economical penalties on the utilities. They really have never become safety issues, but the cost of fuel failure can range from just a small amount up to -- the worst one I heard was like a $17 million loss.

CHAIRMAN POWERS: Well, you know, the one area where failures may or may not come into the regulatory process is if you believe that iodine spiking in, say, a steam generator tube rupture or something like that is the result of water intruding into a perforated rod. Then it comes into the regulatory process.

MR. CHENG: Okay. In the robust fuel program, we have tried to first work with utilities. When they have fuel failure, some of them do contact us. And we provide all of the technical assistance or is all of the industry database.

We evaluate the root cause with them, join their root cause investigation team. And if there is a need to send a rod to the hot cell, we'll call into the effort with the utilities to perform the work.

I showed you the trend plot already. Just basically the last two years, the BWR side has experienced a substantial fuel failure experience. I think about 50 percent of the 33 BWRs all have experienced a certain degree of failure, some with a lot of rod fail, some with only one failed rod. PWR seems to be more steady.

About the root causes, I'll just show you a typical PWR assembly, but the BWR assembly is somewhat different. There are many different root causes. The first one certainly is associated with manufacturing. Okay?

There are many different manufacturing defects that lead to fuel failure. The most outstanding one is internal hydriding, which was a measure of failure of root cause from the beginning of fuel operation.

Last week I talked to one utility guy. They think they also have just recently experience with moisture failure. So although it's really mostly under control but occasionally we do still experience a little bit of this manufacturing defect, the end plug welding was a big issue in the beginning, many years ago, like three, four decades ago. But mostly it's under control now.

Tube flaw, the low corrosion resistance cladding. We talk about corrosion. There is a lot of corrosion there that would actually cause fuel failure as a common measure of corrosion with others that caused the fuel failure.

Now, we have like missing fuel pellets or damaged fuel pellets. And there are other material defects also. But most of the manufacturing defects have been under control.

The second one is foreign material intrusion. It's actually caused debris fretting. And in the last ten years, both BWR and PWR have implemented this debris filter by the nozzle, by the type plates.

In PWR, it seems to be quite effective, gradually removing this small metallic debris-inducive failure. Often with debris, you've got caught by the spacers. And the fret on the fuel rod caused a fuel failure.

In PWR, it seems to be quite effective; BWR, it's not so sure. The data doesn't conform. You know, we continue to have failure with even debris filter by the nozzle. So we still need to see some more improvement that's need in the BWR side.

The grid-rod fretting -- I think Rosa had mentioned this. This is a vendor-specific issue and many different causes. Grid design is one of the major components that cause grid-rod fretting failure. Particularly, at the core periphery, because of this jetting, you know, the jetting issues cause fuel failure on most of the high burnup rods.

The chairman had a question why we did not inspect, so many rods had never been inspected. That was because utilities believe there's a recurrence of grid-rod fretting in discharged fuel. So they thought they didn't know that was the root cause. Somehow they just don't have time to inspect the discharged fuel.

Then, of course, the next one is local power change due to control blade movement caused the PCI failure, pellet-clad interaction failure. Again, most of it is under control, but I will explain a bit later. We do occasionally continue to have a problem on this mechanism.

The last one is, as I said, the interaction with impurities in the coolant, mostly due to crud interaction with the fuel rod. Again, Rosa showed you this.

MEMBER FORD: Excuse me, Bo. Before you move on to that, --

MR. CHENG: Yes?

MEMBER FORD: -- I noticed there was a

surprise on Kurt's presentation but also on yours. He mentioned they were doing some work on fatigue. And you have shown there that fatigue is not, as I seem to remember it, a major failure mechanism. Is that purely because it's a new material, the high burnup fuels?

MR. CHENG: Yes. It's a high burnup fuel concern. I don't think people attribute any of the failure to the fatigue.

MEMBER FORD: That's right.

MR. EDSINGER: It's a margin issue.

MEMBER FORD: Okay.

MR. CHENG: Okay. If you'll notice here, we do have fuel. This is the one. It's the different failures of root cause. The red one was cladding, corrosion failure, some of it due, probably most of it, to crud-induced corrosion failures. We have quite a few incidents of that. Again, we do have a couple of PCI. I will talk about that.

In the PWR side, you see the red ones, those three crud-induced corrosion failure with the three incidents, the risk of the major issue that has been associated with grid-rod fretting.

The reason I bring this corrosion failure up is that when you have fuel failure, typically most other mechanisms in the last ten years, maybe one or two or three rods fail.

Now, when you have corrosion damage, crud-induced corrosion damage, you could reduce the whole reload. Three units in the last, say, five years essentially lost the whole reload due to the crud-induced corrosion.

I already covered this thing. So I will not go over. Our focus on the crud/cladding corrosion, fuel failure. And if you have an interest on other mechanisms, we can discuss that. I do have backup slides of other issues.

I would like to talk about PWR crud-induced corrosion failure. As I mentioned, we had three cases of crud-induced corrosion failure. It affected mostly the high-power, first-cycle rods in all three cases. So this is not a high burnup issue. This is a high-duty fuel issue.

Corrosion separation was thermally induced in at least one case. I will talk about that. We found it from the hot cell examination. And it occurred in a couple of cases. In two cases, it occurred in fresh-to-fresh fuel assembly faces.

Another case was because the fuel duty was pretty high to get to 4.95 enrichment. And this specific utility did not use so-called intermediate mixing band. That's the IFM grid. Okay? In the new design, because the high-duty fuel added three additional grids in the upper part of the core to improve the MB margin and also to increase the mixing, this specific case, they got better high-duty fuel without IFM grid. So this got this localized corrosion failure.

Out of the three failures, we did hot cell examination of the first case of failure that occurred here in 1995. I will show you those hot cell examination data to show to you how we reached this failure root cause and how this problem can be managed.

In the TMI cycle trend, they got ten failures. The first failure occurred about 120 days into the cycle with nine rods failed at span 6. That is the hottest span, number 6.

In addition to the failed rod, they had many rods. And the damage is by crud. And you can see this is visual appearances of crud-induced damage, the hottest span, number 6.

All the failed rods were once burned. And they failed at the assembly periphery. This observation is very similar to the second failure case. I think APS discussed yesterday briefly observation of those characteristics were very similar.

If you look at a core map, all the failure occurs -- and I'm sorry this is not very clear, but the blank, this white blank, area is the fresh fuel. And in this case, there are four fresh fuels that were classed together in a so-called T-joint occasion. And you can see the symmetrical location.

All the damages were on the fresh fuel, mostly between fresh fuel assemblies. And also in some cases, a fresh fuel assembly is a cold location. For fail rods, it actually occurred between fresh-to-fresh fuel assemblies.

We sent four rods to the hot cell. One of the rods, you can see this is a corner rod of an assembly with four fail rods. And you look at the corner. This is the corner facing away from the assembly. It faced the water gap between two fresh assemblies.

This is a corner rod. You can see on the corner side, you have significant crud-induced damage. The side facing the bundle interior looks very smooth. There was no indication of crud-induced damage.

Similarly, this peripheral rod also shows the surface facing the interior side, showing very smooth surface without any crud-induced damage. It was the surface facing toward the water gap between two fresh assemblies showing very significant crud-induced damage.

In the hot cell, we send the rod to the hot cell, of course. We did quite a few things, looked at neutron radiography, profilometry, gamma scan as a means of burnup distribution.

We took a fuel pellet. We took micro samples from the fuel pellet on the side facing the water gap and also on the side facing the interior. And the reason we do that is because when people look at this kind of corrosion damage, this is a dry-out condition. You have a power distribution problem.

Of course, from a utility perspective, that is a very serious problem. So we need to resolve whether there is a power distribution problem or maybe just crud deposit issues.

So we did quite a few things. The key finding was that we look at azimuthal burnup distribution. And we could not see any variation in the burnup distribution azimuthally. So the conclusion was that there was no unexpected burnup distribution. Inside and outside, it looked essentially the same.

One key observation we found, we cut a sample across the damage location. And we looked at the cladding microstructure here and compared the location behind this damage on the interior side. You can see that there is a clear difference in the cladding microstructure, the grain structure. We crystallized the grain structure.

On the back side, on the interior side, this is cold work structure. This is a cold work stress relief structure. This is in a fabricated condition. So in order to transform this microstructure into this structure, you do need a temperature greater than 450 degrees Centigrade. And so the maximum should be less than the 350 C, even though your structure in the PWR should be less than 350. So clearly the crud has induced a temperature rise significant enough, maybe 100 degrees Centigrade or even somewhat higher to induce the localized corrosion penetration. So that is our conclusion.

MEMBER KRESS: You could check that by some sort of thermal calculation knowing the thickness of the crud? That seems like an unreasonable assumption to me, but, of course, I can't argue with your recrystallization picture.

MR. CHENG: Yes. That's right. That's right.

MEMBER KRESS: But it looks like a thermal calculation of that crud knowing its thermal conductivity.

MR. CHENG: Right. It depends on the type of crud. Actually, the thermal conductivity of the crud all depends on the morphology more than from the type, the chemical composition because the crud, say, it comes as a solid, the solid iron oxide conductivity is better than zirconium by maybe a factor of two to five.

MEMBER KRESS: Yes.

MR. CHENG: Okay. But if so, it depends on morphology. If the morphology is such that it would cause a steam blanketing, then your steam has extremely poor conductivity, maybe two orders of magnitude lower than the --

MEMBER KRESS: I see. It might be a very poorest gap.

MR. CHENG: Right. The crud is so difficult to characterize. And the conductivities all so much depend on the morphology.

So what kind of crud would induce the failure? We did collect some crud samples and did an

X-ray diffraction study. Basically those are either magnetite or nickel ferrite. And we also found nickel oxide.

So this is consistent with what Jeff discussed this morning. It's not unusual crud deposits. We could not determine the surface loading. And we really don't know the morphology of the crud.

We collected some crud samples. And the vendor who handled this initial investigation was not experienced enough, and we lost most of the crud samples because of that.

However, we went back six months later and tried to collect some crud samples. All the crud disappeared in the storage before because storage pool has a pH of about 4.5. So they all disappeared.

Also, a lot of crud will dissolve even during the shutdown. You know, first your boil rate, the water, and then you get into oxidizing conditions. You know, a lot of crud will burst out during the plant shutdown. Something like maybe three or four kilograms will induce from the fuel rod surface.

So typically we really don't know how much crud was on the fuel surface during operation. That's probably the most difficult part for the PWR to quantify how much crud was actually on the fuel surface.

I think the vendor did reload thermal-hydraulic calculation. They really couldn't predict subcooled boiling, even the hot span, because PMI is really the low-temperature PWR. The only difference is that for this plant, they are making a transition from I think 18 or 20-month cycles into 24-month cycles. They do increase enrichment to some extent.

However, no matter how we did, the vendor did a calculation. At EPRI, we also used a VIPRE to do the calculation. We simply could not predict that they could have subcooled boiling in the core.

However, we did some sort of case study. And that says that if you have a subcooled boiling to exist, you will need to have some special hydraulic effect. And that hydraulic effect is to cause some sort of flow loss between the fuel assembly gap, either because of the strip, the side strips, or some sort of raw tripper that causes actually the water loss on the gap.

It's not fully resolved why you get the subcooled boiling in such a low-temperature plant. There is another theory that maybe the water gap, the flow was slower because of the water gap is wider than the interior, which would induce the crud to deposition and led to subcooled boilings because of the cladding process. But this generally is not really credible.

Really, the issue is that we know from -- we did bring in a lot of experts, like Saul Levy, Dr. Levy, to look into the condition. Basically a suggestion is you have to really take the plant data, say you do have subcooled boiling. And so do something there.

So it's becoming more an empirical situation to how you mitigate this problem would be an empirical approach. And so you need to assume when you put fresh-to-fresh assemblies together and cross them together, the thermal hydraulic condition may be more complicated than the model predicts. We really need to take a more conservative approach.

MEMBER KRESS: Does that mean you have to get circumferential variations in your model?

MR. CHENG: Yes, yes. We don't know if there is a power variation, but it put a hydraulic condition there. And in the case I mentioned, when they got to very high enrichment in high-temperature PWR, I think that additional mixing maybe is necessary. So IFM grids appear to be necessary to prevent local hot spots.

Of course, there is a chemistry issue here that we need to avoid total inventory within the pressurized loop. One of the first things is TMI did operate the pH something less than 6.9, maybe 6.7, for the first 100 days. They offer a slightly lower pH. And the reason was that they were confined by the lithium limit of 2.2 ppm. So with high 24-month cycle core without enough persons inside the fuel rod at Westinghouse design, they have this lithium.

In the other design, they rely on gadolinium. That was not sufficient to control the reactivity in the beginning of the cycle. So they start up the cycle with about 1,830 ppm of boron. And that's pretty high. So as a result, they got a somewhat lower pH.

After this experience, of course, they made their boron to about 1,750 or 1,800 as the maximum limit now. Of course, Jeff mentioned this EPRI's recommendation is to try to operate at a constant pH of about 7.2.

So from the data, it seems the higher pH total inventory of crud, iron-nickel crud, should be somewhat less than the low pH condition and should also reduce the crud transportation in the primary system.

So the PWR failure, we had experienced those three failures. We think that the problem, that the root causes were understood. And we think the problem can be contained.

I would like to switch, then, to BWR corrosion failure. BWR is a little bit difficult to contain because the feedwater is a single-loop system. So impurity can easily get into the feedwater into the core, unlike PWR, which is a closed system.

There is no way you can get any impurity into the closed system in PWR except during a shutdown. But BWR, there is a chance that impurity can enter a system through feedwater into the reactor water. And also there are a lot of changes in water chemistry conditions.

In terms of the crud-induced corrosion failure, I think the multiple plant/cycle failures that occur by crud-induced localized corrosion failure that occur starting in 1978 into mid 1980s affected many plants at that time was kind of newer plants, BWR/4 to 6 high-powered plants, those plants equipped with brass condensers and Powdex cleanup systems.

The uniqueness of this Powdex system is not very efficient in removing the soluble species in the feedwater. So the feedwater in those plants would contain somewhat higher soluble species than other types of cleanup systems. There are two types. The other type is called deep bed cleanup system, which can remove the soluble more efficiently.

Well, the problem, so-called CILC failures were mitigated. One of the mitigations was to improve the corrosion resistance of the cladding. And that was introduced in the mid 1980s, about '85 or so. And after that, we had a few fuel failures associated with this type of cladding corrosion failure.

The first one was in 1988, we had one plant with very significant cladding corrosion damage. It affected fresh fuel. Then for ten years, we essentially didn't have this type of corrosion because I guess the mitigation was quite effective.

But in 1998, we had one plant that had a very significant crud, very heavy crud, deposit that caused a failure. This plant again experienced the same type of failure just sometime this year, early part of this year. So we had a repeat of this similar failure this year by similar mechanisms.

Last year, we had two plants: Vermont Yankee and Browns Ferry II. Both experienced fuel failure of second-cycle fuel. And we will discuss this CILC failure mechanism a little bit to give you some sort of background.

We thought we were out of the corrosion-induced failures. Somehow they keep coming back. And I think these CILC failures were investigated very, very thoroughly by General Electric. So I think the understanding is pretty good.

However, the information here we learned I think is applicable probably to some extent to those CILC failure mechanisms, too. So I will talk a little bit on the CILC failure mechanism. Then I will talk about a new type of crud failure. Then I will come back to this more mysterious failure.

MR. CARUSO: The 1998 plant, that is River Bend, right?

MR. CHENG: That's right.

MR. CARUSO: You're saying that was CILC?

MR. CHENG: No. That's different. I mentioned that's a heavy crud-induced failure.

MR. CARUSO: Okay. But not CILC?

MR. CHENG: It's different. It's detailed.

Okay. So I'll give you a brief update on the CILC failures. As you know, CILC failure was due to cladding at a very significant nodular corrosion. And you can see this nodule corrosion, the spots, the nodule corrosion, grew at a rate much higher than the typical protective black oxide.

In this rod, you've got corrosion damage fail. When you look at it in a hot cell, you can see that a failure occurred due to corrosion pitting types of defect. And this is just to show you this metal loss at this location.

MEMBER RANSOM: Has anybody suggested that that may be due to subcooled boiling, you know, sort of the opposite of cavitation? You know, you get collapsing of the micro bubbles and then fatiguing of the metal surface.

MR. CHENG: I think because there is a PWR course, there is a lot of boron. I can see here and show you here, if you look at the location, some rods show nucleus nodule corrosion at about 20 to 40. And then the corrosion went down.

Other rods show the two peaks boiling under the peak here. There you can see here right from each location, this is the place you have vigorous subcooled boiling. It occurred about 30 to 40 at each location. So the boiling occurred on the fuel rod surface. That is to strip off a lot of crud.

I don't know if fatigue was involved, but if you look at a cross-section, it's really metal loss.

So this is kind of puzzling, right? You have a double peak here. I can come back a little bit on that. But, anyway, if you look at the rod in the hot cell, there is a lot of cross-section. There is some very heavy modular corrosion.

If you cut a cross-section, you have a nodular oxide here. A nodular oxide is locally penetrating into the cladding. Between nodules, it's forming this kind of crud deposit here. And there is a heat perturbation.

If you do the heat calculation, you will see because the nodule is getting so close, you start getting really high peaks. The boiling rate was much higher here than on the nodular oxide surface.

When you look at a cross-section exam with a scanning electron microscope, you can see here there is crud imbedded inside zirconium oxide.

This is the EDX. Using this SEM, you can see the zirconium oxide here. And this is Zircaloy cladding. Inside, there is nothing in between. Then you've got this porous deposit with a lot of copper here and also some zinc, and actually also some manganese, some cobalt, some nickel, found mostly all the soluble species in the reactor wall. They got stripped out by the boiling process.

If you look at this cross-section -- I'm sorry the quality is not very good -- you can extend a very large area with zirconium oxide on the outside and on the inside. In between, you have a layer of this type of porous crud embedded there.

So then we come to what then was this structure. Then you will say, "Okay. If you have water come" -- I'm sorry. Outside of this area, there was some sort of fluffy site but removed or lost.

But, you know, under this boiling condition, of course, with the porous crud, you can contain steam, but water cannot come inside. So the conductivity, thermal conductivity of this will be mixed structure of some dry steam in here, taking dry steam, mixing it with a copper-zinc oxide mixture here. And some of it is difficult to measure how much it is.

If you have enough steam pocket here, locally heat will not go through very effectively. So you are bound to have sort of local temperature rises. And with this type of structure, Zircaloy becomes very soluble oxide with growth very fast.

Somewhere about 500 degrees Centigrade,

you can perforate cladding in just a few days at 500 C. with poor nodular corrosion-resistant cladding.

In laboratory tests, this oxide will grow like 10 ml per day for 24 hours. So you can perforate the cladding if you overheated cladding over 400 degrees Centigrade.

So this clad mechanism I believe is well-established through quite extensive study. In the old days, during this investigation, General Electric actually retrieved 12 rods from Vermont Yankee and did very extensive testing, even testing sample for corrosion, for everything in controlled radioactive environment because it was much cheaper to do this type of work a long time ago.

So the solution, mitigation, was to implement is the heat treatment and the belt-polished cladding. So there are two things. You heat-treat the cladding and you finish the surface with belt-polish versus the old cladding, which was not well heat-treated and the cladding was autoclaved.

In addition, many plants re-tubed the brass condensers to remove the copper source. Some of them did not repress the brass condenser. They put a pre-filter for the condensate cleanup to reduce all the impurity sources before the water gets into the feedwater stream. And so, thus, the solution becomes quite effective.

After the implementation, some fuel surveillance programs of those heat-treated cladding show that, actually, you can really eliminate nodular corrosion in the reactor by using this heat-treated cladding. Even before the brass condenser was changed out, many plants show very low or no nodule offset at the change-out.

There are some unresolved issues. The issue is why nodular corrosion or the CILC occurred predominantly to gadolinium fuel rods? A large percentage in the first CILC failures, all the failures occur to the gadolinium rods. But then the subsequent failures start somewhere to also show failure in the damages.

Why does gadolinium play a significant role in the failure mechanism? A second one is why it occurs also at 100-inch elevation. And this is kind of an interesting mystery.

Okay. So that's the CILC failure. Then I want to talk --

DR. MEYER: Could I interrupt and ask a question?

MR. CHENG: Sure.

DR. MEYER: This is Ralph Meyer from NRC.

On the previous slide, you showed heat-treated belt-polished cladding. Do I understand from that that in the mid '80s, they started polishing the surface of the cladding during fabrication?

MR. CHENG: Yes, yes. As a finished product, all the vendors switched to belt polish. Before the mid 1980s, all the vendors used autoclave.

DR. MEYER: Because this is entirely consistent with what we saw with the E110 cladding, which was developing nodular corrosion.

MR. CHENG: Yes, yes.

DR. MEYER: When it was polished, it improved the situation dramatically. Thank you.

MR. CHENG: Right, yes. Cladding surface can be a very major part of the whole corrosion equation.

Okay. Now I will switch to this heavy crud-induced corrosion failure. Again, this first occurred in 1998. We helped the utility a bit. And the data was published in this meeting in year 2000.

The crud was very heavy crud. You can see here in the picture here some of the crud was so heavy that this is about a 40, 50-inch elevation from the bottom. The crud was so heavy, some of them, between the two, they would almost touch each other. So it almost crossed the gap in some of the rods.

It occurred to the first cycle high-power rods. When you brush it off the rod, you see this is opening damage. One thing interesting, you don't see the nodule type of corrosion on those damaged rods. So the detailed mechanism is somewhat different.

The main constituents are iron, copper, and zinc. This is a brass condenser is not a typical CILC plant in that it has a deep bed cleanup system and supposedly is more efficient in removing copper. But, yet, they still have about .2 to .3 ppb of copper in the feedwater. So they still have copper less than the subpipe of copper in the system, but it still has some copper.

And they inject zinc. Okay? Of course, the zinc injection was for dose rate reduction consideration. They inject zinc into the feedwater and enter into the reactor water.

They have a lot of crud deposit. Actually, they have mass balance. And they say that they think they may have some sort of iron intrusion.

So at that time, it was not so clear what really triggered the failure except they believed that maybe they have a significant iron intrusion during the start-up period that caused this iron to go to the fuel rod surface.

But, yet, if you look at the total iron, however, it is really not the highest there. There are other plants with higher iron deposits. Yet, they didn't have fuel failure.

Look at the copper itself was not the highest either. We had a similar copper deposit. We had a similar zinc deposit. However, they're unique in the combination of iron, copper, and zinc. Combined together, they were very unique in the industry.

So there was some sort of belief that they had to do with the copper hideout in tenacious iron oxide crud. This crud was very tenacious. It's very difficult to brush off. So this is somewhat different.

I'm sorry?

MEMBER KRESS: Do you gather that from the clue that the copper and the zinc curves tend to be mirror images of each other? Is that a clue that you --

MR. CHENG: Yes. I think what happened is I will come back when I talk about NMCA. When you inject zinc, the crud can become very tenacious.

Without zinc, crud tends to be very fluffy. I think when you add zinc into the reactor, zinc will interact with iron to form a very tenacious zinc ferrite. Okay?

With the porous tenacious crud, copper found a place to hide out. And maybe that copper plugs up this porous tenacious crud to cause a steam blanketing, too.

Okay. So I will come back to the next slide to --

MEMBER KRESS: The copper and zinc are definitely related to each other. You can see that.

MR. CHENG: Yes, here, certainly.

MEMBER KRESS: Yes.

MR. CHENG: But the source of zinc in this case came mostly from zinc injection, although there is some zinc in the system because brass contains it. But the majority of the zinc came from the zinc injection.

CHAIRMAN POWERS: Yes. I was just looking at the shape of the curves. High copper means low zinc and low copper means high zinc. So they're related. And that gives you a clue as to how they're interacting with each other.

MR. CHENG: Yes. Okay.

MEMBER KRESS: I'm not sure what clue that is.

CHAIRMAN POWERS: Well, I'm not either, but there is a clue there that we need to look at.

MEMBER FORD: The zinc displaces in the magnetite. That is the basis of the GeSiC process.

MEMBER KRESS: And then, therefore, the copper can't get to it.

MEMBER FORD: Yes.

MEMBER KRESS: Yes. That's the clue.

MR. CHENG: Well, I think the problem, they had a similar experience this year. And they're still working on the root cause, the final root cause. So the utility is actually taking the lead.

You know, previously the vendor made it more strong inference in the final root cause and here decided that this is their plant they have to have their voice, their stronger voice. So they took over the root cause investigation and lead the effort.

We are providing them the root cause analysis. And also we are helping them to analyze this tenacious crud, particularly to try to understand the morphology.

In addition to that, the plant already installed this full-flow filter to remove copper and iron. So right now the plant has very low iron and very low copper. So we think the problem should just go away by that.

In addition, I will come back on this. We have requests as to the chemistry guidelines to be revised to limit the zinc, how much zinc you can put into the reactor water.

MEMBER FORD: At less than .4 ppb, you can still control the cobalt 60?

MR. CHENG: Well, that is something that could be a problem for a high-iron plants. Some plants may have to take exception to this limit. For a plant with copper, we like to stick to .4 or even lower.

So I would like to summarize the crud corrosion failure here, historical case, including the high crud-induced case I just mentioned. I think that it's the localized corrosion acceleration will occur when stagnant steam pockets are formed.

There are two ways to form closer from the data details. In the delaminated nodular oxide layer, like we found at the -- due to intrusion of soluble species outside that caused this steam blanketing.

The second case is tenacious crud, where the pores got partially blocked. And we still think and I think this type of conclusion, this we're still working on the cross-sample characterization, but we think the case probably is very similar to what happened at SGHWR in the 1970s is a heavy water, steam-generating reactor.

They had a very tenacious iron oxide crud. And then they got this copper oxide plug up this crud area from the outside. So it created a steam pocket here that the dry steam escaped. So water cannot come in. So you are creating a heat barrier in between.

So we think copper can cause this delamination in the plug of the tenacious crud. And the reason is the copper is the most abundant soluble species. You go in. You've got boiled and precipitate iron oxide plus copper oxide.

However, iron is different. You know, all the species behave differently. Iron when it goes to the BWR core, it precipitates out as a colloid of iron oxide. It's not a soluble. So the position characteristic of iron oxide is very different. Mostly it would occur at the bottom part of the core, like a 20 to 40-inch location in the subcooled boiling regime or most of the insoluble iron oxide will be stripped out. We have a lot of crud data that indicate that to be the case.

So the equation is how about other solubles? We are adding zinc. And zinc is also soluble. So that's the equation that we need to address.

How do we deal with this tenacious crud issue? If we look at the history of all the BWR crud data that we have collected, we can safely say that before zinc injection that was started in 1986, we had seen tenacious crud. Most crud, you brush it, it comes off.

Occasionally in some of the brass condenser plants, you had natural zinc. But a zinc concentration is substantially low such that you don't really form a tenacious crud there.

So only zinc injection in the last many years, we start seeing very tenacious crud formation. Other crud formation characteristics also have to do with the thermal hydraulic condition in the BWR core because it's a two-phase flow boiler.

You have a subcooled boiling at the bottom part of the core up to about maybe 40, 50 inches into about 40 inches. You've got bulk boiling. But that region, there the two-phase flow depends on this film boiling. There is a very thin film on the water, on the fuel while keeping it thermodynamically stable.

If we produce the water film, we can get into the dry-out condition.

But this also could potentially become location for a hideout because water droplets deposit on the fuel rod surface and they'll vaporize as pure steam. So it will leave behind some of the impurities.

So there is some sort of hideout condition here. We talk about nodular oxide. We have got tenacious crud. We also have been looking into this thermal hydraulic-driven type of hideout as a possibility here. So this is an issue, the raw thermal hydraulics. It's not so clear and is a subject that we need to look into further.

The nodular corrosion issue, as we see, fabrication through heat treatment, through proper reduction, we can control the nodular corrosion, clear data to support that, but that's another story.

Sometimes I don't know. For whatever reason, we start seeing evidence of nodular corrosion more recently. And the possibility of some sort of chemical impurity in the water may reduce the nodular corrosion again. However, we really don't know about other species. This is another area we need to work further.

Then the last issue, again, I mentioned is gadolinium. White gadolinium would induce a lot of nodular oxide. So there is something on this issue. However, certainly we can certainly contain those problems. And the key is to make sure that we don't have excessive tenacious crud. We don't have very excessive nodular corrosion.

So that's the corrosion issue. You know, I want to come back to talk about the current issues. That's the corrosion failure I mentioned. Last year we had two plants with cladding corrosion failure. And Rosa also mentioned this earlier today.

We had three plants with PCI-like failures and then in a water chemistry modification. We'll touch on those three topics. The BWR corrosion failure, okay. The two failures occurred at two different plants, both at BWR/4. Both plants were CILC-susceptible, but one of the plants re-tubed the brass condensers. So they don't have copper source anymore. In about 1990, they re-tubed the brass condenser. So they don't have copper.

The corrosion failure occurs at about 95 to 110-inch location. Remember, I showed you the double peak. This is the case. There was no damage at the bottom part. Twenty to 40-inch only occurred in 95 to 110-inch elevation. So this is kind of a new situation.

We see some sort of crud deposit, but there was nothing excessive. You know, visually it looks kind of typical, noting very unusual to speak of. The fuel design is the same. Damages occur predominantly to the part-length rods.

This is fuel rod nine by nine design with a total of eight rods inside a core, which is 4.95 enrichments or is the high peaking rod.

An interesting part that Peter asked me was because both plants had NMCA applications during outage before fuel failures. And in both cases, failure started about six, seven months into the cycle. So the timing was consistent. You know, both plants had failures that started seven months into the cycle.

The chemistry, plant chemistry, is different. As I say, one plant still had copper because of the brass condenser. But they did not have hydrogen water chemistry, no zinc injection.

Other plants had no copper, but they injected zinc. There was a high level amount of zinc injection. And they have hydrogen water chemistry. So the chemistry condition is somewhat different.

The cladding material, the vendor looked into the material. They found some of the failed rods tended to have somewhat lower iron concentration in the alloys, but, actually, they were always in the specification, not really highly distinctive there.

So that all the root cause investigation, initial phase of our investigation, showed that maybe there are quite a few contributors, like water chemistry, like cladding material and the fuel duty, but really cannot pinpoint which one, what are the predominant triggers.

So robust fuel decided to fund this hot cell program to send funds to the hot cell examination. So that's the current situation. We need to do hot cell examination hopefully to reveal the root cause. I want to talk about --

MEMBER LEITCH: How many pins were failed in this situation?

MR. CHENG: Okay. The first plant, the first outage, and they got five pins in the four bundles fail. And they took out all the bundles in the high-power location in addition to the four bundles that failed. So I think it's total. The reload was maybe 200 bundles. They took maybe almost half of it.

The second plant first had a four-bundle failure, four bundles. They took out the four bundles. They restarted right away. And they it kept failing in the rod. So they had a second missed cycle outage, unscheduled outage.

And they took out I think 25 bundles, failed bundles. And they didn't have enough fuel to redesign the core. So they restarted by fabricating new fuel.

So at the end of the cycle, they took out a whole reload. I think in the end, they may have, I think, maybe 40 bundles fail.

So the first plant, if they didn't pick up all the high-powered locations, the bundles, they might fail a lot of bundles. So it depends on how they manage this.

MEMBER LEITCH: Did either of these plants have excursions in their chemistry, like a major condenser tube leak or foreign material?

I had an experience once with lube oil getting into the reactor coolant system. Any of that kind of history?

MR. CHENG: Hydraulic fluid.

MEMBER LEITCH: Hydraulic fluid?

MR. CHENG: Yes. Well, in the first

plant, no, they did not have the hydraulic fluid in leakage. They did not report a condenser failure. So it was they said just burn no more operation. With chemistry, look at chemistry, very typical, very clean.

The second plant, they had the hydraulic fluid intrusion, but it occurred I think the prior cycle. And they were able to isolate. So they don't believe it entered into the feedwater strain.

So it was not so clear, you know. People would say, "Oh, that was the cause," but they were able to isolate it.

MEMBER LEITCH: Yes.

MR. CHENG: Okay. The PCI-like failures, this is in the three BWRs. They used a single product. It's called iron-enriched zirconium liner. As you know, in order to prevent the PCI failure during control rod pole, people used zirconium liner as a soft liner to prevent a PCI.

Because of the zirconium iron liner in the late '80s into the '90s, there are a lot, a substantial number, maybe one-third of the failed zirconium liner rod that suffered a very long axial split. And they lost few pellets since.

So at that time, a vendor introduced a

different kind of liner. They thought the zirconium liner was iron. And with that additional iron, the corrosion resistance of the liner is improved substantially.

So after fuel failure, they believe that the corrosion rate will be much lower. The hydrogen generation inside the fuel rod will be much smaller, lower. So that won't cause damage to the fuel rod.

And so they use that, but somehow they suffer mostly I think at about 15 gigawatt-days. After about 30 gigawatt-days, in that burnup range, many rods fail during power change. However, utilities do stick to these limits. They do have limits. They can pull the control blade type of thing.

One of the pictures show just a hair line. White lines you have to brush the fuel rod to look at it. Several rods showed this type of feature. But, in addition to that, because there is hydride damage at other locations, but, anyway, possible root cause from failure model evaluation, I think they look at three plants as monitor pool-side inspections, most likely failure of root causes, pellet defects due to when the pellets were loading into the fuel cladding, maybe because of chipping, maybe make the rods more susceptible to PCI. That's one possible scenario.

The second one, of course, the possibility of maybe this liner only reaches iron after irradiation because the iron dissolution or those kinds of things may affect this PCI resistance of the liner.

Another thing that the fuel vendor says is, "Well, maybe it's hydride-assisted cracking because you put noble metal into the reactor. And maybe noble metal causes hydrogen absorption." And, therefore, the outer layer of the cladding may accumulate hydrogen and fuel in the PIE changes, that may induce initial cracking of the cladding.

Now, this is the theory that the Japanese seem to be very concerned about this type of hydride-induced cracking from outside propagating into the inside.

Those are three potential failure root causes.

MEMBER FORD: Sorry. These are Japanese reactors?

MR. CHENG: No. This is all domestic.

MEMBER FORD: I'm sorry?

MR. CHENG: These are --

MEMBER FORD: United States?

MR. CHENG: Yes, U.S. So we think most likely it would probably be the first one, maybe petty differences. But, again, I was just looking at the pool-side. How do you tell? There is no way you can tell. So we sent in the fuel rod to the hot cell for the PIE.

MEMBER LEITCH: Was that last picture the inside we're looking at?

MR. CHENG: This is outside. This is the fuel rod you are looking at from outside.

MEMBER LEITCH: From the outside?

MR. CHENG: From the outside. And that is the brushing. So typically you have a reddish outside rod there. You won't see anything. So you brush it. And they were able to see.

I think, in addition to this, there was profilometry, eddy current measurement. And coupled with visual, you are able to see this type of feature.

MEMBER LEITCH: But what's this down on the right here? Is that where the --

MR. CHENG: Oh, I'm sorry. This is just the picture. I chopped off the picture and maybe just the lighting condition.

MEMBER LEITCH: Oh, okay. This is not --

MR. CHENG: Sorry.

MEMBER LEITCH: Okay.

MR. CHENG: So the schedule I want to show you, we are sending the fuel rods by the end of this year in both cases. We are hoping to get the results sometime next year, but we have this robust fuel committee who would actively participate in all the work and review the results.

As soon as we get a result, we disseminate all the results to the utility members so they will be able to get firsthand the results and hopefully help them resolve their issues.

So that is the fuel failure. Now I want to touch on the water chemistry issue. This is the last topic. On the BWR side, unlike PWR, it's almost a uniform chemistry across the whole industry with minor changes in lithium and pH.

BWRs domestically, we have 33 different chemistry conditions. Every plant is somewhat different in all the impurity levels. And if you look at it back in the 1970s, a lot of plants had recent intrusion, condenser failures, a lot of gas, oxygen, nitrogen in their system, carbon dioxide or carbonate measure in the water.

So in the '70s and '80s, it was a major effort to try to clean up the water to make sure that the industry operates with nice clean water. And one indicator is the conductivity. The reason to clean up water mainly was driven by system material cracking.

We keep cracking the piping. And the conductivity, one of the indicators, you've got a lot of soluble and that is a problem. And the soluble sometimes comes from chloride or sulfate. Those are major efforts. And the measure, as I say, is to plug up the condenser tube to make sure the condenser tube doesn't keep leaking.

The second one is the reason the cleanup. The filtration system, the reason often break down and release of the fines, reason fines, into the feedwater, into the reactor and the reason and then a lot of sulfate, a lot of silica.

MEMBER LEITCH: Are there some plants today? It says "current levels." Are there some plants actually operating at .3 ppb copper?

MR. CHENG: Right now we have probably, I would say, maybe on the high end, the cycle average is less than this. It may be .2, but, as I say, sometimes it would go up a little bit higher, .3, .4.

We right now have probably two plants still with brass condenser.

MEMBER LEITCH: Yes.

MR. CHENG: Yes. They would have. Well, actually, including the one that this corrosion-induced failure would be three, but that plant already put a filter in. And they are able to knock the copper way down.

So we have two plants, probably .2, .3 type of range.

MEMBER LEITCH: Yes.

MR. CHENG: If you take a cycle average, I would say .2, but the range. So the iron actually comes down from like a 10 ppb in the feedwater and the maximum maybe 3 to 4 ppb. Okay? And the iron recommended is .25 to 1.5 ppb.

It's interesting, you know. When the plant runs iron below this level, they are saying that the dose rate will start increasing. So they have to inject iron to bring the iron back to .25

So right now nobody is injecting now, but in the last five years, I would say three plants inject iron oxalate into the coolant, try to bring up the iron level. We are trying to reduce the boiling limit so that they don't have to inject iron again. So this is a range we prefer.

But most plants can meet this range now. A fuel plant may be on the high side. Copper, as I say, maybe of them, I would say that 31 plants have less than .1 ppb now and about 2 of them maybe 2 and another one may be .1 or slightly higher.

And zinc, without a zinc injection, the zinc, even with brass condenser, they say zinc in the water is about half of copper in the brass condenser tube. So without zinc injection, maybe it will be about .15. And copper plant may be in this category but without zinc injection. So the water quality is much better now. And I think it does help the fuel performance also substantially.

In 1983, hydrogen water chemistry was first introduced at the Dresden 2. And the purpose was to mitigate stress corrosion cracking of the piping system. Hydrogen addition, at that time the first demonstration was started at about 1.4 ppm added into the feedwater stream. And they're going to the reactor water to suppress oxygen in the piping system. So that was introduced.

We conducted a very significant fuel surveillance program. And we found this was benign to the fuel. We didn't see any problem with the fuel. If there is anything, we think maybe it would be somewhat beneficial to the fuel. It makes the crud very fluffy. So it may be somewhat beneficial.

Zinc injection was introduced in 1986. I think the first plant was Hope Creek. And it was introduced. The vendor suggested the control was in the reactor water, rather than the feedwater.

Feedwater is not really controlled. It's reactor water controlled to 5 to 10 ppb. This can be a problem because if you are running 5 ppb, you are probably at like a .2 ppb of zinc in your feedwater to get this.

If you want to get a 10 ppb, you probably have to increase the feedwater concentration by 10 times in order to get the double reactor water. And most of the zinc will load on the fuel rod surface. So this can be a problem. And right now we tried to mitigate this problem.

Noble metal chemical addition, NMCA, was introduced in 1996. Again, this is to increase the efficiency of hydrogen in stress corrosion cracking mitigation. The theory is once you treated a system with NMCA, you only need a small amount of hydrogen into the feedwater.

You don't have to go to 1.8. You can go down as low as 0.2, 0.3 ppb. I'm sorry. This has got to be ppm in the feedwater to control the stress corrosion cracking by reducing its chemical potential.

I think Rosa showed you this already. I think there was a problem with this. We have to come back. Let me come back. How this works is the platinum and rhodium in chemical form, soluble chemical form, added to the BWR reactor water, it's about 160 degrees Centigrade when the plant is coming down. They injected chemicals into the line for 48 hours to circulate inside the reactor coil for 48 hours.

During that time, platinum and rhodium metal will fold into monolayers on the piping system. However, the majority of the noble metal will deposit on the fuel surface during the treatment because the fuel surface still has a gamma heating and temperature is hotter.

Because of the crud, the effective surface area is much larger than the piping system. So most of it will go down to the fuel surface.

During the application, if you calculate total amount of noble metal added to the core, divided by total reactive fuel rod surface area, you can come down to like a microgram per centimeter^2. And there is a variation.

Some trends had treated very conservatively, like the bowing is much lower, like

10, and others started with like 62. Okay. And right now I will come back to say right now we try to limit to 30, no more than 30, but they do have a large variability here.

This is equivalent to maybe like one to four kilograms of noble metal added to the core during the treatments, about one to four kilograms.

We know that everybody worries about what happened to the noble metal. You put in platinum and rhodium, this is a catalyst that can cause a lot of change in the water chemical reaction.

So the concern is about the accelerated hydriding of Zircaloy, concerned about its crud transport and deposition because as a catalyst, it could cause a lot of deposition. And we also worry about cladding corrosion.

So we did fuel surveillance at the three BWRs by BWRVIP and robust fuel together to do fuel surveillance at three plants. And, of course, also some utilities did their own surveillance and provided data to the industry for the overall assessment. In this process, we also did hot cell examination twice. We examined the condition of the fuel rods.

To just summarize the condition that we observed after the NMCA, we actually saw increased frequency of surface spallation at several plants. And I will show you this picture.

This is one case with a high noble metal injection. And this is before brass. We've got some crud loss, some sort of tenacious crud. This is an indication of the surface texture. This is a thick tenacious crud here. It could be like 20 microns, 30, 40 microns, thick crud there. And in some cases, there is a tenacious crud spall. And you can see all of those spotty surfaces because of that.

We think the spallation, those with tenacious crud may have to do with the zinc injection. One problem was that after they put NMCA into the core, some see the soluble iron start to increase. And they think maybe the dose rate is also increasing because of that. And they start adding more zinc than they used to.

Rosa said at one of the plants they used to have in the feedwater, they used to put like a 0.2 ppb and suddenly after the NMCA, they increased up to about 0.8 to 1.5 ppb. So plants started adding more zinc after they did the noble metal because they probably got compounded by increasing the injection of zinc. And in one case, we saw some increased oxide spallation. Also corrosion is spallation when you get a very high noble metal treatment.

However, from the hot cell work, we found NMCA did not increase hydrogen absorption. After three cycles, we looked at it as doing our own rod. We did not see any increase in the hydrogen injection. So that's a plus in information.

Out of those 28 plants, -- that's 24 in the U.S., 4 in foreign BWRs -- 2 plants in the U.S. experienced corrosion failure. Domestically, 22 had no NMCA-related failures. However, we are not sure that an NMCA was the root cause. As I say, the chemistry will change. The zinc injection was increased. I cannot say it was attributable to NMCA.

But, again, until we found the root cause, it is just difficult to say what to do. So we do have very close committee to oversee this whole NMCA transition. Zinc injection changes.

When we prepare a white paper and we also develop this guideline for the BWRVIP on the fuel issues. And our recommendation was to limit the noble metal loading to less than 30 micrograms per centimeter^2.

I showed you before this some had 50 to 60 micrograms. So we try to limit it to no more than 30. We also ask to limit zinc to less than 0.4 ppb in the feedwater. As we say, up to today, there is no controlling the feedwater. It's all in the reactor water. And we are imposing a limit of zinc in the feedwater. I think some plants may have trouble, but I think they have to review their situations case by case.

We also asked the plant to reduce iron to minimize tenacious crud. And, of course, they need to talk to fuel vendors to assess the overall situation. A very important issue is they should keep the hydrogen available all the time to prevent the crud transport.

Okay. So I touched on several issues. I'll give you a summary. We think the fuel failure induced by manufacturing defects have mostly been under control. Fuel duty and water chemistry failures continue to occur. And we think it will be more challenging, particularly in the BWR side.

The preventive measures, of course, we have robust tests and in-plant demonstration for any changes and technical and experience basis guidelines. We need to continue to fine-tune that. Any duty changes or the peaking for the particular when increased, there was some discussion about how about enrichment of over five percent.

I think, of course, when you increase the enrichment, the first thing, of course, is fuel economics. You want to reduce the batch size. And that would certainly increase the peaking in the whole core. This is something we need to watch out.

Okay. Thank you.

CHAIRMAN POWERS: Thank you. That was a survey of all problems that your noble metal chemistry can cause.

MEMBER FORD: We don't know that. Sorry. I can't say it.

CHAIRMAN POWERS: That was very generous. The other way of putting it is that we don't know that it doesn't cause problems.

Are there any other questions for the speaker?

(No response.)

CHAIRMAN POWERS: Thank you very much. Most interesting stuff we don't ordinarily get to hear about, and it gives us ammunition for harassing Peter for several months now.

And, Rosa, I want to thank you for bringing everybody here and giving us a very interesting talk. This is really fun stuff that you've got here. And it's obvious that you have got more than your hands full with all of these activities going on.

At this point, I think we need to go off the recorded record and discuss what the subcommittee's strategy is for the future.

(Whereupon, at 4:47 p.m., the foregoing matter was adjourned.)