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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

REACTOR FUELS SUBCOMMITTEE

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MEETING

+ + + + +

WEDNESDAY,

APRIL 4, 2001

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ROCKVILLE, MARYLAND

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

PRESENT:

DANA A. POWERS, Chairman

GEORGE E. APOSTOLAKIS, Member

MARIO V. BONACA, Member

THOMAS S. KRESS, Member

WILLIAM J. SHACK, Member

ROBERT E. UHRIG, Member

PRESENT (Continued):

AUGUST W. CRONENBERG, ACRS Fellow

ACRS STAFF PRESENT:

MEDHAT EL-ZEFTAWY

ALSO PRESENT:

MICHAEL ALDRICH

SUDHAMAY BASU

EDWARD BURNS

RYAN T. COLES

MARGARET CHATTERTON

SKIP COPP

RALPH CARUSO

DAVID DIAMOND

GARRY GARNER

RICH JANATI

STEVE LA VIE

RICHARD LEE

EDWIN LYMAN

BOB MARTIN

LARRY OTT

JACK ROSENTHAL

HAROLD SCOTT

UNDINE SHOOP

C-O-N-T-E-N-T-S

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

(8:31 a.m.)

CHAIRMAN POWERS: The meeting will come to order.

This is a meeting of the ACRS Subcommittee on Reactor Fuels.

I'm Dana Powers, Chairman of the Subcommittee.

ACRS members in attendance are George Apostolakis, Thomas Kress, William Shack, Mario Bonaca, Robert Uhrig. We also have the ACRS Fellow, Dr. Gus Cronenberg, attending this meeting.

The purpose of the meeting is to discuss the safety issues associated with the use of high burn-up and mixed oxide fuels. The Subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberation by the full Committee.

Medhat El-Zeftawy is the cognizant ACRS staff engineer for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on March 14th, 2001.

A transcript of the meeting is being kept, and it will be made available as stated in the <u>Federal Register</u> notice.

It is requested that speakers first identify themselves and speak with sufficient clarity and volume so they can be readily heard.

We have receive done request for time to make oral statement from a representative of the Nuclear Control Institute regarding today's meetings.

Do members have any other comments they'd like to make before we enter into today's rather interesting discussions?

(No response.)

CHAIRMAN POWERS: Seeing none of those, then I think we'll just proceed directly ahead, and I'll call upon Dr. Ralph Meyer to begin us in this discussion of some of the most interesting research going on in the Agency.

PARTICIPANT: This is when we figure out if Ralph is a theoretician or an experimentalist.

(Laughter.)

CHAIRMAN POWERS: Know the answer? I know the answer. Dr. Meyer can organize both research and analysis to produce useful outcomes for the regulatory process, right, Ralph?

DR. MEYER: Couldn't have said it better.

(Laughter.)

DR. MEYER: Okay. I have a lot more material later for the second presentation, which is a summary of a meeting that was held recently on the subject of the embrittlement criteria. So I'm going to try and stick with the time period that's been provided here, and that means that I have a couple of slides that I just want to throw on for background so that they'll be in your package, and I didn't mean to dwell on every single slide in the package.

I'm going to spend most of the time talking about the PIRTs and trying to say what we learned from them and what we're going to do about it. If there's a little time left over, then I can talk about the status of some of the various research programs.

CHAIRMAN POWERS: Okay. That would be useful. Because this is a Subcommittee meeting, I'm pretty liberal with the time allotments because there's no other opportunity we have to discuss things.

DR. MEYER: Okay.

CHAIRMAN POWERS: So I'll hold the schedule roughly correct, but if you have things that you think we ought to hear, feel free to tell us.

DR. MEYER: Okay. The first slide is just some background information on the alloys that we'll be talking about. Zircaloy has ten in it. I think everybody knows that. Low tin Zirc is tin with a concentration in the range of 1.2 to 1.4 percent. ZIRLO is like low tin Zircaloy. Add a percent niobium and M5 is zirconium and one percent niobium, more or less.

We will be referring to these off and on throughout the day.

Also I want to point out some of the criteria that we are looking at. We are looking at criteria for postulated accidents. These are the things that were identified in the agency program plan a couple of years ago, and just in general, we have criteria on fuel damage to make sure that the damage is limited and that we don't get uncoolable core geometry.

Specifically for over power events, we have a criterion of 280 calories per gram fuel enthalpy as a limit for a rod ejection accident, which is the big over power event in the PWR that's analyzed.

We have embrittlement criteria in the regulations for the loss of coolant accident. We'll talk about those a lot today.

There are similar limits on fuel damage during dry storage, and these are related to creep deformation and also to peak temperature during the early stages of dry storage.

This work on the fuel damage limits for dry storage is also going on in our program. I don't plan to talk about that today unless I get questions.

CHAIRMAN POWERS: I think I would try to keep the two separate, but it doesn't hurt to parenthetically note if one result relates to the storage issues.

DR. MEYER: Okay.

CHAIRMAN POWERS: Yeah, parenthetically noting where there's overlap is fine, but I don't think I want to go into the storage stuff in great detail right now.

DR. MEYER: Okay.

CHAIRMAN POWERS: Stay with the real stuff.

DR. MEYER: Okay. The safety criteria that we used for all developed for fresh or low burnup Zircaloy clad fuel rods. We believe for many years that low burnup also provided the limiting conditions, but with the movement to the higher burnup fuels and the large concentrations of burnable poisons, you can now see peak powers occurring later, not at the beginning of life, but as late as end of second cycle.

So we have to take a look at the criteria at higher burnups, and of course, we started doing this in a general way some time ago.

The criteria that apply to these situations were also developed for Zircaloy cladding, and in the beginning at least there was an assumption which seemed like a good assumption, that if the advanced alloys improve the performance during normal operation, that it would do so during the accidents as well, and in some cases that may be true. In some cases it might not be true.

But in any event, we are now looking at high burnups and other cladding alloys to try and confirm these assumptions or find other results if that's what happens.

DR. CRONENBERG: Ralph, why did you think that early ripe (phonetic) conditions were more limiting? You didn't have much fission product buildup. You didn't have much corrosion, embrittlement. So what was the original thoughts on that?

DR. MEYER: Yeah. Usually the big actor

is the power, is the linear heat rating of the fuel rod. In a loss of coolant accident, both the stored energy and the decay heat from short-lived species is proportional to the power, and that often dominates other things.

We've been looking for a very long time at things like rod pressure and gap opening at high burnup during normal operation because they also have a fairly significant impact on the conditions during a loss of coolant accident. The gap conductants is a big player.

DR. CRONENBERG: I'm surprised at that view because water side corrosion was an early -- you know, a phenomenon identified early with Zircaloy, when new corrosion was a problem.

DR. MEYER: It's just a historical fact.

DR. CRONENBERG: Okay.

DR. MEYER: I mean, we're not being governed by this point of view at the present time.

DR. CRONENBERG: Yeah.

DR. MEYER: But this is sort of how we got here.

Now, the status of where we are right now is that we have burnups approved to 62 gigawatt days per ton. This is average for the peak rod in the core. In Europe they tend to report their license limits in terms of average for the peak assembly, which is a lower number by about ten percent. So you have to keep that in mind. We are not that far ahead of the rest of the work in our burnup approvals.

And this applies to the three major alloys that are in use at the present time. Specific questions now have been raised about these criteria for postulated accidents. A long time ago we learned from both the Cabri program in France and the NSRR program in Japan that the 280 calorie per gram number that we're using for the reactivity accidents is probably not valid at high burnups.

Oh, four or five years ago we raised question about the effect of corrosion during normal operation, the oxide buildup during normal operation, and how that should be added into the corrosion during a high temperature transient in LOCA in order to compare with the 17 percent criteria and whether there would be some other effects.

And so we've recognized some -- and more recently, the questions that will be addressed heavily in this meeting by the later presenters and then in the summary of the meeting that I'll be describing about the possible effects of niobium on the embrittlement criteria for loss of coolant accident.

So there are now some -- we had general questions about whether we should be looking at the validity of these criteria for high burnups and other alloys. Now we have some specific questions, and we're just continuing a broad approach to this whole thing.

We have in our agency program plan of a couple years ago agreed that we would not ask the industry to do the confirmatory work for the currently approved burnup range, that we would do that ourselves. And that's the big mission in the research program.

So we are specifically addressing all of these criteria, effects of burnup and alloys for burnups up to 62 gigawatt days per ton.

The industry has been told that they will have to do all of those things for the burn-up extensions above that.

In order to try and improve our progress on the work with the NRC's confirmatory obligation, we organized these PIRT panel meetings which I'm going to talk about here.

PIRT is a phenomenon identification and ranking table. You build tables of phenomena that occur during the events that you're studying, and you try and learn something about that by discussing the importance in each of those, of each of those phenomena.

DR. KRESS: Ralph, when you talk about a burnup limit, like the 62 --

DR. MEYER: Yeah.

DR. KRESS: -- that's for the limiting high power assembly?

DR. MEYER: That's average burnup for the peak rod.

DR. KRESS: For the peak rod?

DR. MEYER: That's a peak rod, yeah.

DR. KRESS: Now, what does that translate into for the average burnup of the whole core?

DR. MEYER: Well, it's a lot lower.

(Laughter.)

DR. KRESS: Yeah, I would assume.

DR. MEYER: You know, you go from the peak rod --

DR. KRESS: The peak rod is like 1.4?

DR. MEYER: Mitch Nissley from Westinghouse probably has an answer right on the tip of his tongue.

MR. NISSLEY: These are very approximate.

Mitch Nissley from Westinghouse.

I would say at the beginning of the cycle a reasonable core average burnup would be in the order of 20,000 gigawatts or 20 gigawatt days per metric ton, and that by the end of the cycle they're probably in the low 30s.

DR. KRESS: Okay. That's --

MR. NISSLEY: And that's for a fairly aggressive core design.

DR. KRESS: Okay. Thank you.

DR. MEYER: Okay. The dry storage issue, the dry storage situation is a little different. The task had been proved for fuel burned up to 45 gigawatt days per ton, and we are able in our reactor oriented programs to look at the dry storage conditions. So these are folded into one of the big programs that we're doing.

So we did three different PIRTs, which we refer to together as the high burnup PIRT. One was on the rod ejection accident. For a PIRT activity, you're supposed to assume a very specific sequence, and so in this case, we assumed that the rod ejection accident occurred in TMI-1 with high burnup fuel at hot zero power.

TMI-1 was chosen because it had been used for an international standard problem. There were input decks. We have done extensive analysis in our cooperative work with IPSN in France and Kurchatov in Russia.

So we had a lot of analysis on TMI-1 rod ejection accident, and we chose that as the base case for that PIRT.

For the BWR power oscillations, we chose Lasalle-2, which had some oscillations and a lot of analysis. So, again, there was an analytical base that we could build on, and again, we assumed high burnup fuel in that core.

When we came to the loss of coolant accident, however, we did not pick a specific plant. We didn't even specify whether we were talking about a BWR or a PWR or a small break LOCA or a large break LOCA.

We did, however, have discussions on each of those. We had major presentations given to the PIRT panel members prior to their ranking activity on small break, large break and BWR and PWR. So all of that information was given to the panel members, and in the end, we decided to just go with a generic loss of cooling accident with Zircaloy clad fuel at 62 gigawatt days per ton.

Now, I think last year at this Subcommittee meeting we were already into the PIRTs, and so we had talked about them. I don't go into a lot of detail. We had about 25 fuel experts from all over the place. The approximate sign is not because we can't count to 25, but because it varied from time to time, and we would tend to have a slightly different mix of people for the BWR events and the PWR events.

We held eight meetings, a total of 25 days of meetings. This is really quite a large commitment of resources to this activity. We prepared three NUREG reports, and I think most of you, if you have not seen the reports, you at least have had access to them. They're quite large. They are on the Web, and they're nearly finished. We have final draft versions, which are out electronically to the PIRT panel members for final comment, and our hope is to publish them at the end of this month.

We also have a staff report which I wrote that tries to give our interpretations of what we learned and some suggestions about how we can move forward with that. That is also written up as a draft report. It's not on the Web in its final form. We're trying to decide how to publish that at this time.

However, the three main components of that report are on the Web. They were developed as we went through the PIRT process as little white papers, and they're on the Web, along with the PIRT reports.

CHAIRMAN POWERS: Well, you've been through the PIRT exercise. Would you do it again if you had a similar problem?

DR. MEYER: Probably. It's an imperfect process when you apply it to a mixed situation like this. I think the PIRT process probably works best when you apply it to development of a computer code, like one of the large thermal hydraulics codes, and I believe that was the environment in which the technique was developed.

When you apply it to a more general subject, the we found that we had to be a little bit fast and loose with some of the concepts and a little bit creative in the way that we tried to put it together.

In fact, at these eight meetings that we held, the first three-day meeting was basically written off as one where we just floundered around and tried to figure out how to go forward, and we started over again with the rod ejection accident in the second meeting.

So there's a high cost to this because we had people from the industry, from overseas, from all over the place coming in largely at their own expense, and I don't know how many times you can generate enough interest and enthusiasm to do that.

We are trying again with the source term. It's an important subject, and probably we'll be able to generate the same kind of interest in the source term.

I'm not sure that we could do this every four or five years as a routine matter.

Also, I would say since we're on the subject of opinions, the result of a PIRT ranking by and large are boring. I mean, you list a lot of phenomena and you rank each one as high, medium and low importance with regard to some outcome, and you usually get what you knew at the beginning.

So we got a lot of tabulated results that just summarized what we already knew. The thing about it was that there were for some of us in any event, there were some surprises and some light bulbs that went off, and this just would not have happened without the broad discussion with all of these people in the room.

And I think that's what made it

worthwhile. It also makes it risk because if a light bulb doesn't go off, then maybe you've spent a lot of money and didn't get anywhere.

So let me now try and go through these three PIRTs very quickly. Just for calibration purposes, the rod ejection accident occurs when you postulate to the control rod drive mechanism, brakes, and is ejected from the vessel by the pressure differential.

You get a prompt critical power pulse. In a power reactor the width of the pulse at half maximum is about 30 milliseconds. You get the cladding temperature rise that lags this a little bit. You get a strong negative Doppler feedback due to the power pulse, which basically shuts it down.

DR. KRESS: Now, is this local or --

DR. MEYER: It is local. It's localized to several neighbors around the ejected rod, and so it is not a core-wide event.

DR. KRESS: Not core-wide event.

DR. MEYER: Right.

DR. APOSTOLAKIS: Which one is regulatory guide to 177?

DR. MEYER: One, seven, seven is for the rod ejection accident. It's -- I don't know the exact title, but it's the methods and assumptions for analyzing a PWR rod ejection accident, specifically for that event.

And it has the assumption of 280 calories per gram in that --

DR. APOSTOLAKIS: I'm confused. Don't we have a risk informed guidance on 177 as well?

PARTICIPANT: Seventy-four.

DR. APOSTOLAKIS: Five, six, seven?

PARTICIPANT: Those are the same.

PARTICIPANT: It's 117.

DR. APOSTOLAKIS: Oh, 11?

PARTICIPANT: This is an oldie.

DR. MEYER: Oh, it's very old. I think this was safety guide 77 in the prehistoric time.

DR. APOSTOLAKIS: It's one. But you said interesting things about PIRT, and for years now I've been hearing people talk about PIRT in awe. What's so big deal about it? Why are people so impressed by PIRT? Was K used before?

DR. MEYER: That's a fair question. I think to some extent, I think there is a little over expectation. I've felt this from the beginning and have tried to make the best of it, and I think we have come out pretty well on this one because we learned a lot.

It is not much more than a little bit of organization in a big discussion of a lot of experts. So it's a way of getting experts around to get their opinions in a more or less organized way. That's what it turned out to be for us.

CHAIRMAN POWERS: George, I would say that in this context and having attended one day of one of the PIRT discussions --

DR. MEYER: I hope it wasn't the first one.

CHAIRMAN POWERS: No, in fact, it was the second one, but I think this floundering that you encountered on the first one is typical even among the thermal hydruaulicists when they undertake a PIRT. The first round is always a bunch of floundering because you're asking everybody to get on the same page at the same time, and that's difficult because they come in with different imperatives in which their expectations are.

But it seems to me that when you're struggling to understand how to approach a problem that is calling into question things that are as old as 1.77, and it's not a question of is it 280 calories or 220 calories or 100 calories. Is the whole concept any good or not?

When you're struggling with that, you want to get the best people to look at it and say, yeah, you've thought about all of the things that are likely to be important.

Now, they can be flat wrong because they don't have a great deal of experience working in this regime, but you're confident that you've tapped into as much knowledge as you're likely to have in setting up and planning something.

Now, the idea is that you go on and you do some research and some experiments and things like that, and you're going to learn more about it, but at least you start off knowing what you ought to be looking for.

DR. APOSTOLAKIS: And there is consensus at the end? You said that there is a ranking of high, medium and low, and so on and so forth. Are we at the end of this phenomenon?

DR. MEYER: We had a very large panel, atypically large, and we're told by our panel organizer, Brent Boyack, who's done a lot of these, that typically with the panels on the order of six to eight people, that they do, indeed, reach consensus just naturally on these.

We did not, and we did not attempt to reach a consensus. Instead we voted, and we recorded the votes and the rationales, and so you tend to get a distribution of answer, high, medium, and low, and often there's a sizable majority, and you can go and look and see what the reason was for that and why some other people didn't quite agree with it.

DR. APOSTOLAKIS: That assumes, of course, that everybody's vote is equally important.

DR. MEYER: Well, you know, we even addressed that. We asked the PIRT panel members to vote only when they felt that they had a good basis for voting and that we didn't expect them to vote on every item because we had a range of subjects from analytical to experimental, and so there was some restraint on that.

DR. KRESS: If you had a split vote, 16 --

DR. MEYER: If you had a what?

DR. KRESS: Sixteen of your members voted high and the rest of them voted it low.

DR. MEYER: Yeah.

DR. KRESS: Would that automatically make it high? Is that the way you would have ranked it?

DR. MEYER: What we did in the end was we agreed on some -- I forget what we call them -- but some scoring criteria, and we went back and had a little formula for deciding two things about a particular phenomenon. If it was important and if it was well known because we would address both of those at the same -- you know, in the same discussion, and what you're really looking for are things that are believed to be important and not well known, and those are the items that you ought to focus on.

And the tables are so large that we developed a little formula and put the numerical score in the table. So you could run down the table and pick these out.

And that's exactly what I did in developing this implications report that I prepared, was I went down the tables, and I skimmed off the items that were of high importance and not well known.

You also sometimes find something from the inverse of that. You look for a subject that is not thought to be very important that you might have felt was important, and I have one of those on this list.

DR. KRESS: The final product is you're looking for where you need more research or finally decide --

DR. MEYER: Well, some people would use it that way. What I was looking for was insights on how

I could plan a way to resolve the issue, and it involved doing additional work, but it also involves a method to get there. So that was -- I mean, you could do a lot with the PIRT, and the information is all recorded. So you can do other things as well, but that's what I tried to do with it.

So let me try and move through this now, and you'll look at some of these items here and see that they're perfectly expected results, but not all of us knew all of these things at the outset.

The first one, for example. I have to confess that I saw this as a little bit of a surprise. I always thought that, you know, the energy deposition was just a function of something that could never be changed, and if you went over 280 or 220 or 100, whatever it was, you were just out of luck.

But core designers know that that's not the case. You can design the core. You can put high burnup rods near or far from high worth control rods and do other things.

Another thing where a real light bulb went off had to do with that discussion and with the calculations that David Diamond was doing for us on the rod ejection accident.

We have believed for some time now that

the 280 calorie per gram number should come down in the neighborhood of 100 or 80 calories per gram for high burnup fuel, and so we asked David Diamond to do calculations of a rod ejection accident where he gets 100 calories per gram deposited in the fuel rod.

And so he makes the presentation to the PIRT group members, and somebody asked him what control rod worth did you assume, and he says, "Two dollars."

And you hear a chorus of utility people and others say, "There's no way you can have a control rod worth two dollars and 50 calories per gram, $1.20." Well, maybe.

And so the idea comes up that perhaps for screening a large number of operating reactors, the current ones up to the current burnup limit, that maybe we can do some generic calculations based on some enthalpy limit in the range of 80 to 100 calories per gram, discover something about the core design that you would have to have in order to achieve that energy deposition, and then use those to screen the reactor population.

And if, for example, you have to have two dollar control rod worth, and NRR knows for sure that we don't have two dollar control rod worth out there, then you're done.

DR. BONACA: I have just a question. Did the group discuss the high level objectives that set the --

DR. MEYER: Yes.

DR. BONACA: -- pure enthalpy limit?

DR. MEYER: Yes.

DR. BONACA: I seem to remember in ancient times as you said one of the concerns was challenge to the vessel.

DR. MEYER: Yes, we did, and this is where that first meeting went, and so I probably shouldn't characterize it as a waste of time, but we started out considering the general design criteria.

There are two general design criteria that govern these two event, 23 and 27 or something. I forget the numbers, but one on the LOCA and one on the rod ejection and rod drop accident. And they talk in terms of maintaining coolable core geometries, of pressure pulses that don't damage the vessel more than just a little bit of yielding or something like that.

And for the first couple of days we decided how we could adopt those directly as the high level criteria for the ranking exercise, and a conclusion from that discussion was that was going to be really difficult because neither the codes that we were looking at, nor the experiments we were considering would take you all of that distance.

We were not looking at codes that calculated the coolability of a debris bed, and we were not looking at experiments that would get pressure pulses large enough to threaten a pressure vessel.

And so as a practical matter, we backed down to another level, which seemed to be conservative, but workable, and probably not penalizing in any significant way, and we ended up using a concept of fuel damage with significant fuel dispersal.

So we know that there's going to be some fuel damage, and that's not a problem, but it's the fuel dispersal that's the problem, whether you're in a loss of coolant accident where you fragment the cladding and you lose the structural geometry of the core, you get fuel spilling out or in a very high energy rod ejection accident you actually expel fuel through the cracks.

And so those were things that could be addressed with the codes and the experiments that we were talking about until we settled down to that level, and we used that throughout.

DR. CRONENBERG: I think you might want to respond. Didn't you have a tutorial? Even though these were experts, there were some tutorials on -- by like Phil MacDonald -- on experience, fuel behavior experience for the various accidents; is that correct?

DR. MEYER: Yes, that's right.

DR. CRONENBERG: For each one of these?

DR. MEYER: We tried to do this with each of the PIRTs. We would start out the PIRT discussion with two or three tutorials. Phil MacDonald gave one of them on the reactivity accidents.

David Diamond back here in the audience gave one on the same subject.

Larry Hochreiter gave a couple on PWR loss of coolant accidents.

Jens Andersen from GE talked about LOCAs and also about the power oscillations.

So we had a lot of tutorials. We, in fact, used a court recorder for most of the sessions. We captured the tutorials on transcript, and we took the transcripts and edited the transcripts, send them back to the authors, the presenters for editing, and included a select number of those presentations as appendices in these PIRT reports.

So those tutorials, some of them, are in the PIRTs.

DR. KRESS: Ralph, how many calories per gram does it take to go from normal operating temperature up to fuel melt temperature?

DR. MEYER: It takes -- fuel melting is about 267 calories per gram and normal operating fuel enthalpy is -- it's in the range of 15 or 30. So it takes a lot, 230 or 240 to get to melting.

And you know the technical background here. Originally with fresh materials we thought that you had to start melting something to get some real action, and with high burnup cladding, you see a completely different mechanism come in where the expansion of the pellet against the cladding, which has lost a lot of its ductility results in splits, and you also then have the gassy microstructure of the pellet, which can blow particles out through these splits.

So that's the kind of thing we've see. Well, okay. Some other results of the PIRT was the majority thought that you needed to run tests in the burnup range that you were really looking for because part of the action is in the cladding, but part of the action is in the pellet, and even if the properties of the cladding are dominated by oxidation or hydride distribution, the loading is going to be determined by the pellet, which is affected by burnup.

We talked a fair amount about testing the MOX rods because of plutonium enriched agglomerates. This was a subject where there really wasn't any big change in views because we all knew this going in, and we knew it coming out.

Testing in the right coolant environment, we talked about that before, and that came out highly ranked.

This one is a little bit of a surprise for the reactivity accidents, the PIRT panel members didn't think that the alloy was such a big deal, but this was in the context of did you have to run an integral test like in the Cabri reactor or the NSR reactor. Did you have to run those tests for all different alloys?

And their thought was, no, probably not. As long as you knew the relative mechanical properties, you could extrapolate from some base case, and so, in fact, the cladding alloy was not ranked high, although you might have expected it.

Also, near the end of the discussion of the rod ejection accident, we realized that there may be some of the newer alloys which have so much ductility even at high burnup that they don't fail by this pellet cladding-mechanical interaction, and in those cases, then you would be able to go on up to higher energy depositions before you failed, and that the phenomena that would come into play would be more like the high temperature transient effects in a loss of coolant accident.

And we have some experience with the Russian cladding that showed that. The E110 Russian cladding that was tested in IGR reactor and later with short pulses in the BIGR reactor always shows ballooning type deformation and gas pressure rupture rather than a PCMI, even at 55 or 60 gigawatt days per ton. The stuff is very ductile.

DR. BONACA: I am still surprised that you did all this work and there was no linkage to some high level objectives as discussed before. Two, eighty used to be, if I remember, was a true threshold. If you demonstrated that you were below that, you didn't have to consider effects on the vessel. For example, the pressure pulse that may cause a challenge to the vessel were all issues of coolability, too.

I understand what you're doing. You're

trying to say, well, you know, pragmatically let's go to a lower value. To accomplish what? I mean, it's not clear yet that you have linked a value, whatever value you're searching for, to a high level objective such as coolability or pressure pulse.

And without that, you could always have the industry coming back and saying, "Well, I want to go to 70,000 or 80,000 megawatts per metric ton," and there is no basis for 100 calories per gram.

DR. MEYER: Yeah, yeah. Well, we talked about that, and we decided as a practical matter to tie it to fuel dispersal. If you don't have fuel dispersal, you're not going to have pressure pulses because you won't have a fuel-coolant interaction.

DR. BONACA: Okay.

DR. MEYER: And you won't lose coolable geometry. So we tied it to fuel dispersal, and I think there was a general belief that if you work with an enthalpy level that corresponds to fuel dispersal, that you will always be able to get under that comfortably and won't be penalized.

DR. BONACA: Oh, okay. So you have a linkage to that. I mean --

DR. MEYER: There is. Yes, there definitely is.

DR. BONACA: Because I haven't heard the NUREG so I don't know, but all right.

DR. MEYER: Okay. Now, I can't remember whether I discussed this last year or not. So I'll just go through it very, very quickly, but the idea now to bring some resolution to the reactivity accident is, first of all, to improve an empirical correlation that we have, and you've seen it before. I've stuck it in as the next slide. This is what we call our paint brush slide. It's not really a correlation yet. It's just sort of a failure map of the tests that have been done.

But it's that kind of a plot that we would look at and try and draw some boundary between survival and failure, looking at enthalpy increase as a function of either oxide thickness or some fractional oxide cladding thickness to accommodate different cladding diameters.

DR. KRESS: What do you do with those black dots that are below the line?

DR. MEYER: Yeah. Well, this is kind of reminiscent of NUREG 0630 and the ballooning and rupture data from before. You have to know the personality of these data points to realize that these things ought to be moved up on the plot.

Those were tests in NSRR. They were tested at room temperature. The accident isn't at room temperature. It's at hot zero power, which is pretty hot. It's about 280 or 300 degrees Centigrade. So there's a big ductility.

DR. KRESS: It just tells you you've got the wrong parameters plotting.

DR. MEYER: Well, in the past in the NSRR reactor, they've only been able to test at room temperature because they didn't have a high temperature capsule, but now they're building a high temperature capsule.

And one of the things that we want to wait for are some data from the high temperature capsule because if they can quantify how much too low their room temperature test was, then we have a basis for bringing these up.

Here's another one. This is REP Na-1. This is the very first test done in the Cabri reactor. Then intense discussions going on still to this day.

DR. KRESS: That's the one that got everybody excited.

DR. MEYER: Got everybody excited, and it probably is an anomalous result. I think we understand this one now. The understanding that we believe we have is not universally accepted, but it looks like that the precondition of that fuel rod was at such a high temperature that it caused hydride redistribution that affected the ductility.

We've been looking at that at Argonne National Laboratory and have been discussing it as recently as two weeks ago, a full day meeting, and it's very controversial because this was a pitfall that was recognized.

When they prepared this rod, they realized that they shouldn't take it up too high in temperature before the test and thought they had kept the temperature low enough, and the only thing we can conclude is either their temperature measurement wasn't real good or we just didn't quite understand where this boundary was because it seemed inescapable when you look at the microstructures before and after the test, that the hydrides were redistributed before the test.

DR. KRESS: That's why I thought maybe you had the wrong parameter. Oxide thickness must be a surrogate for --

DR. MEYER: Oxide thickness -- well, it's largely the hydrides that affect the ductility in this temperature range, and the --

DR. KRESS: -- and ductility of the remaining material in the clad or something.

DR. MEYER: You've got a little bit of LOCA thinking coming into that question about the remaining metal thickness. It's --

DR. KRESS: Well, those are only microns, aren't they? Yeah.

DR. MEYER: Yeah. This is the --

DR. KRESS: Pretty much.

DR. MEYER: This is the corrosion. This is the amount that was accumulated during normal operation, and approximately 15 percent of the hydrogen that is released during the dissociation of steam that results in the oxidation. So about 15 percent of the hydrogen that's formed is also absorbed.

DR. KRESS: So it's a surrogate for the amount of hydrogen --

DR. MEYER: That's exactly right.

DR. KRESS: Okay.

DR. MEYER: That's exactly right.

DR. KRESS: Thank you.

DR. MEYER: It's easy to measure the oxide thickness. It's hard to measure the hydrogen concentration. It's a surrogate for hydrogen.

CHAIRMAN POWERS: I guess what puzzles me a little bit about the discussion of REP Na-1 is, okay, these guys tried very hard not to redistribute the hydrogen, but despite their best intentions, they did.

DR. MEYER: Yeah.

CHAIRMAN POWERS: Okay. Does that mean that hydrogen can never be redistributed in a real core?

DR. MEYER: Well, it is distributed in the real core in a very characteristic way because you have a temperature gradient across the cladding and the hydrogen congregates to the cooler outer shell, and this tends to embrittle the rim of the cladding, but leave a lot of ductile material underneath, and when you look at the fracture surfaces, this is exactly what you see.

You see a blunt cracked tip through the hydrided rim, and then a 45 degree shear through the ductile part of the cladding, and what you saw in REP Na-1 was a blunt cracked tip throughout the specimen. It's the only one that looked like that. It's the only specimen that they took the temperature up to 390 degrees Centigrade during preconditioning. All of the rest were kept at much lower temperature.

I don't know if there are any conditions in the reactor that could do that. What we are asking ourselves though is if there are conditions during vacuum drying for storage which could cause this to happen because this redistribution happens when you don't have the normal pellet expanding putting stress on the cladding, and in the storage casks when they dry them, you get -- I don't know the exact numbers, but I've heard them talk about numbers in excess of 400 degrees Centigrade sometimes.

And so I think one of the things that we have fed back from this experience into the dry storage work that we're doing is to look specifically at the ductility of this material after it's gone through a range of vacuum drying conditions, in addition to just looking at the creep rupture, which is what is currently used to get the limits for dry storage.

CHAIRMAN POWERS: The redistribution of hydrogen that you're talking about, it's really an equilibrium phenomenon. It's driving itself from being dispersed hydrides along the grain boundaries into a more coherent hydride to reduce surface area of hydrides.

So, I mean, the hydride redistribution

that you want, I mean, it wants to do this, and it's just a question of whether you have enough temperature and time for that to accomplish.

DR. MEYER: That's right.

CHAIRMAN POWERS: So there's a time-temperature tradeoff here.

DR. MEYER: Right, right.

CHAIRMAN POWERS: And it's not clear to me that you don't have time even though you might have modest temperatures --

DR. MEYER: Yeah.

CHAIRMAN POWERS: -- to accomplish that in a real reactor.

DR. MEYER: Yeah.

CHAIRMAN POWERS: In which case it would not be an anomalous point. It would be characteristic of a point where there had been redistribution of the hydrogen.

DR. MEYER: Well, the only thing I can say is there have been a lot of rods looked at out of the reactor, and they have this characteristic high hydrogen concentration near the OD. They do not look like this one did.

DR. KRESS: The higher burnup implies they're going to stay in there longer.

DR. MEYER: Implies that?

DR. KRESS: Those high burnup rods will stay in there longer and will have more time to potentially redistribute the hydrogen.

DR. MEYER: Yeah, well --

MR. SCOTT: Ralph, also the orientation. I mean there's always hydrogen, but sometimes the orientation of what the hydrides look like --

DR. MEYER: Yeah.

MR. SCOTT: Is that part of it?

DR. MEYER: It certainly can be part of it, but in this case, Hee Chung (phonetic), who is examining this issue, has not made the reorientation a big issue. The orientation of the hydrides is affected by the street that you apply to the cladding when it's hot enough for the hydrides to be mobile, and he's not arguing that they reoriented from circumferentially aligned stringers to radially aligned stringers, which right away will really ruin your ductility.

There just seems to be a redistribution, a sort of homogenization of the hydrides. They are no longer all packed up on the OD, and there are a few radial ones, but it's not predominantly radial.

It just looks like you annealed it and

gave it a chance to relax the highly organized distribution into a more random distribution.

DR. KRESS: Those are predominantly axial. You said circumferential.

DR. MEYER: When you look at them in cross-section, they are stringers around the circumference.

DR. KRESS: They are circumferential?

DR. MEYER: Yeah. So to try and wrap this one up, what we want to do is improve the correlation, to get mechanical properties for all three of these because the correlation is predominantly Zircaloy, and so we have to have the relative mechanical properties of all of these, use our FRAPTRAN code to try and make the adjustment for the mechanical properties differences, and then use the three dimensional neutron kinetics code to do the plant analysis and hopefully relate some enthalpy limit to control rod worth or some other parameters that could be easily used to screen the core.

DR. KRESS: Well, does FRAPTRAN deal with the hydrization of the plant?

DR. MEYER: That's going to be just imbedded in the mechanical properties. The mechanical properties are being measured under the conditions --

DR. KRESS: You'll input mechanical properties.

DR. MEYER: That's right, and the mechanical properties for the reactivity accident, which compared with the LOCA these are low temperature, high strain rate, whereas the LOCA are going to be high temperature, low strain rate.

The mechanical properties for ZIRLO and M5 are going to come from the Cabri program. We have a commitment from ENUSA in Spain to provide a ZIRLO rod for testing in Cabri and a commitment from Framatome in France to provide an M5 rod, along with the permission to do mechanical properties testing on these and provide all of that to the participants in the Cabri program.

And these, there will be one test of each of these in 2002. That's next year, in the sodium loop.

CHAIRMAN POWERS: And one test, and the uncertainty in the outcome is?

DR. MEYER: I'm sorry?

CHAIRMAN POWERS: What's your uncertainty in your outcome when you have one test?

DR. MEYER: Large, but we have -- hopefully we'll have ample mechanical properties measurements, and we'll have other tests. We have all of these other tests with Zircaloy.

I know it's not going to completely satisfy you in terms of the quality of this correlation, but what my proposal is to my office, which is trying to resolve this issue, is that we go ahead in 2003 and try and go through the exercise and see if we get an answer that's favorable.

I think the answer is going to be favorable. This is one where we now have enough information to have a "seat of the pants" idea of where it's going, and hopefully the margin will be enough that we can discuss the uncertainties and see where we are.

The reason for pressing to do this in 2003 is that there's going to be a three-year delay before the water loop starts, and I think it's better for us to go ahead and try and go through the resolution with what we have from the socium (phonetic) loop and from NSRR and hopefully a few tests and a high temperature capsule from NSRR.

We're going to be on a plateau of understanding for at least three years, and so we might as well go ahead and try and go through the exercise, see if we can finish it off, and then when we get to the water loop if we see any surprises, then we'll go back and make an adjustment.

DR. CRONENBERG: So what is it, 2003 you go to the standard review plan and say for 50,000 megawatt days per ton, the enthalpy will be 100 calories per gram and anything less it remains 280 as in the original review plan or what?

DR. MEYER: I can't say that that's what we would do. What I'm saying is that in 2003 that the Office of Research will try and write a paper of some sort that says we have assessed the operating reactors with the current fuel up to the current burnup limit, and we have this database. We think the enthalpy limit -- a reasonable enthalpy limit to use for this is such-and-such. We've done the neutron kinetics calculations. Everything is honky-dory. We have some big uncertainties. There will be some additional work in the future to look for mistakes. Case closed, and --

DR. CRONENBERG: But case closed means we remain with 280 calories per gram?

DR. MEYER: That would depend on how I think NRR wants to handle this, and we haven't had any discussion on that. How you implement this into the regulatory framework is another step. At the moment

I'm just talking about establishing the technical basis to do it.

I would expect during the same time period that the NRR will address the regulatory guidance and maybe even the Office of Research might be asked to do that. I just don't know.

DR. CRONENBERG: There's things on the docket now that are kind of pressing, like the power upgrade for I don't know if it's Commonwealth Edison anymore, but the Dresden, Quad Cities. They're going for 17, 20 percent power upgrades with extended fuel burnup. I think with the new GE design to above 50 or 55, maybe even 62. So where does research come into play with NRR that NRR has to review these applications?

DR. MEYER: Ralph Caruso from NRR wants to answer your question.

MR. CARUSO: I just wanted to make the comment about the power up rates. The power up rates for the BWRs do not involve raising any of the burnup limits above 62,000. They do not involve changing any of the burnup rates for any of the fuel.

DR. CRONENBERG: Okay. I guess it's more on the power oscillations when we get to the BWRs, not this rod ejection, but still I'm sort of seeing how the research falls into near term licensing, licensing amendments.

MR. CARUSO: Well, right now what we're doing is we're following the work that's being done by the Office of Research, and we take it into account as we make our licensing decisions.

But right now none of the power up rates involve any changes to any fuel licensing limits. We've not changed any fuel licensing limits to accommodate the power up rates.

DR. CRONENBERG: So you look at the standard review plan as it is written right now, and that's what you base your review on, the 280 calories per gram. If PWR comes in, what is it? Two, thirty or BWR? It's all based upon the old standard review plan.

MR. CARUSO: The vendors have approved methodologies for their existing fuel designs, and they are going to continue to use those approved methodologies to analyze the behavior of the plants at the higher power levels, and as long as they continue to meet the standards that have been already approved at those higher power levels, we'll find them acceptable.

MR. ROSENTHAL: Yeah, Jack Rosenthal,

Research.

You have to do this very piecemeal. Okay? For the ejected rod, if you say that the limiting ejected rod action is at hot zero power because at hot full power you have far less rods in the core, then the fact that when you are at full power you're going to be running at a higher power doesn't enter into that hot zero power calculation.

Like I said, you just have to piecemeal it through, you know, think it through event by event and what's limiting with.

CHAIRMAN POWERS: It's what I'm still wrestling with a little bit, Ralph, is how one selects the fuel and clad combination that one would test. Grant you you cannot test all conceivable clads, all conceivable fuels, all conceivable degradations of that clad, and you get around that by saying, well, I've got these computer codes that are going to allow me to extrapolate and interpolate within the data set I've got, but the question comes up: which one do I test?

Do you test a representative piece of a rod, or do you test the worst piece of a rod?

DR. MEYER: We have done both, but we're generally focusing now on the worst piece of the rod.

The worst piece of the rod is -- well, the one that we select is the uppermost span between grids where the power is still level. So we don't take the end where you have a big power gradient, but we take the next one. It's from the hottest elevation in the core. It has the highest oxidation on it of the other grids, and those are the ones that we almost always select now.

We had some interesting -- we had three pairs of tests. If you go back and look at both the NSRR and the Cabri test, you can find three pairs of tests were -- Span 5 and Span 3 were tested, and each of those three pairs, the Span 5 failed, and the Span 3 didn't fail. They had exactly the same burnup level, but their oxide thicknesses were quite different.

CHAIRMAN POWERS: Okay.

DR. MEYER: Okay. Can I go on to the --

CHAIRMAN POWERS: Please.

DR. MEYER: -- the next one? I'm a little anxious about the time here.

CHAIRMAN POWERS: Well --

DR. MEYER: But I'll go on.

So the next PIRT that we did was for boiling water reactor and for power oscillations that were not stopped by a SCRAM, and this is an accident for which we do not have clear regulatory guidance, but for which GE has in the past done some analysis and have used the same 280 calorie per gram limit to show adequacy in this analysis.

And that limit probably -- it either suffers from the same problems that it does for the PWR or maybe it's not appropriate at all for this event, and so we just worked our way through this event with some interesting understanding of an event that hasn't been understood very well before, at least from the point of view of fuel behavior.

Just a few basics. The accident that we considered started at about 85 percent power, and the recirculation pumps tripped, and then you got some oscillations and you didn't get a SCRAM. So the oscillations build.

Now, the oscillations come at about three second intervals, and this three second interval, two to four seconds what's seen in all of the analyses that have been done.

It takes about eight seconds for a fuel rod to transfer its heat out. So this is less than the time constant of the fuel rod. So if you look at this part, it looks like the rod ejection accident on a small scale. These little pulses have about 15, one, five, calories per gram in them instead of 50 or 100 calories per gram.

And so they cause the cladding temperature to start warming up, and it starts to cool down, and it warms up again, and pretty soon it gets to a high temperature, and the experts expected that you would get to a point where you would dry out and you would not rewet.

And now you had a transient that looks something like a LOCA transient.

So the opinions and insights that we got from discussing this accident are highlighted here, was nearly a unanimous feeling among the experts that you would not get failure by this mechanical interaction of the expanding pellet pushing on an embrittled cladding because the energy was just too small in that pulse, and by the time you get to the second pulse the cladding is now heated up and it's more ductile, and so forth.

They did expect that you would eventually get a high temperature transient during which you would have oxidation, high temperature oxidation, something like you have in a LOCA, and you might even have ballooning and rupture depending on the pressure in the rod.

The BWRs I don't think tend to run quite as high a differential pressure as the PWRs, but I believe they do use the same liftoff criterion. So there can be a positive pressure differential, but it might be a negative pressure differential.

If you get this kind of high temperature excursion with oxidation, you would get classing embrittlement just like you do in the LOCA. There was a fairly lengthy discussion about what bad things do we have to worry about. Do we have to worry about embrittlement of the cladding? Do we have to worry about melting of the cladding? Do we have to worry about melting of the fuel pellets?

It was decided that we don't have to worry about melting of the cladding or melting of the fuel pellets because you're going to embrittle the cladding at a far lower temperature than those two events, and so what we really have to look at it embrittlement of the cladding.

I did not expect runaway oxidation. We had a number of discussions on that. There doesn't seem to be any magic temperature at which you get some autocatalytic reaction that runs away. It's simply a matter of heat balances, how much heat from the chemical process and how much can you pull away?

And it was not thought that that would be a problem, particularly since we're going to run into our problem at a fairly low temperature. Well, fairly lower temperature means around 1,000, 1,200 degrees Centigrade.

And it was further thought that LOCA-like criteria may be even the LOCA criteria, might just apply to this transient.

DR. BONACA: I assume that this event is bounding with respect to a drop for BWR?

DR. MEYER: We decided to focus on the power oscillations a couple of years ago when we did our little agency program plan Commission paper, and we focused on this as a result of our perception of the risk.

We looked at the probability of occurrence and the risk, and what we know is the power oscillations without SCRAM are a -- I don't want to overstate it, but they're a significant risk contributor in BWR PRAs, whereas the rod drop is not. The rod drop is of very low frequency.

So we just focused on this one. I think that, in fact, a lot of what we learn for the PWR rod ejection accident in terms of fuel behavior and damage limits can be transferred, but not all of it because the Japanese continue to study BWR power pulse events and have recently looked at some high burnup BWR cladding in their NSRR reactor and find unusual behavior that hasn't been seen before that seemed to be related to the bonding between the pellets and the classing, which in the BWR cladding that they were looking at has this soft zirconium liner on the ID.

So, you know, working is going on on things that aren't at the center of focus for some regulatory agency, and we're plugged into it.

DR. BONACA: The reason why I asked it, yeah, was that maybe embrittlement is not the issue if you have that kind of transient.

DR. MEYER: Well, I guess it might not be, but the group of experts thought that that was going to be the issue, and so following that --

DR. BONACA: Even for rod drop? Okay. I just --

DR. MEYER: Well, for this -- well, look. For the rod ejection accident, embrittlement is a different -- it's embrittlement from a different temperature range from a different cause, but it's still embrittlement.

Anyway, now --

DR. SHACK: But you're not proposing to use LOCA type embrittlement criteria for a BWR rod drop. I mean --

DR. MEYER: Not for BWR rod drop.

DR. SHACK: You got rid of that on your frequency argument.

DR. MEYER: Right, right. I think what we tend to do is if BWR rod drops continue to be analyzed, you'd probably use the criteria that emerge from the PWR

DR. BONACA: Okay. Because, I mean, right now still in the FSAR if you were licensing a plant today, you would still have to analyze rod drop.

DR. MEYER: Right.

DR. BONACA: Not necessarily power selection. That's why I was leaving that --

DR. MEYER: Again, this is some decision that NRR would make and that --

DR. BONACA: So you would have to infer an equivalent temperature or enthalpy, the position from the PWRs, and I was intrigued by that process, how you would go from one to the other.

DR. MEYER: I think it would make sense to use the criteria that are developed for the PWR for the BWR rod drop.

DR. BONACA: Okay.

DR. MEYER: Although there may be some differences because of the cladding.

Now, for the power oscillations, we are still lagging behind on attacking this issue. This is the one that we know the least about and that we're doing the least on, but it looks like that resolution of the power oscillation question is going to depend largely on analysis. We're going to have to calculate our way through a high temperature transient and look at dry-out and rewet and cladding oxidation.

We have talked to JAERI, the Japan Atomic Energy Research Institute, about doing some repeated pulse test just to confirm that the pulse part of this isn't playing a role, and hopefully they'll be able to schedule a few tests like that in the next two or three years.

DR. UHRIG: That would be in that three-year reactor that they have?

DR. MEYER: Yes, yeah. We talked at length about the test, and they don't have to do them every three seconds. They might do them every three days. They just do one, leave it in there, raise the temperature up a little bit and do another one, and if you do two or three of these, you can probably see what is going to happen or not going to happen, and so that's the kind of repeated pulse testing that's being talked about for NSRR.

Halden has done a number of dry-out tests, and are interested in doing a test specifically planned for this BWR event. We're trying to help plan that test. I wouldn't say that we're very far along, but the capability is there. The interest in the project, in doing this kind of testing is there, and if we can get our act together and define a good test, I think they will do the test as part of the joint program.

CHAIRMAN POWERS: When you think about these ATWS and the embrittlements that occur, do you think about the ATWS processes?

DR. MEYER: I'm sorry. I didn't understand you.

CHAIRMAN POWERS: The ATWS recovery processes, you know, where you drop the core down and then try to promote mixing by raising the coolant level back up.

DR. MEYER: I'm afraid the only thing that we considered was that some time the process, the oscillations would stop, but we did not look at the process of stopping in any detail.

DR. KRESS: You probably don't do much more oxidizing of the clad.

CHAIRMAN POWERS: It's not the oxidizing that I'm worried about. You know, bring the core down and then bringing it back up to prolonged mixing where you must be putting some sort of forces on the clad.

DR. KRESS: Yeah, looking at forces on it, okay.

DR. MEYER: Yeah, but see, these are exactly the considerations that we're talking about now for LOCA. What are the forces on the rods and how do you cover? And we'll get to that in just a few minutes.

DR. KRESS: It looks to me like, Ralph, with the frequency of these oscillations for BWRs being what they are the only difference between that and the single pulse is just the integrated energy that you put in, other than how you deal with it otherwise, put different forces on it.

DR. MEYER: Well, the second thing is only the first one is going to take place with cold cladding.

DR. KRESS: Yeah, and then you're heating up.

DR. MEYER: And then you're heating up,

and you're less vulnerable to the brittle failure.

DR. KRESS: Yeah. So I think this would be amenable to calculation rather than --

DR. MEYER: Yeah. Well, that's what we hoped, and the code, the code that we're hoping will solve this is a combination of our FRAPTRAN code and a code you might not have heard of before called GENFLO, which is a Finnish, sort of a utility thermohydraulics code that has been coupled in Finland with FRAPTRAN more or less specifically to do this calculation.

Keijo Valtonen, who is known by a number of people here at NRC as the principal person at STUK in Finland who is doing this with support from their laboratory at VTT, and just a couple of weeks ago I was given two reports on the progress of this, and I want to say to you that this is a completely voluntary effort on the part of the Finns. We don't even have a formal agreement with them on this, but we have been working cooperatively with them on a voluntary basis for four or five years.

They're doing actually more work on this than we're doing, and so, you know, if you have any interaction with people from Finland, tell them the research people certainly appreciate this.

MR. ROSENTHAL: Let me just make the point that you know, you use the systems code like RELAP or TRACK to drive a hot channel code, to drive a fuel code in an integrated, you know, sequence and calculation. When we're all done, we're still going to have to sit back and say what do we really know, and we're planning that.

And so that, you know, I mean, it's still a piece of work to do, and we shouldn't be dismissive of it. I mean, we'll do the work, but it's --

DR. CRONENBERG: Can you run fuel codes or do you use still contractors to do most of your FRAPTRAN or can you do it in house now?

DR. MEYER: We do it in house.

DR. CRONENBERG: Okay.

DR. MEYER: I don't want to oversell either the capability of the code or our in house work at this time, but we do run both of the codes, FRAPCON and FRAPTRAN. We are running LOCA scenarios and ATWS scenarios in house and at the lab.

DR. CRONENBERG: And at PARCS is Purdue still doing that or you guys can run that yourself?

DR. MEYER: Gee, I don't know whether anybody on the staff can run it, but David Diamond at Brookhaven is doing the rod ejection calculations for us. PARCS is a Purdue University developed code, but it's run other places, and David runs it at Brookhaven.

MR. ROSENTHAL: And as we speak, we're moving PARCS into TRACK M as an integrated product.

DR. CRONENBERG: So you'll be able to run that in house.

MR. ROSENTHAL: Yes, sir.

DR. MEYER: Okay. I'm well behind now. So let me move on and talk about the loss of coolant accident where we have both embrittlement criteria and evaluation models. EM stands for evaluation models. PCT is peak cladding temperature, and ECR is equivalent cladding reactant. That's the jargon of the LOCA trade.

The PIRT tables for the loss of coolant accident were extremely long, and I only skimmed off a couple of things of interest here. One was it surprised me that these fuel experts who had also some experience with the large system codes -- at least some of them did -- they identified a lot of thermal hydraulic models that were of high importance and not well understood, and these are the traditional thermal hydraulic models that are in our LOCA code.

CHAIRMAN POWERS: You don't even need to

understand the momentum equation.

(Laughter.)

DR. MEYER: So I just had to point that out.

CHAIRMAN POWERS: We're desperate.

DR. MEYER: They also found that for the loss of coolant accident that the cladding type was very important, but the most interesting result of the discussions on the loss of coolant accident was the second bullet where George Hache from IPSN in France got up and gave us a summary of our own U.S. history of the development of the ECCS criteria and reminded us that the embrittlement criteria, these numbers 2,200 degrees Fahrenheit and 17 percent oxidation were, in fact, based on ring compression tests made by Hobson in the early '70s, and that the quench tests were only confirmatory because there had been a lot of discussion about whether the quench tests could reasonably represent the axial forces or other constraints that might be on a fuel rod during the quench.

I should say that another way. The discussion was that the external forces on the fuel rod, whether they come from the quenching process or from some other source, including things like earthquake, could be adequately represented in these quench tests. It was felt that they could not, and so the quench tests were used only as confirmatory tests, and the criteria themselves were derived from these ring compression tests.

Well, I didn't know that, and I think most of the people who knew about the details of the development of these criteria during the ECCS hearings are retired, and we had not planned such a test in our program at Argonne National Laboratory. So the very first thing, you know, as soon as this presentation was made, we knew that we had to modify our program at Argonne where we had only planned quench tests to include some measure of post quench ductility from a test, either a ring compression test or something better than a ring compression test.

So that was the immediate result. There was a sort of delayed reaction to this when in France and in my office we discovered some Eastern European papers from the early and mid-'90s reporting on ring compression tests with the Russian alloy, E110, which is zirconium, one percent niobium, which is very similar in composition to M5.

So all of a sudden light bulbs are going off. Here is some information on a similar alloy that shows a marked reduction in the amount of oxidation that can be tolerated during a loss of coolant accident.

And so this then led to meetings with Framatome and Westinghouse. It led to modification of a conference that had already been planned under the OECD framework, and you'll hear directly from Framatome and Westinghouse on this subject, and then I'll come back and give you a summary of the conference which focused on that subject.

So just quickly to go over some steps in trying to resolve this, we do have a test program at Argonne National Laboratory with what we think of as an integral test or a LOCA criterion test where we take a piece of a high burnup fuel rod with the fuel inside, pressurize it, run it through a LOCA type transient, ballooning rupture, oxidation, cool down, quenching, everything present, and try and look at the results.

We also have a number of separate effect tests in the same laboratory where we're looking in separate measurements of oxidation kinetics and mechanical properties, including now the post quench mechanical properties.

The work started with real specimens last summer when we received the BWR rods from the Limerick plant, and it's slow going. We have done a number of the oxidation kinetics measurements, and I can just give you a qualitative result of that.

Oxidation kinetics seem somewhat faster for high burnup fuel than for fresh fuel. So we get oxidation rates that are higher than Cathcart-Pawel correlation, for example, whereas when we measure for fresh tubing, we can reproduce the Cathcart-Pawel correlation.

CHAIRMAN POWERS: And do you exceed Baker-Just?

DR. MEYER: I'm sorry?

CHAIRMAN POWERS: Do you exceed Baker-Just?

DR. MEYER: I don't think so.

CHAIRMAN POWERS: That's harder to do.

DR. MEYER: Yeah, it would be harder.

CHAIRMAN POWERS: But which in a regulatory world, that's the one that counts.

DR. MEYER: The Halden reactor is also planning to do what we would call an integral test. Take a piece of a fuel rod and run it through a transient.

The principal interest in the Halden

program is to look at the possibility of relocation of fragmented fuel into the balloon section, but it, again, will allow you to look at a lot of things, including oxidation, ballooning rupture.

There are a lot of related studies going on in Japan and in Russia, and our FRAPTRAN code will be used in performing the work, but not in a major way in terms of coming to some resolution of this, unlike resolving the BWR power oscillations, where it looks like our job is going to be to analyze our way through the transient.

In this case, analyzing your way through the transient will be done with the large LOCA codes, and our job is limited to just looking at what the embrittlement criteria and the modeling for oxidation and ballooning and rupture are.

We also are interested in doing the same kind of testing for ZIRLO and M5 cladding, and in fact, in the meetings that were held at the end of February with Framatome and Westinghouse, we asked them if they would cooperate with us on this work and provide the materials, and we'd do the work right at Argonne and involve EPRI in the program at the same time, and so we're kind of waiting for a response on that.

I think that's all I want to say at this time. There are two other slides in your handout. This one is a list of the work that we're relying on. I put NRC in quotation marks here because we don't fund or direct all of these programs. There's a range.

For example, the JAERI program, we neither fund it nor direct the work in it, but we have full cooperation with JAERI on this, and they do provide us with all the information.

Some of these programs we participate in as paying members. The Russian work, we provide a portion of their funding and a lot of the direction of that work, but this is pretty much a list of the research programs on which we will be depending for information on fuel behavior.

CHAIRMAN POWERS: One of the questions that came up in a previous discussion of the Argonne out of pile test was the question of what temperature scenario you put them through to simulate the LOCA.

DR. MEYER: Yeah.

CHAIRMAN POWERS: And do you track some sort of average temperature history or do you try to find the temperature history of a particular rod in those experiments?

DR. MEYER: I'm not sure that the temperatures have been set for this, but our current thinking is to run these integral tests at 1,204 degrees Centigrade. So we would run them up.

We have a linear -- Harold, what is the run-up? Five degrees per second heat-up or is it higher than that?

MR. SCOTT: It's about that.

DR. MEYER: Sud knows the numbers for that.

MR. ROSENTHAL: Let me just offer that we need to be thinking this thing through because the heat-up rate of the evaluation model, large break LOCA, is going to be different from a small LOCA, is going to be different from the best estimate LOCA, and so we need to think it through, and we don't have all of the answers yes.

DR. MEYER: I know that Dana is concerned about some stressed that might be applied on the way up. We have, in fact, focused more on the way down and have given more attention to the cool down part of this because this is when the oxygen and hydrogen and distributing themselves in the alpha phase and in the prior beta phase, and we believe that the cool down conditions are going to ultimately determine what the ductility is, and then you do the test when you're down at a relatively cold temperature. You run it through the oxidation transient, come down, and then the ultimate challenge is near the end down at the low temperature.

So I know that you have for some time asked us to look carefully at the heat-up. We've brought this question up. We haven't found much there to accommodate. You know, if there's something more specific that you can help us with, these conditions have not been set in concrete yet.

CHAIRMAN POWERS: Yeah, my concern is that when we look at an individual rod in one of these scenarios, nearly always -- I can't say always, but frequently -- what you see is the rod heats up, then it cools down, and then it heats on up and hits the plateau, whatever it is.

On the average, if you plotted the core average, it looks like you ramp up to a plateau, sits in a plateau, and then it cools down, but by looking at the individual rod, it's actually going through a fairly complicated scenario, and it does have this cool down period, and it is, indeed, that cooling off that you become most concerned about.

DR. MEYER: I think if it had a cool down period prior to the ultimate cool down and quench following a long period at a very high temperature, then you might have some interesting effects. It was my impression that the ups and downs occurred at a relatively low temperature as you're approaching this high temperature period, and I don't think those would have a very big effect because you still have ductile cladding and a very small amount of the oxidation taking place.

We can continue to --

CHAIRMAN POWERS: Well, I mean, when you do the tests, you're going to have to have some justification --

DR. MEYER: Yeah.

CHAIRMAN POWERS: -- for that, I mean, and what you outlined is probably an appropriate justification, but it would have to be substantiated with something quantitative, the analysis.

DR. MEYER: Okay.

CHAIRMAN POWERS: The heat-up that shows that all of these things are taking place at relatively low temperatures, and they don't go up, sit in a plateau, oxidize for a while, then cool down, then heat back up again.

I think you'll find though --

DR. MEYER: Sud Basu is the project manager for this program, and I'll at least say that we will go back to the project and tell them that we've been reminded of this again, and to make sure that we have either a justification for what we do or we change our says.

DR. SHACK: I mean, I think if you look at it, you know, you're pumping all of this hydrogen in during this oxidation. Then the tricky thing about this thing is as Ralph said. You know, you don't really get the big thermal shock until you've cooled the thing down, in which case, you know, while this thing is hot, it's ductile as hell. It's after you cool it down again that it re-embrittles, and then you hit it with the big thermal shock.

But the embrittlement that you get because you've pumped all of the hydrogen in because of all the oxidation that's occurred at the high temperature and the huge thermal shock that you finally get when this thing re-wets, you know, that really does seem to be the limiting material and stress condition that you're ever going to see. You know, one of these cycles before you haven't pumped all of the hydrogen in. You certainly haven't got a stress that's anything like the re-wet stress.

DR. MEYER: Well, I mean, I understand the argument.

DR. SHACK: You're at the worst --

DR. MEYER: But that's all I ever get is this argument, and I get other people showing me calculations and individual fuel rods that don't seem to be consistent with the argument, and nobody ever coming back to me and saying, "Look. All right. Here's the calculation we've done with our code that we're happy with, and here's how the fuel rods behave, and indeed, the limiting stress conditions are always calculated to be in the quenching."

I mean, you can wave your hands make those --

DR. SHACK: Well, the stress and the limiting --

DR. MEYER: You can make those arguments as long as you want to until you come back and quantitatively show me that that's, indeed, what you expect to be.

The problem with the old scenarios is when we were worried about just oxidation, then sitting at the high temperature plateau was the conservative case. It's not clear now that we're worried about fuel embrittlement that sitting at the high temperature condition is the limiting case.

And how you get there suddenly become important, and making a qualitative argument all the time, I've heard it. I agree it. Now show me quantitatively that that's the case.

MR. NISSLEY: Mitch Nissley, Westinghouse.

We've done a number of calculations with both evaluation model and realistic codes, and I would support the general conclusions that Ralph has offered, and we'd be more than willing to share that information with the staff to help resolve this issue.

I'd also say that some of the higher stress in the cladding during re-wet are really very early in re-wet at the bottom of the core where you've not had much oxidation. It's the higher levels in the core generally we will have a slower cool down and a less severe quench load because there's a lot of precursor cooling as the reflood front progresses up through the core.

But we would be willing to provide quantitative information to the staff to help address this concern.

DR. KRESS: Ralph, the research and the PIRTs deal with three design basis type of accidents. Are you planning an additional PIRT to look at severe accidents and effects on the core melt behavior and fission product release?

DR. MEYER: There's a PIRT that's been organized to look at source term, which is kind of serve accidents.

DR. KRESS: Yeah.

DR. MEYER: And I won't be running that directly, but Charlie Tinkler and Jason Shaperow, who have been involved with the severe accident program, will be conducting that PIRT.

DR. KRESS: So questions about effects on high burnup on core melt and source terms will be addressed later.

DR. MEYER: Yes.

DR. KRESS: So it's not part of this.

DR. MEYER: Yes.

DR. KRESS: The other question I have is has anybody raised an issue of the potential effects of high burnup on the iodine spike and steam generator II rupture accidents? Has that ever been brought up as a potential issue?

DR. MEYER: I can't answer that question.

Jack, can you?

MR. ROSENTHAL: Yeah, in response to the ACRS report, et cetera, we're just now planning out how to take on the iodine spiking issue. So actually it's a very timely comment, and in my own mind you make so much iodine per fission, and it's a question of where is that iodine before the hypothesized event occurs. Is it in the fuel or the gap, or is it already outside in the --

DR. KRESS: I think that is very relevant.

MR. ROSENTHAL: It is probably more dominant than the fact that at higher burnups you'll end up ultimately with some sort of equilibrium iodine concentration. That is the time we have to take it on.

DR. KRESS: Yeah.

MR. ROSENTHAL: A different project.

DR. KRESS: And also the spike is a rate at which things get out of clad, and that's not just a function of where the iodine is. It's a function of what has happened to the clad.

So, you know, it could affect both of those things, but anyway, it's something I think ought to be thought about.

MR. ROSENTHAL: Right.

DR. MEYER: And finally, I just want to mention EPRI's cooperation in the big program at Argonne National Laboratory and to say to you that we finally have the H.B. Robinson fuel rods in a hot cell.

So we have Odelli Ojer at EPRI to thank for a lot of hard work on that, and also John Siphers at CPNL in the end stepped in and was a big help.

So that's all I have right now. I don't know if Med is -- we're really going to be pressed for time. We have a 17 minute video on the Cabri program that Med might show at lunchtime.

CHAIRMAN POWERS: Yeah, I think we're planning on doing that at lunchtime.

DR. MEYER: Or some other time.

CHAIRMAN POWERS: What I want to do now because I don't want to break up the next presentation is go ahead and take a 15 minute break now and we'll come back and listen to the presentation on the assessment of LOCA ductility of M5 cladding, and we can understand better the difference between quench and ring compression test.

DR. APOSTOLAKIS: When are you going to show this video at lunch because I had other -- at the beginning, 12 o'clock or 12:30?

CHAIRMAN POWERS: When I get around to it.

(Whereupon, the foregoing matter went off the record at 10:18 a.m. and went back on the record at 10:33 a.m.)

CHAIRMAN POWERS: Let's come back into session.

Ralph, I have TBD on my speaker for the Framatome testing assessment of LOCA ductility.

DR. MEYER: Garry Garner will give the presentation.

CHAIRMAN POWERS: Okay. So it's actually Garry Garner is TBD. Strange initials.

MR. GARNER: If you like what you hear, it's Garry Garner. If not --

CHAIRMAN POWERS: It's that other guy, right? Good. Good strategy.

MR. GARNER: Well, good morning, gentlemen and ladies. My name is Garry Garner. I am a metallurgical engineer, materials engineer at Framatome ANP in Lynchburg, Virginia, and I will be speaking this morning of the LOCA ductility with M5 clad testing results.

At the end of February, latter part of February, this presentation was given to the NRC staff. We took about three hours and we had about 100 slides.

I've pared that down a little bit for this morning. We had our in-house LOCA man give part of these results, and I gave primarily the mechanical test results at the end. It's just me this morning. I'll, of course, try to answer all of your questions. If I don't know the answer, I'll defer, and we'll get it for you.

I want to stress at the beginning that our primary mission in life is not pure research. Our goal with getting alloy M5 developed and licensed and in reactors is to do those tests that are required by the codes and the criteria and compare the results to Zirc-4.

And you'll see, I hope, this morning that those results compare favorably or are the same in some cases.

The way I would like to proceed through this subject material is to start off with just a very brief review of a couple of things about in-reactor operating experience, not LOCA, but just normal in-reactor.

I want to talk a little bit about the alloy composition and fabrication parameters, and then I want to show you that it is a low oxidizing alloy and that it has a low hydrogen pick-up, and so I'll show you the oxidation curve and the hydrogen curve.

And then that is just as a way to set the table for the LOCA, post LOCA discussion that will follow, and we'll talk about the oxidation tests that we did, the quench tests, and the post quench mechanical testing, and then we'll follow with a brief conclusion and a summary.

So for the in-reactor performance, M5 is a binary alloy primarily of zirconium and niobium. Tin is an impurity in this alloy.

Three things that might differentiate this particular Zirc-1 niobium alloy from an E110 or from someone else's zirc, one percent niobium are we do target iron in this 250 to 500 ppm range for improved corrosion. Oxygen is targeted rather high. The spec limit is 11 to 17. We target it right in here for improved creep performance.

And sulfur. Sulfur is an impurity. It's not called out even in the spec as anything other than an impurity, but what we found -- and if you've kept up with the work of Mr. Sharke (phonetic) and others from Framatome -- we found that a very small change in an impurity element has a fairly dramatic change on macro properties like creep.

So when people talk about M5 being of a similar nature, similar chemistry to other alloys, yeah, on the surface, but on the other hand, very small changes can have very drastic effects.

Thermal mechanical processing also plays a role. There's more to the alloy than just its chemistry. This particular alloy is fully recrystallized, and in the tube making process and in the strip making process also, we do all of the intermediate temperature anneals below the transition, below the 610 transition.

DR. SHACK: While we're at though, I mean, if the sulfur has such a big effect, why isn't it spec then rather than just left to float as an impurity?

MR. GARNER: We found out sulfur, when we were developing the alloy during the creep tests, we were noticing that the thermal creep properties were all over the place with each ingot, and it turned out that some of the raw zircon coming from some of the beaches had an unnaturally higher sulfur content than the others, and some of them were low.

So we did the research. We found out where the knee in the curve was, and now we specify ten to 35 ppm sulfur in our spec.

By the way, we also found that same effect for Zirc-4 to a lesser degree, but I think all of the zirconium alloys are sensitive to that.

So just to make sure that we always get

the right creep properties, the good creep properties, the best thermal creep properties that we can get, we do specify it now between ten and 35. But you won't find that in the ASTM zirc specs.

Again, my point on the mechanical processing, we do the anneals below the transition. We found with this alloy that that makes a marked and significant difference in the microstructure, the appearance of the microstructure, and the stability of the microstructure of the alloy.

If we can go into a LOCA and a post LOCA with the stablest microstructure possible, that's what we want. So it's not only a stable microstructure and a good chemistry. It's not only important in the normal operation. It's important in an accident condition as well.

The two properties that I would highlight this morning are the corrosion, and you've seen these kind of curves before. This curve -- and I apologize. It's hard to read because it is just so small on this viewgraph -- but it's the maximum oxide thickness versus fuel rod average burnup, and you can see that all of the colored dots are M5 data points. They come from a wide variety of reactors, from 14-14 to 17 by 17, and the colors are just differentiating those.

And there is a linear behavior up to a burnup so far of 63 gigawatt days. This is sort of the line through the middle of the Zirc-4 data.

The points that I would make here is that we're getting more and more additional data in the 50 to 60 gigawatt area. We're seeing no increase in the oxidation rate at the higher burnups. The highest oxidation so far has been about 40 microns at 60, 63.

So it is a low oxidizing reactor, and that's important when we start talking about what's the condition of the alloy, when you go into an accident condition.

CHAIRMAN POWERS: If I look at the data points from the 16 by 16 --

MR. GARNER: Yeah, the red ones.

CHAIRMAN POWERS: It looks to me like you could probably convinced yourself as you went out toward 60 you would get the same kind of upturn that you see for Zircaloy-4 based on those data points.

MR. GARNER: I don't really think so. These reactors are different duties, granted. There does seem to be a little bit higher effect in the 16 by 16s. I think the behavior still though is rather linear. I don't see any kind of a two slope upturn like you do see with the Zirc-4 type alloys.

Yeah, when we get more data out here for 16 by 16s, we'll see what that's doing, but so far I would point out that the max oxide there is 41 microns, and only in that point.

So if it does turn up, it's going to turn up at a significantly different rate than these guys are turning up. Hopefully.

Similarly, the hydrogen plot for these alloys, I had hoped to have because the results are going to be given to us in April, some burnups in the mid-50s to almost 60 or right in here, to show you that this linear trend with M5 continues, but you have the hydrogen content MPPM versus fuel rod average burnup here, and there's the Zirc-4.

As you would expect, the source of most of the hydrogen for these alloys to pick up is the metal water reaction that's going on. So you would expect a similar kind of behavior. This alloy has a significantly lower pickup fraction than does Zirc-4, and so we get a flat behavior.

Again, this is going to get important when we talk about how much hydrogen is in the alloy in the event of the LOCA, either at the beginning, middle or end of life.

As you can see on this curve, it's going to be less than 100 if this trend continues out here as we expect it, of course, to do beyond 60 gigawatt days.

So just a summary for just that brief portion of this presentation. It is a low oxidizing alloy. We don't see any increase in the oxidation rate at the highest burnups that we've achieved, which are 63 gigawatt days.

If the alloy is lower in sensitivity, to temperature and rod power, we've seen that it has less, dramatically less response to those kind of duty factors, temperature and power, than do the Zirc-4 alloy.

The low oxidation rate and the low hydrogen absorption, the low hydrogen pickup fraction for this alloy end up with a low hydrogen content at high burnups, end of life burnups.

DR. CRONENBERG: When did M5 go into use? '95?

MR. GARNER: Yeah, it went into just rod by rod demonstration rods in the early '90s. It went into our first batch deliveries were in '98, full batch reloads, and now we're well on the way of delivering those full batches now.

DR. CRONENBERG: And that's all in France?

MR. GARNER: No, no, no, no. We have full batches at North Anna and Oconee at this point and some more being delivered later this year.

Our North Anna reactor burnup is after -- we just finished our second cycle, and we're on our lead assemblies there, and our burnup was 40 to 46 gigawatt.

MR. ALDRICH: Mike Aldrich in Framatome.

I think right around 46 peak rod.

MR. GARNER: I think it was 46, 300 peak rod of gigawatt days. So, yeah, we do have it in the -- the alloys in TMI, North Anna, Oconee.

MR. ALDRICH: Yeah, the full batches that we have are at Davis-Besse, Oconee Unit 1. We're supplying Oconee Unit 2 right now, and at TMI will also be getting a batch this fall.

DR. CRONENBERG: And then for the hydrogen pickup, you take them back to Lynchburg and do your constructive testing there or --

MR. GARNER: I didn't mean to mislead you on that. We haven't done a hot cell within the U.S. M5 yet. Those are planned.

These hydrogen analysis were done from the European exposures, yeah.

Okay. Now, I would talk about the results in the high temperature testing, the oxidation quench test and post mechanical quench test. It was called the CINOG. That was the facility in Grenoble where the work was done, and beyond that I don't even know what CINOG means.

The test matrix for the high temperature oxidation tests were we tested both M5 and Zirc-4. It was a double sided oxidation experiment. Length of the samples, about 20 millimeters.

We tested as manufactured, unradiated cladding, just as received from the cladding from the tube vendor, at temperatures between 700 and 1,400 C.

At 1,200 C. we tested some pre-hydrided cladding, which was pre-hydrided at 200 ppm for the M5 alloy and 200 and 450 ppm for Zirc-4. The reason that we didn't go to the 450 for M5 is for the obvious reason that we're not even going to get 200 possible in normal behavior, plus the oxidation. We're going to show you that in a few minutes. We're not going to get so -- 200 was felt to be very bounding for M5.

We did three oxidation times at each test temperature. To try to get these, you know, you time it, and you try to get 50, 100, and 200 microns per side, and for three samples for each test conditions we're done.

The results of the oxidation testing are presented on this plot. It's a little bit busy, but really the results aren't as busy as it might seem.

On the left is oxide in terms of weight gain, milligrams per centimeter squared, versus the oxidation times square root of seconds, and you'll see a series of lines here.

For instance, in like I say it was 700 to 1,400. At 1,400, Zirc-4 and M5 oxidation kinetics are right on top of each other. If you had the time and inclination to go through this legend, you'll see that at that temperature they're the same. At 1,250 they're the same. At 1,150 they're they same. At 1,100 they're the same.

At 1,050 the Zirc-4 and the M5 are parting company rather dramatically with the M5 having a much lower oxidation kinetic than the Zirc-4.

Now, I didn't draw lines through the data, but the NFI did some independent research on our alloy, on M5, and got the same results, and that's what you see right here. The open triangles are Zirc-4, and the closed triangles are M5. So, again, you're seeing that behavior.

Mr. Lebourhis at the OECD meeting two weeks ago in France presented the results on this curve of another French test at 1,000 degrees saying the same thing.

And then down here at 900 again, the alloys are having the same oxidation kinetic again, 900, 800, and 700. So this area here between 1,100 and 1,050, lower than 1,100 and greater than 900, the M5 alloy is clearly oxidizing at a lower rate. It's the only place in that spectrum that that's happen.

I put the 17 percent for folks that want to think about weight gain in terms of the ECR, the equivalent clad reacted. It's right in there, about 24, 25 milligrams per centimeter squared. So that's about 17 percent ECR.

You can see that we behave better or similar to Zirc-4 at these temperatures. The values are consistent with the literature, and they were verified by independent folks, NFI in this case.

CHAIRMAN POWERS: Do you know why you're slow in the oxidations in the 1,050 to 1,100 degree range?

MR. GARNER: I don't, no.

CHAIRMAN POWERS: There's a phase transition in there someplace, isn't there?

MR. GARNER: Yes, yes. You know, and the alloy -- we know that the chemistry of the alloy has to do with what temperature that phase transition goes in and like that. That's certainly the speculation, but I'm not an expert on that. I don't know exactly why that is, but it's very well documented, and it is confirmed.

DR. CRONENBERG: Do you have the diffusivity measurements at these temperatures, too, that for the two different alloys, oxygen diffusivity measurements?

MR. GARNER: We did not make diffusivity measurements, no.

DR. CRONENBERG: Is there in the literature that show that, yeah, this is all in sync, that there's a phase change, there's a diffusivity change, therefore, there's an oxidation rate change?

MR. GARNER: Right.

DR. CRONENBERG: I mean, is that all --

MR. GARNER: It's all consistent.

DR. CRONENBERG: It's all consistent?

MR. GARNER: Yes, sir, yeah.

Okay. In those results compared with literature results, compared with the correlations, this is the weight gain function again, and in this case one over the reciprocal of temperature. So temperature is going down as you go this way.

We've plotted the Baker-Just correlation with the solid line. The dotted line is the Leistikov correlation, and the points here are the M5 and Zirc-4. The open squares are Zirc-4. The solid, the diamonds are M5, and you can see at the higher temperatures that the data are consistent with each other, and also shows that Leistikov does a fair job of predicting actual data, whereas were conservative to Baker-Just.

At this lower temperature, and this corresponds to about 1,300 degrees C., you see that difference again where M5 and Zirc-4 are behaving differently, and with the lower oxidation kinetic associated with M5.

So we are bounded by Baker-Just in all the encountered configurations, and I think we were surprised that Leistikov does a fairly good job of predicting the real data.

DR. CRONENBERG: Prater-Cartwright was used during -- developed during severe accident program here for Zirc-4. Have you benchmarked anything against Prater-Cartwright for severe accident conditions with the M5 class?

MR. GARNER: We did have a slide in the presentation at the end of February where we showed consistency with the Prater-Cartwright data, yes, and I can --

DR. CRONENBERG: It's less?

MR. GARNER: Yes.

DR. CRONENBERG: Your data is less than what would be predicted by Prater-Cartwright?

MR. GARNER: Yes.

DR. CRONENBERG: Okay.

MR. GARNER: Now, in terms of what we saw --

CHAIRMAN POWERS: Radiation has no impact on these?

MR. GARNER: Excuse me?

CHAIRMAN POWERS: Radiation has no impact on these oxidation rates?

MR. GARNER: I think radiation can be expected to have a small impact on them, yes.

When we looked at the oxide coming from these oxidation tests, at the high time, 1,000 degrees, these were two sided tests, and so in this picture you see the oxide, the base metal to both the alpha and the prior beta, and then the inner layer oxide. This is the mounting, the medium here.

And you see for Zirc-4 that you do have this layer, this flakiness, this layering. It's a trace amount of it, but it is present, and we saw that on both the inner layer and the outer layer of the Zirc-4 samples.

When we looked at the M5 alloy, same magnification, you see the less oxide here in this case. This is the mounting material. This is the base metal, and where all of the etching in these photographs to see the oxide and any flaking.

You see that it is a less, but the important thing is that there is a homogenous barrier there. There are no cracks through it. There are no -- none of these delaminations through it that we saw a slight bit of in Zirc-4 that you're going to see a whole lot more of in the E110 in a few moments. So we didn't see that.

Now, just to put some numbers to these pictures, I thought it might be interesting if on the two sided test you have the external zirconium layer, the external oxide, the internal oxide, and then you have the oxygen stabilized alphas next to both of those, and then in the middle the beta layer.

And for Zirc-4 you can see the expected difference in the thickness of the oxides, both on the inner and the outer, but you can see that the alphas and the beta phase are about the same.

This is interesting because in other results for Zirc-niobium alloys, and specifically the Bohmert paper, he explains in there that he had a hard time differentiating the alpha and the beta, and he couldn't find it.

In a picture that I'll show you in a little bit you can sort of see what he's talking about there.

In this alloy and you'll see it in some other pictures in a little while, those layers are very discernable, and you'll see that in a minute. So those numbers sort of just go with those pictures. That's the magnitude of the thicknesses involved there.

Now, the quench test, the quench test matrix, again, comparing M5 and Zirc-4, double sided oxidation test. Failure was defined as if you put a slight after the quench, if you put a slight over pressure in that and you see some bubbles coming out; that's failure. It's a fairly conservative definition for failure because just a pin hole is a failure under this criteria.

The temperatures tested at were 1,000 through 1,300 degrees C. in 100 degree increments. Again, as manufactured tubing.

At 1,200 degrees C., again, the pre-hydrided samples, 200 ppm for M5 and the higher ppm added for Zirc-4, and generally you did five or more tests to establish where that failure occurs. You test until you get that failure, and so generally that took five or more times.

And then there was post test metallography and hydrogen analysis, which I can show you. The results, just in a nutshell, on this plot you can see that the two alloys in this column, that the temperatures 11, 12 through 13 and the time to failure, and you can see at these higher temperatures they're fairly consistent, the two alloys.

At the lower temperature, the 1,000 degrees, the M5, it took twice as long to fail,a nd you'll see this again on the curve in a moment.

For events of equal duration, alloy M5 seems to be superior to the Zirc-4.

Plotting that up as a function of ECR, we have ECR on the left and temperature on the bottom here. This is the Baker-Just correlation points. I hope nobody asks me why that dips because I sure don't know. It surprises us.

This is the Leistokov correlation points, lower understandably, and uniformly. And then this is our data. We're plotting failure points up here, and as you can see, at the 1,300 degrees temperature, the red line is the 17 percent linking the criterion, and so that's a failure point at 1,000 degrees, and it took four and a half hours to get there.

And this is the last unfailed point, and it took three and three quarters hours. So somewhere between three and three quarters hours and four and a half you fail this alloy, and it looks like it's pretty close to the 17 percent criterion.

It's really for this kind of reason that we think that 17 percent criterion is a decent criterion for this alloy, because it's of no concern until you get to times of failure that are just so ridiculously large that it's no longer interesting.

We measured the hydrogen content for the two alloys. Zirc-4, at these oxidation temperatures, this was, again, the durations of these tests, and you can see that the hydrogen content here -- these are the results of three different measurements, and you can see that they're in the 20s, and they're fairly consistent. M5 might be just a tad lower. It's not significant.

The significance of this chart to me is in some of the Eastern European papers, specifically

Bohmert again, at 1,100 degrees where we're showing 18 to 20 ppm of hydrogen in our oxidation and quench test, that study produced over 400 ppm of hydrogen. Don't know why.

So the results, just a summary of the results. The oxidation and the quench. It's clear that M5 is performing equivalent or superior to Zirc-4. The hydrogen uptake is low. That's clear.

The M5 accident survival is definitely superior to Zirc-4. At temperatures greater than 1,100 they're about -- they're the same. At temperatures less than 1,100, it's surviving up to two times longer than Zirc-4. That's consistent with those oxidation curves and that small band of temperatures where M5 has the greater oxidation resistance.

The oxide itself in the quench and in the oxidation, it's not delaminating. It's not showing any signs of breaking down. It's not cracked or delaminated.

If you use Baker-Just to establish the criteria, of course, M5 always meets it. We do successive oxidation times to achieve -- if you want to get down to 17 percent criterion, it takes a long, long time to get there with a low oxidizing alloy, and again, we agree with the criterion.

Now, in our efforts to license this with the utilities and the power authorities in Germany, they are very aware of the Bohmert paper, and they wanted to see how we did in post quench mechanical testing similar to what he did, and so a year and a half, two years ago, Framatome undertook to do some of those tests.

This was the test matrix. We tested at 1,100 degrees C. We did it for times that would give ECRs from three to 17 percent. This series of tests was a single face oxidation, and again, we used as fabricated M5 and compared it to Zirc-4 cladding.

After oxidation it was water quenched, at which point we did mechanical tests. We did a three point bend test, an impact test, and split ring compression test.

That begs the question. That matrix begs the question: why did you test at 1,100 degrees? And, again, we go back to this chart. We wanted to test in an area where the alloys of M5 and Zirc-4 are oxidizing at a similar rate.

It's not very interesting down here to test M5 because it takes so long to get anywhere close to 17 percent. It's really out of the realm of what we're interested in. So we picked 1,100 degrees, where the two alloys are oxidizing at a fairly steep rate, and they're oxidizing the same. A test in that region might learn something was the thought.

This just briefly was the test rig that we used for that series of tests. It's just a four zone heater with the sample hanging here. This is the little quench tank. The reason I wanted to show this slide is mainly for that little piece of white cotton that's sitting in there. That collects the oxide that falls off of the sample upon quenching, and we wanted to show you the results of that.

So each sample, that oxide was collected and weighed and compared to the weight gain that that sample achieved in its oxidation phase.

DR. CRONENBERG: Were you measuring any hydrogen off-gassing besides hydrogen pickup?

MR. GARNER: No.

DR. CRONENBERG: No?

MR. GARNER: No.

And here are the results of those tests. At 1,100 degrees Centigrade, again, for the longest exposure times, the Zirc-4, these were the weight gains observed, and that was the oxide spalled in grams, and this is expressed as a percentage of the weight gain.

And you can see that with the Zirc-4 because of that slight delamination that we saw, that slight flakiness in that alloy, it's losing a lot on quenching. It's losing between 65 and 80-some odd percent of its oxide, whereas the M5 oxide seems to be very tenacious. It's losing only between two and four percent.

That confirms quantitatively what those pictures were attempting to show qualitatively about the difference in the character of the oxide in M5 and Zirc-4.

Now, the pictures, again, also support those results. This is the Zirc-4 at the high time. On this sample you can see clearly the oxide layer, the alpha layer, the oxygen stabilized alpha layer, and the prior beta, and you see the large greens.

In this picture, and you can see it a little bit here, but more in this picture that was etched specifically to bring this feature out, the oxide is up here and you can't really see it, but this is this alpha area here, and you can see these cracks. That oxide is cracking, and it's breaking down, and that explains the results that we just saw.

Now, in contrast to that, the M5 oxide

looks like this. Again, it's the same kind of picture. There's the oxide, and then there's the alpha, and then the beta below that, and again, over here you can't see the oxide, but you can see this alpha area.

And I guess you have to take my word for it a little bit. Those are not cracks. They're shadows. Most of what they are is this linear distribution of niobium particles.

At these temperatures, what we noticed, and you can see it here, within the matrix of the grains, you see the particles lining up in a linear fashion. That's a microstructure that we specifically prohibit in the alloy for a normal operation, but in a LOCA event, that's what happens.

When you go above that oxygen or alpha-beta transition, you tend to get that, and that's what's going on these, these agglomerations of beta Zirc or beta niobium sitting there.

Again, no cracks, and again, you get that linear distribution.

Now, to compare that with what people have observed in some of E110 alloys, this is a picture that was not in Mr. Bohmert's paper. It is in a Russian report, and I can give you the reference of that if you need that, and in a second, I'm going to show you a quote from Mr. Bohmert's paper where he describes in words what he's seeing here, and other folks have seen this, too.

Again, the stratified oxide, in this case highly stratified. In the Zirc-4 that we looked at a little while ago, it typically had, you know, one of those going through there. This alloy is full of them.

Mr. Bohmert also makes the point that he can't find what's going on in the base metal between alpha and beta. This picture, although probably not optimally etched for that, tends to support that.

The point here is that it's a very stratified and cracked oxide layer, and it has a completely different morphology than M5. In words, Mr. Bohmert said that not at a late stage -- that photograph that I just showed you was taken after like 9,000 seconds -- but Mr. Bohmert and his work said that at an early stage he found the same thing in multi-layer oxide scales formed which tend to flake. We saw that flakiness in the Zirc-4.

And, again, we just didn't see that. We don't see it in M5. We've never seen that kind of a morphology, and in the quench test, you can see that when we weighed the amount of oxide that's falling off, falling off, flaking off, it's not there.

DR. CRONENBERG: I think 110 has higher niobium and higher tin or --

MR. GARNER: I'm going to say that I don't know. Nominally it's the same niobium. Nominally it's a Zirc one percent.

DR. CRONENBERG: I thought it was like two percent.

MR. GARNER: No.

DR. CRONENBERG: No?

MR. GARNER: There are alloys that are two, two and a half, and even Framatome has fooled with those from time to time. E110 is nominal one percent, but as far as their tin, their impurities, their other things, I don't know, and I specifically don't know with respect to the version of E110 that Mr. Bohmert tested back in the early '90s. It could be vastly different from the E110 that's in reactors now for all we know.

DR. CRONENBERG: Did he put in his paper what the --

MR. GARNER: He put the chemistry in there. Yeah, and like I say, it's a nominal one percent.

DR. CRONENBERG: Do they use the one percent now or is it two percent?

MR. GARNER: They use the one percent.

Post quench mechanical tests, the three point bend test was the first one that was done. This is just a picture of the test rig showing the two mandrels with about a nine millimeter rod, tube going through there and pushing down on the center of it.

The maximum deflection that they got on all of these was about seven and a half millimeter displacement off of that line. That's the rig. Did it for M5 and Zirc-4, and that's the results.

And you can see that the Zirc-4 and the M5 in this case are right on top of each other in terms of the displacement versus weight gain. They are behaving similarly in three point bend tests.

The next test was an impact test. I don't have a picture of the test rig for that, but it was like any impact test. It was a tube made with a notch and a hammer coming down, and you're measuring the energy that's absorbed in the material here called resilience joules per square centimeter, again, versus weight gain, and you can see again the two alloys, M5 and Zirc-4 behaving very similarly.

When you look at the fracture surface like you like to do it with impact tests, you notice that the Zirc-4 was a ductile ruptured in the ex-alpha-beta phase and brittle in the oxygen alpha. M5 was essentially the same, just a tad more ductility. Maybe that explains that in the alpha phase.

DR. SHACK: Now, if you did a sort of typical LOCA transient, what would your expected weight gain be?

MR. GARNER: A LOCA transient.

DR. SHACK: Just to calibrate myself on this curve.

MR. GARNER: Yeah. Well --

DR. SHACK: It would be less than 17 percent.

MR. GARNER: Yeah. What we saw in one of these curves back here a minute ago, the weight gain for 17 percent is about 24 milligrams per square centimeter. So on that curve you could see where we would be relative to that.

DR. SHACK: Okay.

MR. GARNER: Yeah.

DR. CRONENBERG: Well, then what's going on between the E110 and the M5 if it's not composition? Was it --

MR. GARNER: I didn't say it wasn't

composition.

DR. CRONENBERG: Okay.

MR. GARNER: In fact, I tried to imply just the opposite of that. The compositions are nominally the same, but what we found out in the development of M5 was very small changes can have very large effects. So it might be something like that. There might be a compositional --

DR. CRONENBERG: And it's not in the annealing process. So --

MR. GARNER: It could very well be. If you don't anneal below the alpha-beta transition, you will not get a stable microstructure. One of our developmental precursors to M5 was we called it 5R, and we even put it in test rods in reactors, and it didn't do as well as M5 does, and that's when we made the change.

What we were doing with 5R was we liked to anneal above that transition because we got better creep properties. What we found out was that that had detrimental effects on some of the local oxidation, specifically oxidations under spacer grids and like that.

DR. CRONENBERG: But it's also time and temperature for annealing and so it's not sorted out then. You said you think it's probably chemistry and trace.

MR. GARNER: All we can speculate is it has to do with the stability of the microstructure. Beyond that I wouldn't care to speculate because, number one, I don't know much about E110. It's not our position in life to compare our alloy to E110. We're trying to compare it to Zirc-4.

Sure, we're as interested and curious as anybody as to why these differences might be, but we've not done any testing on E110. We read what we can read.

What we do know from our own experience is the target of some of these even impurity level chemistry have large effects. We know from our own experience that the thermal-mechanical processing at the tube manufacturer is extremely critical to the stability of the microstructure and in areas of corrosion specifically.

That's why we went from 5R to M5. That 5R microstructure that I was telling you about that has those banded beta niobium particles, M5's microstructure is uniform and stable under irradiation, and that's all a function of that intermediate annealing temperature.

So I wouldn't say that those two things don't have something to do with the differences that we see in E110, but I'm not an expert on E110, and I don't want to stand up here and talk about it as if I were.

DR. CRONENBERG: But M5 is not used for any guide tubes or --

MR. GARNER: Yes, it is.

DR. CRONENBERG: Oh, it is?

MR. GARNER: Yes. We use it for guide tubes, and we have our first spacer grids in lead test assemblies hit Davis-Besse right now. So our intent is to have an all M5 assembly very, very soon.

DR. CRONENBERG: So are you going to show us the irradiation growth properties of M5?

MR. GARNER: I could. It wasn't part of this presentation.

DR. CRONENBERG: I was thinking of the small rod problems that we --

MR. GARNER: Right, right.

MR. ALDRICH: So far the -- this is Mike Aldrich, Framatome again -- the growth data from the guide tube material at North Anna and the LTAs that Garry was referring to earlier at the peak rod burnup of 46,000 we've seen virtually no growth of the guide tube material at all.

MR. GARNER: It's not that much different than the Zirc-4 and that's because the Zirc-4 guide tubes are also fully recrystallized. So that growth function, it's not just totally dependent on the recrystallized versus SRA nature, but it's primarily driven by the structure of the alloy. Recrystallized alloys have a lot less growth than do stress relief in annealed alloys, and that's why the M5 guide tubes, they do grow a little less for other reason, but just a little less.

DR. SHACK: The mechanical tests we're looking at were all done in a single heat of material?

MR. GARNER: Yes, yes. Those tubes were provided from a single lot at the tube vendor.

DR. SHACK: And how do you then set the spec on, say, the iron limits? Is it you're checking the microstructures, that over that fully range -- you know, how do you test the stability of your microstructure, since that seems to be your argument?

MR. GARNER: Right. Lots and lots of tests there. We did a lot of test reactor testing, a lot of out-of-pile testing, autoclaves and things like that.

Every time we tweak something like a

sulfur, like an iron, we went through that whole gamut We wanted to be sure that we weren't buying ourselves some creep property or some growth property or some corrosion property at the expense of something else.

So there's an extensive test base behind those targets for all of those constituents, yeah, and it's a tradeoff. I mean, when you don't have tin in your alloy, you have to get creep properties from somewhere else, and in our case, we've done it with oxygen, and we've controlled it and controlled its uniformity with sulfur and these other things, iron.

So, yeah, that was the whole trick with this alloy. People knew years and years ago that corrosion was going to be good with a niobium alloy. The trick was how do you get there and still have these other properties.

Those ranges were set after lots of testing. The last mechanical test --

DR. CRONENBERG: What this tells me is that it has to be a go slow process when you're talking about these sort of things. When you have small changes in composition it can affect different properties in different ways, and so we had had surprises like control rod insertion problems.

MR. GARNER: Right.

DR. CRONENBERG: The bending of guide tubes, the irradiation growth, things like that, and so that's just a general statement.

You're also saying that small changes in composition can give you a surprise change in mechanical performance.

MR. GARNER: You bet you, and like I said, the development -- and we agree with you -- the development of the alloy was a slow go, and we went through many iterations before we got to M5. Now all of those properties are controlled so that one reactor doesn't get one iron in one oxygen and another guy get another sulfur. Those are all controlled in our specification as you would control these things with any alloy in any specification.

The development of those ranges was slow go, and now we insure the properties like every vendor insures its properties, with its spec. And we agree.

MR. ALDRICH: I might also add, if you were through.

MR. GARNER: Oh, yes, sir.

MR. ALDRICH: As far as the deployment of the alloy and the fuel surveillance section of the SER for M5, we are required to take additional PIE data of things like you're referring to, control rod insertability, as the burnup of the fuel in reactor exceeds higher and higher levels up to the license limit, we are required to take PIE data. So that type of performance would be verified.

MR. GARNER: We do take an awful lot of PIE data.

The last post quench mechanical test that we did was the ring compression test. Again, that's just the rig, and you can see the sample sitting in there waiting to be pushed on. And again, the similar results with Zirc-4, displacement versus weight gain. The alloys are the same.

DR. SHACK: I mean when I look at these things, is this really telling me that if I pump the sort of same amount of oxygen and hydrogen into these alloys, they act about the same?

MR. GARNER: Yes.

DR. SHACK: And the difference really is the rate at which you pump hydrogen into it because of the corrosion properties. When you look at these things, you sort of see the same thickness of the stabilized layers --

MR. GARNER: Yes.

DR. SHACK: -- for a given weight gain?

MR. GARNER: For a given weight gain we

do, and that was different than some folks have seen with other Zirc 1-niobium alloys. We do, and I wanted to show on that one chart that our oxygen stabilized alpha and our retained beta were almost identical to that of Zirc-4.

Now, the reason we're here is Bohmert, and so we plotted our ring compression tests against the same variables that he did, the relative deformation on the left, ECR value across the bottom.

The black line here is sort of the line through his data, which showed the embrittlement at the lower temperatures. This is the line below which you consider the allow brittle. Above 65 you can consider it ductile, and in the middle it's mixed.

What you can see here with the blue, the solid blue, the squares and the open blue squares -- the solids are our results for Zirc-4. The opens are Mr. Bohmert's results for Zirc-4, and you can see that by and large, with the exception of maybe that point, we agreed. This told us that his work was probably pretty good, and he had pretty good control over all of his test parameters because when we tested an alloy that we know was like the alloy that we tested, we got pretty much the same results.

Where we differed was where we compared

his Zirc 1-niobium, which was the alloy E110 of 1992 vintage to our M5, and as you can see, our M5 is right along on the same curve as the Zirc-4, which our other data has supported, and where we differed was that's where E110 came in at 1,100 degrees.

We don't have any inherent quarrel with Mr. Bohmert's work. What we know is that the alloys M5 and that E110 that he tested are apparently very different, and I tried to show you this morning that they're different in terms of the results that we get, the measurements on the mechanical tests, the measurements and the oxides, the oxidation rates, and even what they look like, the morphology of the oxides.

These two alloys, while nominally Zirc one percent --

DR. CRONENBERG: They are not the same.

MR. GARNER: -- they are not the same, and that's what I showed you.

Now, I haven't got the data to win the Nobel Prize yet on why, but they're clearly two different alloys.

So just to conclude just a summary of the post quench mechanical test, we tested in the Bohmert range. We tested at that 1,100 degrees temperature and for the reasons that I tried to explain. That's where the two alloys, Zirc-4 and M5, are oxidizing at the same rate so that you can see what's really going on there with those guys.

We have an order of magnitude less hydrogen uptake than Mr. Bohmert's 110. He was getting 400 at 1,100 degrees. We got 20, and I've showed you that we had a completely different oxide morphology.

And we had no delaminations in our oxide, in our mechanical test. We had similar bend test, similar impact test, similar ring compression test to Zirc-4, significantly better than E110.

We agree with Mr. Bohmert's conclusions regarding the Zirc-4, significant different results though in the two different alloys that we've tested, his E110 and our M5.

Now, just one last slide to summarize the entire high temperature, oxidation, quench, post quench mechanical test results. I hope that I've demonstrated this morning that the M5 in reactor operating performance is clearly superior to the Zirc-4; that our LOCA/post LOCA oxidation rates are equal to or a little bit slower than Zirc-4 and significantly slower in certain temperature ranges.

Our LOCA/post LOCA mechanical performance is equivalent to Zirc-4 essentially.

The performance is acceptable and is equal to or better than Zirc-4 of events of equal duration. For a low oxide it takes an awful long time to get to 17 percent ECR. If you had the ultimately perfectly alloy that didn't oxidize at all, you'd never get there. So some consideration of time has to be taken into consideration, and I think everybody does, and that's why we agree that the 17 percent criterion is valid, if you consider how long it takes to do that.

And, again, with respect to the E110 alloy, our data is completely different.

So that concludes that I had to say.

CHAIRMAN POWERS: Thank you.

Any other comments for the speaker?

Ralph, do we know more about this E110? We're going to learn more about E110.

DR. MEYER: In the presentation that I plan to summarize the meeting that we went to, I have further information on E110 --

CHAIRMAN POWERS: Okay.

DR. MEYER: -- from other laboratories as well.

CHAIRMAN POWERS: Okay.

DR. MEYER: And I'll give you what I have.

CHAIRMAN POWERS: Good, good. Well, thank you.

MR. GARNER: Thank you.

CHAIRMAN POWERS: The next presentation we have is from Westinghouse Electric Company on the ductility testing of the Zircaloy-4 and ZIRLO cladding after high temperature oxidation and steam.

Just for Mr. Garner's benefit we will acknowledge this as a Garner presentation or the previous presentation as a Garner presentation.

MR. LEECH: Good morning. My name is Bill Leech of the Westinghouse Electric Company. I'm also accompanied this morning by Mitch Nissley, who is sitting back and has already responded to several questions.

We're both engineers at Westinghouse. I am a mechanical engineer primarily in the area of fuel rod and modeling and data analysis, and Mitch is also a mechanical engineer with an emphasis on thermal hydraulics, and his primary emphasis is on LOCA modeling and methods development.

Our purpose here is to give you an overview of some of our current work in determining the properties of both Zircaloy-4 and ZIRLO after high temperature oxidation and steam. Again, this is an ongoing program. We started it in late January, early February as a result of some of the information discovered by Dr. Meyer. It's an ongoing program. It has still some time to go to completion, but we do want to give you an update on what we've discovered so far.

Now, just some background, and I'm sure by now you've heard it, but let me repeat it once more. The ductility measurements on Zircaloy oxidized in high temperature steam were used to establish the embrittlement criteria, 10 CFR 5046. And those, in fact, are the basis of the two criteria, of the peak cladding temperature of 2,200 and an ECR limit of no greater than 17 percent.

Now, testing consisted in the early '70s of both quench tests and ring compression tests. However, we were aware of the presentation by Mr. Hache of France, and we went back and thoroughly reviewed the Commission's deliberations, the staff evaluations, and agree with him that these were primarily based on ring compression tests, and quench tests were simply used as confirmatory data.

And the purpose of the criteria was, again, to insure cladding would remain sufficiently intact to assure easily coolable geometry, and as a practical matter, they met that criteria simply by assuring themselves that after the transient was completed, the cladding would retain some ductility. So basically it's a ductility retention after the LOCA.

Now, before we proceed, I'd like to talk a little bit about ZIRLO. ZIRLO is our advanced alloy. It was developed actually starting about 20 years ago, included autoclave tests, extensive tests in the BR-3 reactor in Belgium, and reactor demonstrations here starting in the '80s, and it's up really now to basically full implementation.

There may be several of our reactors that don't have ZIRLO, but there are very few, maybe three or four. Well over 90 percent of our cladding we manufacture now with ZIRLO. That includes both ZIRLO cladding, ZIRLO thimbles and ZIRLO grids.

To date, the peak rod burnups that we've gotten are 70,000. Those are a limited number of rods at North Anna. We have had four assemblies in the V.C. Summer reactor with individual rods that have gone over 66,000.

We have taken extensive in pile measurements both on the growth, corrosion, creep, growth, both axial growth of the rods and the assemblies and lateral growth of the grids. Generally we find that for equivalent corrosion duties, the corrosion is probably 60 percent of what we get for Zircaloy-4. Creep and growth are about half.

So these questions I'm sure you would ask later if I didn't answer now, and that's what our experience has been.

So we do consider it in all ways a much better alloy for normal operation.

DR. UHRIG: One question.

MR. LEECH: Yes, sir.

DR. UHRIG: It's described here as being low tin content. Do you have a number?

MR. LEECH: It is one percent nominal tin.

DR. UHRIG: What?

MR. LEECH: One percent nominal tin, yes.

DR. UHRIG: One percent.

MR. LEECH: Again, we started licensing this in 1991. The firm formal licensing process was initiated, and there was an extensive testing program that supported that included material mechanical properties, density, thermal expansion, thermal conductivity, specific heat, phase changes, high temperature creep, high temperature oxidation at rod burst. Plus there was an extensive irradiation program in the BR-3 reactor.

And our conclusion was there were some phase change characteristics because of the composition. The phase change from alpha to beta takes place at a lower temperature. I don't recall the exact number. I believe about 75 degrees Centigrade. So it is a lower phase change.

Other than that, we found that the mechanical properties were essentially identical.

DR. CRONENBERG: Did you show any changes in creep with sulfur, too?

MR. LEECH: Creep? That becomes a complicated question because creep is a function of both thermal creep and in reactor radiation induced creep.

DR. CRONENBERG: But I'm just thinking of the presentation before where he said sulfur affected their creep.

MR. LEECH: We did not make any attempt to see if sulfur had an effect on creep. The overall in reactor creep is lower.

Now, as I say, that gets complicated because that doesn't necessarily mean the thermal creep. Out of pile thermal creep is lower. The two components really interact, and we find must less irradiation creep.

So the overall in reactor creep rate is much less.

DR. CRONENBERG: What do you have tech specs on for trace elements?

MR. LEECH: I can't answer. I don't recall all of those. I mean there's a long list of them, but I can't remember them. I can supply them for you if you'd like.

DR. CRONENBERG: I'm just curious because, you know, prior indications indicated that they make --

MR. LEECH: Yes. I simply can't recall them.

So because we saw that the mechanical properties were essentially identical during the licensing process, we argued that because of the close similarity of Zircaloy, ZIRLO and Zircaloy-4, which again has been described to others as simply Zircaloy-4 with a little niobium added, that we thought that the 17 percent criteria should continue to apply, that no additional testing was necessary.

The NRC agreed with that, and 10 CFR 5046 was amended to say state that the acceptance criteria applies to ZIRLO. So that was our licensing history on ZIRLO.

However, as you know, we got some new information. We became aware of the Bohmert work in January. Ralph had done some research in December, I guess, early to mid-December, discovered the Bohmert work, several other papers by Griger and the Kurchatov Institute. There were several references that we became aware of. Basically in mid-January we became aware of those, and we did a thorough evaluation of those.

And just some of the things that we saw in the Bohmert paper, some of the summaries, that the ECR to cause complete embrittlement -- this is for the E110 alloy -- is about one third the value for Zircaloy-4, and that is, in fact, also consistent with other work that was done with E110. So it was not only Bohmert.

However, in looking at that, we also noticed a number of physical differences in the oxide layers of E110 and Zircaloy-4, and several of the things that Bohmert mentioned was E110 displays a heterogeneous appearance to the oxide layer; that typically if we look at the oxide layer, there were two separate oxide layers separated by cracks, and these tend to play -- multi-oxide layers do tend to play, and his tests, the Zircaloy-4 always had a glossy black, firmly adherent single layer, relatively free from mechanical failures, and he noticed a high hydrogen uptake -- low hydrogen uptake. I'm sorry. He noticed low hydrogen uptake only if firmly adherent, crackless oxide layers were formed.

So there seemed to be a good correlation between the hydrogen pickup and the condition of the oxide layer itself.

Our previous history, particularly in high temperature steam oxidation tests that we had done as far as the high temperature burst test, showed that we always had glossy, shiny, adherent, black oxide layers on both Zircaloy-4 and ZIRLO. So we suspected right away that there was some difference, and it may have something to do with the oxide layer.

And let me see if -- however, again, we thought that in the review of all the papers Ralph had raised some pretty good points, and we really did feel that we should do some experimental work and verify that the 17 percent limit continued to apply.

So we did. Having said that though, let me reiterate that one other thing we wanted to look at was clearly to make the point that ZIRLO and E110 are not equivalent for a number of reasons, and the number one reason of course is that ZIRLO also contains tin here at the one percent level, a substantial amount of tin. It contains iron. The iron level is a tenth of a percent, and it does contain oxygen. The spec on oxygen is about .125 percent, or 1,250 parts per million, whereas in E110 it's typically 700 parts per million. So there are some differences.

Again, the tin and oxygen are alpha phase stabilizers, which means that the transition temperature from alpha to beta is slightly higher when those are present, or somewhat, just slightly higher, about 100 degrees or so higher than it would be in a zirconium-niobium binary alloy. So there are some differences in the phase change temperatures.

We see simply varying differences in the structure of the oxide layer.

But we did decide to run some tests, and we put together a test rig in February. Let me explain to you what it does. Okay. The main test section is an Iconel tube here, and inside this basically are two test specimens. The two test specimens are a piece of ZIRLO tubing and a piece of Zircaloy-4 tubing.

So we're putting both tubing types in and testing them simultaneously. They're held in here. Basically there's a sheath thermocoupler that goes up here. It has a small ring on it, and we sit the samples on top of that.

So here in the constant temperature zone we have a short piece of ZIRLO tubing, a short piece of Zircaloy-4 tubing. In alternate tests, we actually rotate them. So one time one is on the top; one time the other is on the bottom. So we rotate them.

And basically the objective here is to oxidize them under identical conditions, and then test them and see how the results compare.

This is a resistance furnace. It's a clamshell furnace. We preheat it to about 500 degrees, open it up, and then slide the test section in, close the clamshell and start the heat up.

We go to final temperatures. We actually have some thermocouples on the outside of here, outside of the test section which controls the power when we get to the final temperature.

We have, again, I said that there was a main sheath thermocouple coming up through here which sits in the middle of the tubes. So we have temperatures -- both two temperatures on the outside and then the temperature on the inside, and typically they're within three or four degrees of each other. So we are getting fairly uniform heating.

Okay. We have basically de-aerated water from an autoclave. It's pumped through our system. There's a steam pre-heater. We introduce steam into the test section. Actually prior to heat-up we run a purge gas through it, purge gas. There's another line which is not shown here. Purge the system, heat it up, and start the steam flow through it.

We run it then through a steam condenser. The hydrogen is vented out to the atmosphere, and we actually condense the steam so we know what the steam rates were and how much steam we run through.

Again, the heat-up rates here. There has been some discussion of what the heat-up rate should be. In this apparatus, our heat-up rates are about one degree Fahrenheit per second. Now, that is -- Mitch, how is that relative to LOCA heat-up rates? I meant to ask you that.

MR. NISSLEY: For a large break LOCA, typical heat-up rates would be on the order of ten to 15 degrees Fahrenheit per second. Small break LOCA might be as low as two or three degrees Fahrenheit per second. So that is a little low.

MR. LEECH: Okay. So this is somewhat

slower than the actual. It is somewhat significantly faster than Bohmert used. He used, I think, heat-up rates of about one third that. I believe he was using about a third of a degree per second.

The final temperatures when we got the temperatures ranged from 1,800 degrees Fahrenheit to 2,200 degrees Fahrenheit, which is, I believe, 986 degrees Centigrade to 1,204 degrees Centigrade.

We did run another test. We've run one at 1,700 degrees Fahrenheit, which is 926 degrees Centigrade, because as we'll discuss later, there was some concern that there was a temperature range between 950 and 1,000 identified by Bohmert where he seemed that the E110 alloy was particularly susceptible to hydrogen pick-up. So we ran that test.

Okay. We studied those for times ranging from five to 30 minutes. At the end of the time at temperature, we opened up the clamshell furnace, let the section cool by both radiation and convection. The cooling rates averaged about nine degrees per second for the test temperature down to 1,000 degrees.

Then, again, Mitch, you had some ranges. I believe that's reasonable.

MR. NISSLEY: A pretty good cool-down.

MR. LEECH: Pretty reasonable with what we might actually expect.

We don't quench. We let it cool completely to room temperature. Now, the objective here is not to run a quench test to see when we fail during quench, but to prepare specimens for subsequent ring tests.

We believe that if anything, this may be somewhat conservative in that we have a relatively slow cool-down rates for long periods of time. So if there is going to be any oxygen infusion to transform the prior beta phase, then this gives it more time to occur.

So basically the purpose here is to get specimens for ring compression tests.

Now, let me just give you the status of where we are in the process. We have done now -- where my notes are -- I would say we've oxidized about three quarters of the specimens that we expect to oxidize. Let's see. Okay. Let me first tell you what we're going to look at before I tell you how many we've done.

First of all, the number one priority is oxide layer characteristics. We believe that of all the things that we've seen with E110 and Zircaloy-4, that seems to be the biggest difference, and we want to take care to look at those. We're doing those by optical metallography and just general observations.

The next thing would be ring compression tests to assess the cladding ductility. Those will be done at room temperature at 275. Two, seventy-five, I believe, is the official number at which the 17 percent criteria was set up at.

With a tester similar to those performed by Hobson and Rittenhouse in ORNL report in 1972, we've attempted to maintain the same length-to-diameter ratios of the specimens, maintain the same head speed on the compression rate on the slow compression rate tests, and these were also similar to Bohmert, although there were slight differences.

Well, one thing that we did different was Hobson and Rittenhouse only went to a fixed displacement and stopped their compression test, where Bohmert continued to going until he either got clear indications of a failure or was getting too close to where he simply couldn't compress them anymore and backed off. We did that. We thought it gave a little more information.

There are some other differences. Bohmert cut his specimens into short sections prior to oxidizing them, where we oxidize a specimen about that long, and then we cut the rings out afterwards.

We measure the weight gain of the total specimen, and then we cut sections out of it, which is a slight difference, although I don't think it should make much difference.

Again, we cal look at the oxide thickness. We're going to look at the thickness of the alpha stabilized layer and the transformed beta layer. We will do micro hardnesses across the cladding wall to assess the oxygen penetration, and then we'll do measurements for total hydrogen and oxygen concentrations.

There's some of the matter we've gotten so far. What this is is a plot of the measured oxide thickness in microns. This was developed from metallography, plotted versus the oxide thickness that would be present if all the oxygen weight gain was transformed to an oxide layer.

And so there's a couple of interesting things here. One is that if you look, you'll see that if all the oxygen had been done into an oxide layer, then we would expect to go across about -- for a prediction of 100, you go across and we actually measured 70, which indicates that about 70 percent of the oxygen is going into the oxide layer and about 30 percent is going into the metal.

But what we also noticed is that for Zircaloy-4 and ZIRLO they're identical. There's really no difference between them, and I think that's a key difference because in one of the papers, when they looked at the E110 alloy they said that although for equivalent weight gains the distribution of the oxygen could be significantly different. A much higher percentage of it actually for E110 has ended up in the metal rather than the oxide layer.

So we believe that's a significant difference. We don't see any difference here between ZIRLO and Zircaloy-4.

Anything else I might want to say about this? No.

Then the next result we have are the results from the ring compression test. These are the ones we've done at 275 degrees Fahrenheit. What we've plotted is the relative displacement of failure. Relative displacement is the amount of compression divided by the other diameter of the specimen versus the measured ECR fraction. Now, this is not calculated; measured. There's an important distinction there, and that's the ECR assuming all the oxygen weight gain is stoichiometrically combined with the metal.

We see several things. One is we see that Zircaloy-4 and ZIRLO are for all intents and purposes the same over the whole range that we've tested. We see no difference whatsoever.

This is Bohmert's brittle limit. Whether that's our brittle limit or not, that still needs to be investigated because we need to look at each of these specimens and look at the nature of the failure. Was it brittle, ductile, or partially brittle and partially ductile?

We know from already that these were clearly brittle, and some of these actually are still in one piece, you know. After we bent them down, they're still in one piece. So they're obviously ductile, but we have to take some care to look into this area to suggest exactly what is the ECR at which we get transition or we are in a position where we're totally brittle.

Again, one other thing I might mention, too, which I haven't plotted, haven't shown you. We're also doing this at room temperature, and we've looked at some of the preliminary results that we got for Zircaloy-4 at room temperature, and they're reasonably in good agreement with what Bohmert got in his test for Zircaloy-4, which again is another, probably a second opinion that what he did was really pretty good work. There was no problem with what he did. It's just that the E110 seems to be substantially different than Zircaloy-4 because our Zircaloy-4 results seem to be consistent with his.

So which I guess is good. It tells us our Zircaloy-4 results were consistent with his, and our ZIRLO results are essentially equivalent to our Zircaloy-4 results, indicating that for ZIRLO-4 there's no reason to think that the 17 percent criteria doesn't continue to apply.

This, again, is measured ECR. It's not Baker-Just. Baker-Just probably is conservative by a factor approaching two. So we don't see a problem.

Again, what did we see? Just comparisons. Both oxide layers were dark adherent with no laminations. Both have similar fractions of oxygen in the oxide layer and in the metal. Ring compression tests of similar values of displacement of failure versus the measured equivalent planning reactant. We believe that the ZIRLO and Zircaloy-4 are just essentially exhibiting the same behavior. I see no difference at this point.

Again, we still have some more work to do on this. We're going to prepare for the remaining sample preparation. We've got to complete all the tests. We have got a few more samples to prepare. We've got some of the -- about a third of the ring compression tests to still do. The metallography samples have been made, etched. They have not necessarily all been evaluated yet.

We want to get all of the data, and what we really want to do then is get a good independent review. Those of us working on the project have reached our conclusions, but we want to bring in outside people both from in our company and potentially from outside the company to look at what we've done, document and review the results.

And our next scheduled meeting to discuss this with the NRC now is May 16th, I believe. There will be a review meeting. So we'll give another update at that point.

That really is what I planned to say today.

CHAIRMAN POWERS: You mentioned several times that your Zircaloy oxides showed no evidence of delamination.

MR. LEECH: Right.

CHAIRMAN POWERS: And the previous speaker showed some micrographs in Zircaloy-4 that had evidence of delamination.

MR. LEECH: Okay. Excuse me. One of those, I believe, was after spalling, wasn't it? Was that before or after?

After spalling it certainly showed delaminations.

CHAIRMAN POWERS: I guess my question is, really boils down to: what causes the delamination?

MR. LEECH: What causes? Obviously it's a stress and a differential thermal expansion.

CHAIRMAN POWERS: Okay.

MR. LEECH: But what causes one to crack and one not to crack, I guess I don't -- I don't know.

CHAIRMAN POWERS: Okay.

MR. LEECH: I don't know.

CHAIRMAN POWERS: Any other questions of this speaker?

(No response.)

CHAIRMAN POWERS: Well, thank you very much.

MR. LEECH: Thank you.

CHAIRMAN POWERS: Our next speaker has protested he's hungry, and so I'm going to recess for lunch, and we'll pick up Dr. Meyer's discussion of his

OECD meeting after lunch.

Thank you.

(Whereupon, at 11:58 a.m., the meeting was recessed for lunch, to reconvene at 1:00 p.m., the same day.)

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

(1:01 p.m.)

CHAIRMAN POWERS: Dr. Meyer is going to give us a precis of the OECD topical meeting on LOCA fuel safety criteria.

DR. MEYER: The meeting was organized by an OECD related group. Within CSNI there are several special expert groups, and there's one on fuel, on fuel safety margins. And it is this group, on which I am a member, that organized the meeting.

We'd had a similar meeting. A similar group in OECD had organized a similar meeting in 1995 on the reactivity accidents, very early in the period where we were looking into that. And it was very helpful because it brought a lot of people out of the woodwork and got a lot of information out in public that could be talked about.

And we decided before the Bohmert paper surfaced to organize this meeting, but when we learned about the Bohmert paper, it became sort of the center of focus of the meeting.

So the meeting really had three groups of papers: one on post quench ductility, one on axial constraints during quenching, and one on relocation of fragmented fuel into the ballooned region.

I have more material in the handout than be covered in a reasonable amount of time. So I think I'm going to just focus on this first group here. And also I'll skip over quickly some things that have already been discussed.

The first couple of slides in the package were from an introductory presentation by George Hache. They go over the ECCS rulemaking hearing and the fact that the criteria were developed from ring compression tests and that's been discussed, and I don't think that's a matter in contention. So I'll just skip that.

Now, Bohmert is from a research institute in Dresden, Germany. I did contact him. He was unable to attend the meeting. But George Hache presented, among other things, the main slide, the main figure from Bohmert's report in 1992 that shows the effect.

Now, you saw a few of these points on Framatome's slide, where they picked out the ones at 1,100, and they picked those out from Bohmert's slide and showed them on their graph.

But Bohmert had tested over a wide range of temperatures, both Zircaloy-4 and the VVER cladding, E110. And you can see this is the line that was on the Framatome slide. And you can see it coming down here around five percent cladding reacted.

I think Bohmert did his tests at room temperature. And George Hache looking back at all this says, "Well, it really should have been at 135 degrees Centigrade," so it would be a little higher than that.

But nevertheless you can see here, although there is scatter in the data, you can see a separation between the E110 ductility results and the Zircaloy-4 data results.

Now, Bohmert is not the only person who's seen this. This has been seen at four different laboratories in four different countries, was seen in Germany. It's been seen in the Czech Republic, in Hungary, and in Russia.

The Hungarian researcher who did the confirming work there was present at the meeting and has a paper and I have a slide from that.

The Czech researchers did not document it in a public place or in English. They wrote it up in a agency report in Czech, whatever, in Czech. But we have contacted them and we may be able to retrieve that data and get it in an English report.

And then in addition to that, George

Hache, who has this incredible talent to remember things from obscure places, remembered some meeting in Varna. I don't even know where Varna is, in 1994 where the Russians presented such results.

And so added to the three that we had been talking about, the Germans, the Czechs, and the Hungarians, here is the Bochvar Institute with ring compression test results and a line that separates the ductile from the brittle behaving specimens.

And when George -- this handwriting is George Hache's. He's informal sometimes. When he goes down this separating line down to the 135 degree temperature point, and he gets the six percent figure.

So George says, "If you apply Hobson's methodology to this set of data from the Bochvar Institute, you get a six percent ECR," which is consistent with the others that we have seen.

Now, the main presentations on this subject were given by Maroti from Hungary, Sokolov from Russia, Lebourhis from France, Bill Leech out here in the audience, and Hee Chung from our program at Argonne.

There were actually two papers on the subject from Russia and I only have a slide from one of them. The other one was kind of preliminary, and frankly, I was never able to understand the main results of that paper and have gone back to try and get clarification.

So let me just show you a few of the slides which are fairly easy to grasp, and which I think will summarize the essence of the material that was presented at the meeting.

This is the Hungarian work, and I think it's even cleaner in appearance than the Bohmert work in terms of seeing the drop-down in the ductility of the E110 specimens compared with the Zircaloy specimens.

It's interesting that at least in the German, the Czech, and the Hungarian work, they always measure Zircaloy along with their E110 measurements. So there's a control. And Hee Chung at Argonne has taken their Zircaloy results and replotted them along with his own ring test results from the '80s and Hobson's from the '70s, and they're all consistent, which is what we heard this morning as well.

So all of these laboratories appear to be able to make consistent measurements on Zircaloy, and we get these two sets of differences for the zirconium 1-niobium, and the difference is remarkable. It's not just a small difference. I mean, from 17 percent to six percent is a huge reduction.

Now, Sokolov in his presentation included this figure, and George Hache made interesting observation from this figure. This is not ring compression tests, now. These are quench test results. This is a failure map, and we often plot failure maps like this where we have the log of the time, the temperature versus one over temperature, and show on the plot usually the 17 percent line which would go on down, but then truncated by the 2,200 degree Fahrenheit curve.

And I'll show you a figure for Zircaloy. Generally, there is a substantial margin shown above the boundary until you get to the beginning of the failures. And you see, you see a margin along here, but when you get to 1,200 degrees the ductility seems to start a nosedive, and you have very little to no margin right here at the knee in the curve.

Now, that was presented -- that figure was presented at the meeting by Sokolov. George Hache makes the observation during the discussion and George Hache -- I don't know if he used these exact figures, but he pointed me to them and we got them out of our own reports.

But this is a failure map for Zircaloy

test summarized in a report by Van Houten, but Van Houten didn't do the work. This work was done at Argonne.

Okay. The construction lines are not laid on this figure, but the data points are, and what I'm going to show you on the next figure, now, is a figure with construction lines on it and no data points, but it's the same figure, and you'll see this is Figure 2A from the reference and this one is Figure 2B from the reference. And this solid curve here, then, is the one that bounds the thermal shock failures.

There's some other things on here. And here is the construction that shows the 17 percent line and the 1,200 degree limit. And you see quite a bit of margin, and across here there's a good 100 degree C. margin in this, which appears to be absent from the E110 plot.

Just an observation that George is saying is not only the ring compression test that are giving us this message. There's the quench tests that are giving us this message.

Okay. Now, I'm not trying to suggest that this is the same message, but this morning in the Framatome presentation we did see numbers that were close to the 17 percent line which don't have a lot of margin exhibited. I don't know whether that's significant or not significant, but I point it out to you.

On the other hand, and you saw both of these, this one and the Westinghouse figure before, there is just no difference apparent at all when you do the ring compression -- when you look at Framatome's ring compression test and Westinghouse's ring compression tests. So you saw these slides this morning.

I ask Labourhis directly at the meeting what was his opinion as to why there was such a difference between E110 and M5. And his answer to me was, "I have no idea."

Now, there's a suggestion that there's a difference in the material. There are some differences in the test procedures.

Nothing is apparent at this point. It's pretty much a mystery.

George Hache makes another observation which is rather obvious, but kind of important at the same time, is if it really is a difference in the material, we kind of ought to understand it because we may inadvertently move into that material regime. And it makes a big difference.

Now, you were asking some questions about the composition, and I have compositions of E110 and M5 from a couple of sources. The main points in this table are from a recent Halden report, where they're testing specimens. I don't know whether they're coupons or tubular specimens, in some oxidations tests.

And they have reported these numbers. These look like -- I would say these look like numbers that were measured, but I'm not sure about these numbers here.

Anyway, there are also papers in the open literature in the ASTM, you know, the zirconium in the nuclear industry conference that they hold every three or four years.

There's one with M5 results written by Framatome authors, and one with E110 results written by Russians that show these ranges. And you can see a few hundredths of a percent more oxygen in M5 than in the E110, and the iron, there's a little more iron. It's a very small amount.

Both are recrystallized. The E110 is said to be alpha recrystallized, so it's recrystallized. It's annealed at a temperature below the phase transition.

DR. BONACA: Does it show sulfur there?

DR. MEYER: Huh?

DR. BONACA: Does it show sulfur?

DR. MEYER: No, I couldn't find any sulfur content.

DR. BONACA: We heard this morning about --

DR. MEYER: Yeah.

DR. BONACA: -- M5, I thought.

DR. MEYER: Did mention the sulfur this morning, and I don't have any numbers on that.

I'm not sure that the cold work and the annealing is going to make any difference when you get into this regime of oxidizing above the face transition. It just seems to me like it's a soup of elements at those temperatures, and the chemical composition is really close.

I simply don't understand it. I don't have a theory or, you know, a big hunch. It's just hard to believe that it's the test procedures because they use controls all along. It's hard to believe it's the material because the material is so similar. It's hard to believe that it's the fabrication and cold work related things because it's a high temperature process that we're looking at, and I don't know.

Now, at this point in the meeting Hee Chung gave a lecture. Bill Shack will understand that Hee Chung likes to give lectures, and he gave us a lecture on a post quench ductility of zirconium alloys. And he repeated a number of things that we already knew and were talking about.

But he did bring out a couple of other points. I'm not sure whether all have been verified or not. But he points out the matter of the hydrogen induced ductility. And that hydrogen induced -- the role of hydrogen in reducing ductility wasn't understood in 1973, when Hobson's tests were done.

It was all thought to be oxygen. The levels of hydrogen in the specimens at that time were low, less than 150 parts per million, where it wouldn't have been above the threshold for some effect anyway.

But let's see if this is the -- well, I've got a couple of figures here.

Hee Chung now points out that for Zircaloy, that there seems to be a threshold around 600 or 700 ppm hydrogen. When you get that much hydrogen in the specimen, then it also contributes to the reduction of ductility.

And he has looked at Bohmert's data and Griger's paper. Griger is one of the Hungarian workers, and believes that he sees a threshold at a much lower level, down around 150 to 200 parts per million. Now, in the specimens that we heard about this morning, the concentration of hydrogen was even lower than that. So you wouldn't have been there.

And Hee Chung insists that we have to consider several factors and not just one. It's not just hydrogen. It's not just oxygen. It's not just niobium.

And then he presented this one slide, which is rather useful, to talk about the three routes to getting a lot of hydrogen in the specimen and how we only have hydrogen from one of these routes in the specimens that we're testing at this time.

You can get hydrogen during normal operation, and of course, we have not been testing that because the tests that we've been looking at have been on fresh tubes.

You can get hydrogen in the high temperature process. This is what we've been looking at.

And then there's another process that lets hydrogen into the cladding associated with the deformation during ballooning and rupture.

And this, I believe, is the process that led them to identify the role of hydrogen in embrittlement because apparently when you get this deformation, and you now have two-sided oxidation, you have a stagnant steam environment on the inside and the hydrogen doesn't get swept away, and the absorption of the hydrogen locally in that region is very high.

And so when they -- this work was done at a couple of -- I guess it was done at Argonne and it was also done at JAERI, in the early '80's. And when -- if you took slices near the region of the burst, took rings and looked at their ductility, they would not pass the non-zero ductility test related to 17 percent oxidation. So there's a local effect that's fairly strong.

Well, this slide suggests the importance of making some measurements on some real fuel rod material and not just on tubes in the laboratory. And of course, that's what we are interested in doing in our research program.

And then I was asked to give a brief presentation on our research program, and these are a couple of slides that I used. The first bullet outlines the program that we have at Argonne at the present time using Zircaloy. There have been some adjustments to this based on the PIRT process that was completed.

And now that we have our Zirc-2 and Zirc-4 in the laboratory and those tests are planned and ongoing, we'd like to start making arrangements to obtain some ZIRLO and M5 in this program.

And as I think I mentioned earlier, we broached this subject with Framatome and Westinghouse at the meetings that we had in February here at NRC.

I think that if we carry out this full range of studies with Zirc-2 and Zirc-4 that we may not need to repeat everything in that menu for the other cladding types. We might, for example, be able to skip the integral tests. It's an expensive test. I'm not sure that we'll be able to, but you might be able to characterize things well enough from them, from the simpler tests that they were measuring mechanical properties.

And so, in particular, we're quite sure that we'd want to do oxidation kinetics measurements, probably some sort of thermal shock test, look at the oxidation and the phase relations and measure the mechanical properties after running the material through a high temperature oxidation transfer.

DR. KRESS: Ralph, this looks to me like more data is going to an empirical relationship. Does this address Mr. Hache's comment or we need to understand the effects of small material differences? I don't see that it addresses that.

DR. MEYER: You don't see it directly, but it -- we really want to -- I'm not convinced that it's a small materials difference that's doing this. And so one of my main objectives is to find out what it is.

DR. KRESS: Okay. This will do that.

DR. MEYER: Well, it will for it's part of the equation. The other part of the equation is the E110 alloy. And what you don't see up here, but it's buried in one of the bullets on another -- in another presentation, was that we have this program with the Kurchatov Institute, and in starting in late 2001, this year, late in the year, we have them beginning a series of tests that are designed to shadow this program in their laboratory with E110.

So we want to look very carefully at ring compression tests, whether that's the right test or not. These tests have been criticized in the past. They're not real precise. They're good screening tests for some purposes, but maybe an axial tensile test might be a more precise way of looking at the ductile brittle behavior.

CHAIRMAN POWERS: When you look at you specimens, it looks to me like chemical compositions not going to answer the question for you. They're too close together.

DR. MEYER: Yeah.

CHAIRMAN POWERS: Now, maybe EDAX on the distribution of the alloying agents may be different. Maybe that tells you something, but do you also look at things like grain size and surface texture?

DR. MEYER: Well, we would. I don't think we're far enough along to say what we have planned out in a test matrix, but those are the easy things to look at, and the kind of things that we would normally do.

DR. CRONENBERG: Ralph, a couple years ago you had voted the idea of a 100 calories per gram for high burnup --

DR. MEYER: Yeah.

DR. CRONENBERG: -- plus a criteria of retention of residual ductility, that maybe the two might be the way the regulation should be written up. Does this flow from that thinking?

Is that thinking still in effect that there might be a requirement of some residual ductility rather than hydrogen and oxygen, then oxygen and hydrogen uptake?

DR. MEYER: It's not really connected, although you come out at about the same place. The 100 calorie per gram dealt specifically with a rod ejection type accident. And that's a accident where the cladding remains at a relatively low temperature, and where you haven't gone through a phase transformation and wiped out its fabrication history and all of that.

Now, the ductility initially when we were looking at the rod ejection, we were trying to see if we could use the ductility criterion instead of an enthalpy criterion.

And the critical strain energy density method that EPRI and the industry use, and that IPSN uses and EDF uses, is, in effect, a ductility based criterion.

But the origin of the two are quite different because at that time we weren't thinking about the ECCS hearing and what was done there and Hobson's results, and so forth.

DR. CRONENBERG: Okay, but I guess I'm

still not clear. Is your thinking still in terms of residual depility (phonetic) criteria? Is this still in the background for these experiments?

DR. MEYER: Certainly for the LOCA it is, definitely. I mean, this is the result of the hearing,and the philosophy we've been following even though we forgot that we were following it.

I mean, that was these criteria that we're using were based on retained ductility.

DR. CRONENBERG: But it's 10 percent oxidation, not in ductility requirements.

DR. MEYER: Yeah. So we may have to roll it back to the concept of ductility and look again at what attribute might characterize that adequately for us.

DR. CRONENBERG: Okay.

DR. MEYER: Okay. Now, along with the work on irradiated fuel rods, we'd like to -- well, we always in our program at Argonne, where we're looking at irradiated fuel rods, we always look at archive unirradiated material and do pairs of tests so that we can tell the difference between the behavior of fresh material and irradiated material.

There's a lot that we have learned and I think we sill can learn with the unirradiated tubing, and so if we can make some arrangement with Framatome and Westinghouse to work on their materials, we'd like to get started very quickly on the unirradiated tubing.

And here was a list of things that we proposed to do in a program in which we would ask for their cooperation.

And you see at the top of the list is to look at ring compression tests and other post quench ductility measurements to make sure that we're not using a test that itself has some inherent problems.

And we would propose to discuss this until we get some agreement on what is -- if the ring compression test is not the right test to use, what is the right test, and then to carry this out.

And there's a branch point over here where the same instructions go to Kurchatov Institute in our corollary program with E110 alloy.

CHAIRMAN POWERS: The entry on the slide that I guess I don't understand, it says no mechanical properties or other testing at this time --

DR. MEYER: Yes.

CHAIRMAN POWERS: -- later in the high burnup program. I was wondering --

DR. MEYER: Why?

CHAIRMAN POWERS: -- what other program is there?

DR. MEYER: In the Argonne program, which we often think of as a LOCA program, we also have a matrix of regular mechanical properties testing under low temperature, higher strain rate conditions that match up with the reactivity action.

So there's a lot of mechanical properties testing related to rod ejection action and related to the ballooning process.

This is before you get to the high temperature and the oxidation. And what we're saying here is that for the moment we wouldn't enter into those tests immediately. We would do those in connection with the high burnup tests at a later time.

It's partly a matter of resources. It's partly a matter of trying to work with the industry so that we don't reveal too many things about their proprietary materials that aren't necessary to reveal at this time in connection with looking for some explanation of this LOCA ductility behavior.

So that was put in there to try and be nice guys.

CHAIRMAN POWERS: No good deed goes unpunished here, Ralph.

DR. MEYER: And we have a current program that's working very nicely with EPRI, and we would just pattern it -- pattern it after that. So I've said all of these things.

Now, I have a few more slides from the other discussions. If you don't ask questions, I can show them quickly or I can just sit down. So it's your choice.

CHAIRMAN POWERS: Why don't we rely upon the members to review the additional material and -- because I'm anxious to hear what Margaret and Richard have to say.

DR. MEYER: Okay.

CHAIRMAN POWERS: And thank you for your presentations.

I'll comment that the ACRS has made a suggestion to the Commission that this program be given additional resources to test additional types of materials, and it sounds like you very much need it right now.

At this point, we'll shift gears just a little bit and move to the business end of the agency. And Margaret will give us some talk about recent operational issues and experience with high burnup fuel.

MS. CHATTERTON: Okay. It'll take me a minute to get myself organized.

CHAIRMAN POWERS: Oh, yeah, we permit that. Have you been running lately. That's the question we want to know.

MS. CHATTERTON: Have I been running lately? Today was a running day, but there wasn't enough time. So I ran Monday.

DR. KRESS: We messed up your running?

MS. CHATTERTON: You messed up my running.

CHAIRMAN POWERS: You should have protested.

MS. CHATTERTON: Two weeks from Monday is Boston. I will -- did I get this thing on right? -- I will be back at Boston, which I think will be a slow run, but it will be fun, and that's the major thing.

CHAIRMAN POWERS: That's right.

DR. KRESS: Just as long as you don't that shortcut.

MS. CHATTERTON: No, I don't take any shortcuts.

Actually, right now they have a timing chip. It goes on your shoe. It starts at the beginning, and they have a map that you run across every five kilometers.

DR. KRESS: Oh, okay.

MS. CHATTERTON: So they've got your time.

DR. KRESS: They've got you.

MS. CHATTERTON: You can't cheat.

CHAIRMAN POWERS: Well, you can cheat every three kilometers or something like that.

MS. CHATTERTON: Anyway, I'm here today to talk about operational issues and high burnup fuel as we've been using them in the last few years.

And here's kind of an outline to some of the things that I want to talk about. It's been a couple of years, I believe, since we talked about burnup extension activities. So I thought I would just start off with that, talk a little bit about where we are on lead test assembly guidelines, some recent fuel issues, and then talk a little bit about the current fuel reviews that we're in the process of doing.

CHAIRMAN POWERS: Okay.

MS. CHATTERTON: So as you probably remember our basic approach to burnup extension is that we're working with the industry to develop a strategy and a plan. It's going to be up to the industry to do the testing, to come up with the criteria, and then to justify the criteria.

At that point the NRC will review what the industry proposes, review their justification, and at some point, hopefully, be able to endorse their proposal as a regulatory guide.

We simply do not have the resources to do the research, to come up with the criteria like we did in previous times.

So, again, I think you've probably seen this. Our burnup extension guidelines will be working with the industry. We've required that they will give them some advice.

Certainly, it must address the current licensing requirements, the LOCA, the RAA and the ATWS. all of those things that are looked at today.

They'll have to give a justification of why any limit that they decide to use is appropriate going to higher burnups. And just as a review, what the industry has said they want to do is to go to probably 70 gigawatt days for BWRs -- that's the rod average -- and 75 for PWRs.

We've also told them that some of the area's can be risk informed. That's going to be their determination of exactly how they want to handle different things. And, again, it's all going to be subject to our review.

DR. KRESS: When you say risk informed, is that you're thinking Reg. Guide 1.174, risk informed there?

MS. CHATTERTON: Yes. They will be able to use some of the guidance that we've given before. They may look at certain things and decide that they want to make a proposal that certain things can be handled slightly differently on a risk basis.

DR. KRESS: See, what bothered me about that was Reg. Guide 1.174 is based on current burnups, and if you're going to extend the burnup, then you have a little bit of a circular argument because then you have to ask whether 1.174 has the right value of LERF in it, for example.

MS. CHATTERTON: Yes.

DR. APOSTOLAKIS: But it deals with delta LERF.

DR. KRESS: It also deals with absolute value of LERF.

DR. APOSTOLAKIS: Yeah. So I mean I don't understand what it means that --

DR. KRESS: Even delta LERF is going to be hard to determine because you're dealing with the delta fission product maybe. And it's not just inventory. You can handle that pretty easily.

DR. APOSTOLAKIS: In other words, what you're saying is LERF might not be the right metric. Is that what you're saying?

DR. KRESS: That's another issue I have. That's a separate issue.

DR. SHACK: But I think he was arguing that acceptance criteria value.

DR. KRESS: Yeah. I was arguing on --

DR. APOSTOLAKIS: On the acceptance criteria is acceptance criteria. Why should it be any different?

DR. SHACK: Well, I suppose you could look at it that way, too.

CHAIRMAN POWERS: Well, if Tom is thinking the way he has been thinking in the past he says, "Hold it."

DR. APOSTOLAKIS: Says what?

CHAIRMAN POWERS: He says, "Hold it." You derived your acceptance value by looking at the quantitative and health objectives.

DR. KRESS: Absolutely.

CHAIRMAN POWERS: Now, you can't do that anymore because the derivation path doesn't work.

DR. KRESS: That's right. That's exactly the way I was thinking.

DR. SHACK: The source term is different.
DR. KRESS: Yeah, maybe. We don't know.
DR. APOSTOLAKIS: The quantitative health objectives don't change, do they?
CHAIRMAN POWERS: We assume those are are given to us by God.
DR. APOSTOLAKIS: Working backwards, you have assumed certain behavior in severe accidents. And that's what's going to change?
DR. KRESS: Yes.
DR. APOSTOLAKIS: Okay. So the LERF value then may change.
DR. KRESS: That's what I was saying.
DR. APOSTOLAKIS: Now, the CDF will not change?
DR. KRESS: No.
DR. APOSTOLAKIS: Because we lowered it by a factor of ten arbitrarily. Right?
CHAIRMAN POWERS: I mean, I don't know. I mean, it seems to me --
DR. KRESS: Must have had a reason for that.
CHAIRMAN POWERS: Maybe it turns out that things are more susceptible to core damage.
DR. KRESS: Yeah.

DR. APOSTOLAKIS: It's more than a factor of ten what you be now. I mean there's a problem somewhere. But anyway, it might be that that --

CHAIRMAN POWERS: Factors of ten are not our of the question here.

DR. APOSTOLAKIS: Now, you said some parts may be risk informed. So you have decided that some parts may not be?

MS. CHATTERTON: No.

DR. APOSTOLAKIS: Okay. Just a figure of speech.

MS. CHATTERTON: Yes, a figure of speech, but basically we're letting the industry propose how they want to handle -- how they think is the appropriate way and to provide a justification, and again this will go and do the review.

CHAIRMAN POWERS: I guess, when you raise the issue of being risk informed, the challenge I see there has something to do with just what our discussion was. We typically don't have a great deal of information on these fuels under accident conditions, severe accident conditions that will give you any consequence.

Are you saying that the industry can come in, but they've got to come in armed with information on fuel behavior under accident conditions?

MS. CHATTERTON: That might be an option if the proposal is to go that direction. The main point is whatever method they decide to take, they have to provide the justification for why that's acceptable, with a great deal of emphasis on lead test assemblies.

That's one thing that we have emphasized greatly in the last few years, I would say in the last five years, and that's a result of fuel issues that we've had in these last five or so years, and I'll be talking about some of those later, and the things that we've learned that the fuel -- the lead test assemblies in the past did not always give us data or information that was really the most useful.

We've also said that a breath (phonetic) extension program will also have a fuel performance monitoring program. Somebody said the fuel performance monitoring; that's in core. I guess maybe it's really fuel surveillance program.

DR. BONACA: If you'd just stay with that slide, I would like to ask a question.

MS. CHATTERTON: Sure.

DR. BONACA: Clearly, the first bullet I can see that you are concerned about how long the cycle is going to be or the burnup.

MS. CHATTERTON: Yes.

DR. BONACA: And the issues that we discussed this morning. At the bottom there, I see fuel performance monitoring program. Now, currently, I mean, although it may be a concern to have fuel failures, the one percent for the fuel assumptions in analysis allow for 50 pins probably are going to be failed.

Okay. So I'm curious about what would this fuel performance monitoring program mean. I mean, for example, some of the Westinghouse plans have exhibited at times maybe four or five 17's failed in some batches. Okay. That's really an operational concern.

Is it also a regulatory concern right now? Is that what it's focusing on?

MS. CHATTERTON: This isn't focusing just on fuel failures.

DR. BONACA: Yeah.

MS. CHATTERTON: This is focusing on things like corrosion, growth.

DR. BONACA: Okay. I understand.

MS. CHATTERTON: It's focusing on all the types of parameters that -- I want to characterize it fairly by saying in the past many times the fuel has been burned, taken out, put in the spent fuel pool, and never looked at.

DR. BONACA: I understand.

MS. CHATTERTON: As a result we had some problems that might have been eliminated had the type of program that I'm talking about --

DR. BONACA: So for example, oxidation rates because those also go in estimation of performance under accident conditions.

MS. CHATTERTON: Yes.

DR. BONACA: Okay. I understand.

MS. CHATTERTON: Yes. And if you're not measuring your oxidation levels, you don't know if your inputs to your accident analysis are correct.

DR. BONACA: Okay. Thank you.

MS. CHATTERTON: And that's the main point in that.

DR. CRONENBERG: Margaret, on that, the NRC used to publish a fuel performance summary. Every year PNL used to do the work.

MS. CHATTERTON: Yes.

DR. CRONENBERG: They used to summarize it. That's no longer in effect.

MS. CHATTERTON: That's correct.

DR. CRONENBERG: Are you going to reinstitute this type of summary like the PNL used to do, but EPRI or industry or somebody will be -- will it be a formal, published monitoring program?

MS. CHATTERTON: I don't think we're far enough along to really be able too say exactly how that's going to work. How I envision when we will come up with a reg. guide will be it listing the types of testing that needs to be done, giving some ideas as to the frequency and when.

For instance, if you fuel goes beyond 62, but it's only to 63, and it's ten years down the line, it probably doesn't need to be measured again.

On the other hand, the different type of fuel, the slightly different power history, some of those, it's going to be difficult to come up with exactly how we handle this. There's going to be a lot of thought into that such that it provides enough data, but it doesn't totally hamper the industry such that they have to measure everything because that's not the intent.

It's going to have to be set up with controls such that after so much data, there's not need. If, on the other hand, if results aren't turning out to be good, then you need more. And it's going to have to have triggers in it for when you do more results, when you do more testing and also when you would need to do a hot cell.

Most of the hot cell exams, most of this is going to be pool site exams. The types --

DR. BONACA: That's what it was. The PNL was mostly pool site exams.

MS. CHATTERTON: Yes. Oh, yes.

DR. BONACA: But we don't have that data anymore.

MS. CHATTERTON: We don't have that data anymore.

DR. BONACA: I would hope that if you're going to push it to 70-75, that type of program goes on for a few years until you've had that --

MS. CHATTERTON: Yes. And that's the type of thing that I think we're thinking about. Yes, I miss having those reports.

DR. BONACA: Yeah.

MS. CHATTERTON: Those are great.

DR. BONACA: There was a lot of data.

MS. CHATTERTON: Where are we in this whole plan?

Well, in the last year or so there's been some progress. I would say not a tremendous amount.

Although the industry is working on it and it's slow, sometimes it comes in big steps.

We had a draft submittal in March of 2000, and the staff provided comments, and we had another meeting with NEI December 6th. They outlined their approach for RAA, and the staff gave them comments saying that it looked fairly reasonable.

And basically what they're doing is proposing a clad failure and coolability limits that are a function of burnup. They are based on enthalpy increase, and we've seen the preliminary work on this. We haven't seen all the details; we haven't reviewed all the details.

What they presented looks like a reasonable approach. Again, one it's submitted we will do a complete review of it.

We expect a submittal late summer. Again, sometimes work takes much longer than they think. Originally that was an early 2001 date, and it's been changed.

CHAIRMAN POWERS: If you get a submittal, say, in August, when do you think you'd have your review finished?

MS. CHATTERTON: This submittal I expect in August will not be a complete submittal. This will be a partial submittal and it will depend on the amount that's in it.

I think a submittal like this is going to take us a considerable amount of time, six months or so I would say on half of it, possibly a year or more on the complete package.

If it comes in in pieces, which is I think the intention, we will kind of review it in pieces so that, one, we can get feedback that they're headed in the right direction in a given area. But also so that we can keep the process moving.

There's going to be a lot of data needed, and some of this will be actually showing what data needs to be taken, needs to be obtained so that they can --

DR. BONACA: Excuse me. RIA stands for what, rod ejection accident?

MS. CHATTERTON: Yes.

DR. BONACA: Okay.

CHAIRMAN POWERS: Reactivity insertion.

MS. CHATTERTON: Right. I'm sorry.

DR. BONACA: It's more general.

MS. CHATTERTON: Well, I'm sorry. Were there anymore questions on this one?

(No response.)

MS. CHATTERTON: The next thing I wanted to get into a little bit was lead test assemblies just because, as I said, that's been an area that we really want some emphasis on, and we've stated all along that we think they should be prototypical, up to the proposed burnup with reasonable power histories that are similar to what's being used.

In the past we'd always said we wanted them in very nonlimiting locations, and it was very common to burn a lead test assembly to 50 or 60 gigawatt days, but to do it in six, seven cycles. And then when you put the fuel in and burned it in three cycles, you may not get the same -- exactly the same results.

So that's why there's going to be a real -- we're really emphasizing lead test assemblies, and we also know the type of cladding makes a difference, the flow conditions, the water chemistry. Lead test assemblies need to be characterized, of course, before irradiation.

And they will need pool site, and or hot cell exams after. Hot cells exams are probably going to be relatively infrequent, but there will be some need. Certainly there'll be -- full site will be needed certainly after each cycle, final burnup on assemblies that are designated as lead test assemblies before they start irradiation.

However, there may be assemblies that become LTAs after they've had some burnup on them. And so sort of to address that, to encourage lead test assemblies, to encourage the gathering of data, we've taken on a program to try to look for lead test assembly guidelines, something that we haven't had in the past.

Sometimes we had a submittal that we reviewed and approved. Many times there was not an actual regulation or any restriction. So under the 50.59, under the test parts they were able to do lead test assemblies. It leads to a lot of things that we hope by coming out with some guidelines we can improve.

The purpose, basically, to get a consistent approach, to get consistent database, to obtain data. There's a real benefit to the industry, too, in that they will know what we expect and know that if they follow these guidelines, that it's certainty.

I'll give you the outline topics and things in another slide, but that's basically it. We've made some progress on this. We met with WOG in

May of 2000. They put a lot of work into it, got the whole industry together. They submitted a topical report, which we looked at, and then we met with them in December.

We gave them our comments on that document in January, and we expect to hear from them again soon. Some of the things that are covered and need to be covered is a definition. Exactly what are we talking about as a lead test assemblies? What are the conditions?

Characterization, the type of characterization of the rods, full site, hot cell; when are hot cell exams needed?

Characterization will have to address both pre-characterization and after final burnup.

The guidelines will address the number of LTAs that can be in any one core. Also the location. That's what I mean by placement. Location in the core, what restrictions we think are necessary.

Safety requirements. The biggest thing here is in almost all cases the LTAs are designed such that they meet all the fuel design limits that the current core is meeting.

However, they meet them using a code that's been verified to 62. If we're now talking about burnups that are going higher, we're taking a step in saying that the code is valid beyond.

On the other hand, if you don't get the data you can't validate the code. So this isn't an area that we're working on, how to write that up, how to address it such that it's covered conservatively.

Part of the way that it's covered, of course, is the few number of pins that would be LTAs and given the whole number in the core.

DR. UHRIG: There's been reports the last three or four years of a control rod binding and sticking, and the general, as I recall, the exposure was about 43-44,000 megawatt days per ton, in the vicinity of the assembly.

MS. CHATTERTON: A little higher.

DR. UHRIG: Little higher. What's going to happen when you get the higher limits here? Are there going to be more problems of that sort, or is this something that has been addressed?

MS. CHATTERTON: That is one thing that will have to be addressed, and you're right. I didn't have it on the slide. But all the current type problems that we've seen, like the incomplete control rod insertion, some of these crud and oxidation type problems, all of those things are going to have to be addressed in a program to go to higher burnup, absolutely.

CHAIRMAN POWERS: When you think about it, people can go up to the 60 gigawatt days per ton. Now, somebody comes along and says, "Gee, I want to go to 70." That's what, 16 percent extrapolation?

It doesn't sound an outrageous extrapolation to me. Do we have evidence that we would expect changes in physics of the kind we saw between going from 30 to 60 when we go from 60 to 75?

MS. CHATTERTON: Do we have hard evidence?

I don't think we have evidence.

CHAIRMAN POWERS: I mean, there are fuel rods around that have gone up to 75.

MS. CHATTERTON: That's right. There are fuel rods that have gone around. I know of some in Europe that have gone as high as 100.

CHAIRMAN POWERS: That's right.

MS. CHATTERTON: Do we have evidence?

No, we really don't. But I think this is a point that the -- we said there was an extrapolation at one point in the past, and then there were some things that happened that maybe weren't thought were going to happen. And that it's time to stop and really examine all the criteria before we move or leave forward.

CHAIRMAN POWERS: I guess what I'm asking is -- I don't know whether I'm asking -- we're closing the barn door or we're making up for the sins of the past on the backs of the people that are guiltless. And we're talking about relatively small changes here and asking for a heroic amount of work it looks to me.

And I'm wondering is there really merit in that? I mean if we sort out the issues in 60 and say, "Okay, everything's fine here," and that, quite frankly, looks the direction it's going with these superior clads. You know, things look like they're moving along fine. Do we really want to create an enormous burden?

I mean, clearly moving the lead test assemblies out of the benign locations and into more prototypical location, that's something that's been needed for a long time. But after you go much beyond that, do we really learn risk significant information from LTAs?

MS. CHATTERTON: I think we gain a good deal of information. I think we also gain some confidence in reproducibility and uncertainty on -- you know, how uncertain are the measurements to take when you take them only once? You asked the question and --

CHAIRMAN POWERS: Oh, yeah. Ralph's good at that. He knows how to do that.

MS. CHATTERTON: And that is -- to me that is one of the things. This is an opportunity that you have to do that. That's not a difficult one. You can't do that on the accidents that Ralph is talking about. I mean, my goodness, the cost of the tests, you couldn't possibly do that.

But on this, these are some areas that you can.

CHAIRMAN POWERS: There are those of us to take the vote that say you absolutely must do that, especially because of the test are so expensive.

MS. CHATTERTON: Well, I don't look at it as -- it sounds like a lot but let me -- maybe I didn't characterize some of it exactly correctly.

I think there's a lot of areas that the industry is going to be able to right off very quickly.

CHAIRMAN POWERS: Okay.

MS. CHATTERTON: With the state -- going to higher burnup makes no difference and here's why.

We look at this, too, as this will help not only us, but the industry have a really good documentation of what is important and, you know, how things change.

I expect there'll be an awful lot of things that are written off very quickly, and they do, too. They're working on the major ones.

DR. CRONENBERG: Then maybe it's not so small. It's longer burnup, higher burnup, longer fuel duty times, 20 percent power increases. I thought you were asking a rhetorical question.

CHAIRMAN POWERS: No, I don't think I was. I agree with you. Some of the things -- it's more than just an increment in burnup because we're doing an increment in --

DR. CRONENBERG: I mean, Commonwealth Edison has come in on the docket with a 17 percent power increase, one step.

CHAIRMAN POWERS: There's a lot more going on here, none of which is really designed to coddle the fuel at all. It's going to put this fuel under some pretty heavy stress.

But the question then comes back to is it a risk significant issue that we're getting into. They can have all the operational difficulties that they want to volunteer for, and that's their business. Is it -- what we're asking about are -- our concern is more with the risk issues.

And, you know, I think we have to be careful not to close the barn door and put the burden on -- that's all I'm concerned about.

MR. CARUSO: I'd just like to make the observation -- this is Ralph Caruso from Reactor Systems Branch.

Dr. Powers, you had asked if there was a regulatory requirement for us to gather this sort of operational data, and I would make the observation that we are less interested from a regulatory point of view in this operational data than in the knowledge point of view.

One of the reasons why we're encouraging people to do lead test assemblies is to share the data with us. In the past they've been reluctant to do that, but what we're trying to do is we're trying to make the process easier for them so that they can do more testing which we believe benefits them.

And by trying to make the process easier and being a bit less threatening from a regulatory perspective, we hope that they'll share the information with us. We'll understand what they're doing, and we will therefor feel more confident that they know what they're doing.

So there's quite a bit of working together on this, and we're not necessarily going to change any regulations. We're just trying to understand what's going on. I don't know if that helps any.

CHAIRMAN POWERS: Sure.

DR. KRESS: I see two places where operational testing could shed light on or that has risk significance. One of them is on the rod insertion issue.

And the other one is that it's true that the iodine spike is due to failed pins, which are few and far between in a core, but that may be where that's -- may be where that spike comes from. I would perceive that if higher burnup increases the failure rate of those pins, it would increase the iodine spike, and you might be able to see that during the operational -- that's where it comes from anyway -- during your operational observations.

CHAIRMAN POWERS: You've got to convince me that an iodine spike is risk significant.

DR. KRESS: Yeah, it falls more in the category of design basis accidents.

CHAIRMAN POWERS: Design basis accidents. I mean I think there are risk -- there are interesting risk significant features here in the high burnup fuels. I'm not sure that LPAs get to them.

DR. KRESS: Yeah, that's -- I think that was your point.

CHAIRMAN POWERS: Yeah.

MS. CHATTERTON: The LTAs do provide you with the rods you need for something like Ralph's program and for really --

CHAIRMAN POWERS: Now, there's where you get it. Now, Ralph's program's got to be extended to 75 gigawatt days per ton; right, Ralph?

MS. CHATTERTON: Actually, we've said the industry has to then pick up the tab beyond 62. That's as far as the agency program. We said we do confirmatory work to 62 and then beyond that --

CHAIRMAN POWERS: I know what you said. Now, we just won't hold you to it. We'll let you backtrack on that one.

(Laughter.)

MS. CHATTERTON: The last point on my lead test assembly guidelines thing is we don't have reporting in there. Basically, hopefully it would be a template. It would be very easy to fill out, but it would give us -- it would provide the data. Then we would be able to know exactly what's happening with LTAs.

CHAIRMAN POWERS: Nothing that you are legally bound to is easy to fill out.

MS. CHATTERTON: I just finished my taxes.

CHAIRMAN POWERS: That's right.

DR. CRONENBERG: You know somebody was -- that wasn't very expensive, that annual -- that kind of pool site inspections, and I think it was a good thing, and we don't do it any more.

CHAIRMAN POWERS: Yeah, I mean there's not question it's a good thing, but the idea that a licensee is going to have an easy report to fill out, I mean, it just doesn't exist. There is no report that the licensee prepares that's easy to do, because they are --

MS. CHATTERTON: Easier?

CHAIRMAN POWERS: Easier is possible.

MS. CHATTERTON: Okay, easier.

DR. UHRIG: What happens to the lead test assemblies? Do they remain with the utilities?

MS. CHATTERTON: Yes.

DR. UHRIG: And are they usually sent for examination in detail or is this just sort of a -- what kind of data comes out of them?

MS. CHATTERTON: At the end, we would expect an all lead test assemblies to do pool site exams, and that --

DR. UHRIG: Okay.

MS. CHATTERTON: -- that would consist of oxidation measurements, probably growth measurements --

DR. UHRIG: Growth rate, yeah.

MS. CHATTERTON: -- growth rate, visuals, get an awful lot from visuals. And then depending on if anything was found, it would determine what further --

DR. UHRIG: They don't do a destructive examination though. Metallurgy --

MS. CHATTERTON: No. If something really is shown, then we would think that a hot cell exam --

DR. UHRIG: Would be in order.

MS. CHATTERTON: A constructive hot cell exam would be in order.

CHAIRMAN POWERS: How many cells in the country are available to do full length rods?

DR. UHRIG: One. Two.

CHAIRMAN POWERS: Two.

MS. CHATTERTON: Yeah. A number of hot cell exams, few and far between.

So moving on, why do we really want a lot of that?

Well, part of it is because of some of these recent fuel issues. Oxidation higher than predicted. We have several cases where, as I said, the LTAs behave beautifully. If the fuel is burned as the LTAs were, it behaves beautifully. But if it's burned at a higher rate, at faster duty, they've gotten very different results.

I think you're all probably aware of axial offset anomalies that still tend to be -- that's a problem that's still not completely understood.

DR. UHRIG: Isn't that pretty much boron chemistry?

MS. CHATTERTON: It's a chemistry issue, but it's also a fuel duty issue. And it's a very strange --

DR. UHRIG: Well, it does depress the flux in the area and reduces the load on the fuel.

MS. CHATTERTON: That's correct.

DR. UHRIG: But it would force it to be somewhere else for the same power level.

MS. CHATTERTON: Yes, it forces -- it's usually the precipitate at the top of the fuel forcing the power to the bottom. You end up with a shutdown margin problem. Had one utility that had to operate at 70 percent power for four of five months as a result of that, and it's continued through other cycles.

Several other utilities have seen it, not anywhere near to that extent.

DR. UHRIG: This is --

CHAIRMAN POWERS: Do we understand -- I mean this is an inverse chemistry thing. Inverse solubility issue, and you don't ordinarily think of that arising with boron. Do we understand why boron suddenly has an inverse -- boron becomes less soluble at high temperatures.

MS. CHATTERTON: Actually, it's a sub-cooled boiling. Basically, what you've done is you've precipitated some crud onto the control rods, you're in a region of sub-cooled boiling, and in the process of sub-cool boiling with the boron, you precipitate boron into that crud.

CHAIRMAN POWERS: And that's the step I don't follow.

MS. CHATTERTON: You don't follow.

CHAIRMAN POWERS: Why does the boron suddenly say, "I want to precipitate"?

MS. CHATTERTON: Well, with the sub-cooled boiling you've got a mechanism there to -- you want to give me a little --

MR. NISSLEY: I'm not an expert on this but some of the theories are that when you have crud and corrosion in the presence of sub-cooled boiling, that the boiling mechanism is coming off as pure steam and leaving the boron behind.

CHAIRMAN POWERS: And then it gets flooded right back up with water and --

MR. NISSLEY: It's thought to -- it's sometime referred to as boron hide-out where it's not on the -- completely on the outer surface. It's somewhat within the structure of the crud and the corrosion.

MS. CHATTERTON: You get kind of like little chimneys in within the --

CHAIRMAN POWERS: This sounds like on of the things that if you tried to do it, it would be impossible.

MS. CHATTERTON: Probably so. But it's certainly been a problem that --

DR. CRONENBERG: But it's real. I mean, they've measured crud with a high boron content.

MS. CHATTERTON: Yes.

CHAIRMAN POWERS: Well, I'm still asking why.

DR. CRONENBERG: Yeah, I don't know, but

it's there.

MS. CHATTERTON: Everyone has spent a lot of time on this issue, and it's still around. We've had some fuel failures in a couple different plants due to high fuel duty. Again, a combination of crud and high fuel duty.

In all these cases, we've seen the effects of water chemistry, high crud build-up, and we've seen some accelerated growth of rods in assemblies.

That's much more the IRI issue that is pretty much under control. I think I could say that very easily in all plants, or at least appears to be up until very recently.

The last thing I wanted to talk a little bit about is some of the current fuel reviews that we're doing. We have two reviews on cladding types that are in house now. One is the duplex cladding developed by Siemans, used extensively in Europe. That's the one that has rods up to 100 in the Goesgen plant in Switzerland.

CHAIRMAN POWERS: Wow.

MS. CHATTERTON: The review on that cladding will be to 62. And we're just beginning that review right now.

We're also reviewing the use of ZIRLO for

CE plants. We have some CE plants that have fairly high duty that have been using a low tin Zircaloy that's not been standing up to quite what they would like.

And so the use of ZIRLO in those plants would be extremely advantageous. And that's the reason the timetable is they really want this by the end of the summer. So we've got a large review.

The issue is basically a lot of it will be making sure that the interfaces are done correctly on the computer codes, that you get the right properties in, and it's handled in each way. So there's a lot in there.

And basically that's what I had as far as issues.

DR. UHRIG: What do you mean by duplex cladding?

MS. CHATTERTON: Duplex is -- it's a double type of cladding. It's almost the reverse of the BWR liner cladding.

DR. APOSTOLAKIS: The barrier cladding.

MS. CHATTERTON: It's got its corrosion barrier on the outside, and it's a Zircaloy on the inside.

DR. APOSTOLAKIS: Okay. It's a double

cladding.

MS. CHATTERTON: It's essentially a double cladding. Very, very --

DR. UHRIG: They're both Zirc?

MS. CHATTERTON: Pardon?

DR. UHRIG: Both are Zirc?

MS. CHATTERTON: No.

DR. APOSTOLAKIS: Different material.

MS. CHATTERTON: The outside is a -- I have to think.

PARTICIPANT: It's a proprietary material to Siemans.

MS. CHATTERTON: Yeah.

DR. UHRIG: Oh, okay.

MS. CHATTERTON: The strength part, inner part is Zirc-4. And as I said, they've had -- they've used that extensively in Europe. The data from it as far as corrosion and performance is absolutely excellent.

CHAIRMAN POWERS: I hope that once you get through your duplex cladding review, you come down and talk to us a little bit about that.

MS. CHATTERTON: Sure.

CHAIRMAN POWERS: Because I think that would be interesting for us to see.

MS. CHATTERTON: Good. Thank you very much.

CHAIRMAN POWERS: Thank you, and good luck in Boston.

MS. CHATTERTON: Oh, thank you. It'll be a slow run. It will be fun, but it won't be a fast run.

CHAIRMAN POWERS: Don't care how slow it is. the fact that you're there is just amazing.

DR. KRESS: We're going to look for you on TV.

CHAIRMAN POWERS: We'll watch for you on TV.

MS. CHATTERTON: I'll be the last one.

CHAIRMAN POWERS: We're going to switch gears and Dr. Lee fresh from a vacation of over -- almost a week duration in Italy is obviously going to be in fantastic spirits to talk to us about the MOX research program.

Yes, you're in a good mood when you come back.

DR. LEE: I'd like to briefly tell you something about the MOX research that Office of Research undertook, just started last November.

How about now? Thank you.

And you know why our interest in mixed oxide fuel. In February 2nd, the NMSS team came before the full Committee and briefed you on the certification plan and what is the activity related to your mixed oxide fuel, MOX fuel use in U.S. That is basically the disposal of up to 33 metric tons of MOX fuel in using it in our commercial reactors, and the two plan, four units targeted is McGuire and Catawba.

CHAIRMAN POWERS: What I have never understood is why ice condenser plants are particularly suited for using MOX fuel.

DR. LEE: And you were told that they really did not target ice condenser, remember?

(Laughter.)

DR. LEE: There was two Virginia power plants that was involved with it, but they drop out of it, but it happened in the two plants that's left. There are four units left now, ice condenser plant, under Duke Power, and I'm sure your concern has to do with the severe accident issues about fuel dispersal.

CHAIRMAN POWERS: Yep. Comes to mind.

DR. LEE: I think, one, we did the DCH for ice condenser plan. We found the McGuire plant is slight a bit higher than the cutoff point that we use, like .1 conditional failure probability. It came up to .14, but the Duke Power took issue with us that if you take into the real design of the plants, that number will come down.

So when the whole overall evaluation for MOX use in the McGuire come in, those numbers will be --

CHAIRMAN POWERS: Yeah, I think I would have responded by saying, "Yeah, and when I take the degradation of the containment into account, the number goes up again."

DR. LEE: Well, the research activities really focus on supporting a user request that came in back in late '99, and at that time we didn't have any budget to address it, but we just had budget this year to address the technical assistance requested by the regulations, nuclear reactor regulations.

They are interiors (phonetic) neutronics, fuel and source terms. The neutronics, they want to modify the codes that were used for MOX and also, of course, goes with it all the fuel behavior, monitoring assessment for the fuel behavior for design based accidents and under normal as well as design based accidents need to be corrected before we can use it.

And then in the source term area that we also need to validate that the source term that we used for UO-2 fuel (phonetic) is approximate for MOX.

DR. KRESS: How much, what percentage of the fuel would be MOX in these?

DR. LEE: It's normally one third of the core would be MOX.

DR. KRESS: Oh, as much as one third?

DR. LEE: Yeah.

DR. KRESS: Okay. I --

PARTICIPANT: Forty percent.

DR. KRESS: Okay.

DR. LEE: Or even more than that. Thirty to 40 percent.

Now, as I mentioned to you, we started this activity not too long ago, but at that same time before that, Ralph Meyer was doing a PIRT on the high burnup fuel. So since we know the MOX is going to be coming into play, we attached to ask our experts to tell us something about what do we have to do for the LOCA and reactivity accident, and that PIRT has been completed.

And on that Web site you will see the reports related to LOCA as well as the RIA accident.

The source term PIRT now is going to be starting very soon. It's not just for MOX. It's also for high burnup fuel as well, and we expect to finish by this year.

The composition for the experts have not been -- selection not been completed because we're waiting for a response from French and from Japan and also from the industry selecting experts to participate in this panel.

The NRC internal one has suggested some members, and we're working on that.

Now, in the neutronics area, there are three areas that we have initiated. The first one is the PARCS code that we have at Purdue University. That has been used for many years. This PARCS code is a neutronics code being interfaced with our thermal hydraulics codes like TRACK M or RELAP, and we have used it, and we have used it very successfully for RIA type analysis.

And we initiated the modification for this to make it more usable for MOX. That is started in November, when we initiated these activities.

At this time, we extended the number of group of energy that can be handled by the code from two groups to n group because it's very easy to make it general. One time we can use seven groups, four groups, two groups, because if the industry comes in with analysis with two groups, we have to be able to collapse it to two groups so we can analyze it on the same base.

The cross-section because of the isotropic between the UO-2 bundles and the MOX bundles, there will be very sharp gradients of neutron flux. So we have to handle the scattering correctly. So we have expanded the cross-section angle of dependency with P3 approximation. That has been completed as well at this time.

DR. APOSTOLAKIS: Is there a report where I could go and find more about these observations like, you know, why you need to go to the P3 approximation, and so on? The motivation for the research, in other words.

DR. LEE: I think the motivation if you look at the Europeans, the way they analyzed the MOX code, they usually use a high order scattering to do the approximation.

DR. APOSTOLAKIS: So I should look to Italy as well to find that?

CHAIRMAN POWERS: Well, I think there's --

DR. APOSTOLAKIS: There must be a report somewhere.

CHAIRMAN POWERS: Yeah, one of the authors of PARCS put out a document that went through all of these things, and it was given to this subcommittee a couple of years ago, I guess. I can't remember the exact title, and I mean, I am sure we could find that for you.

DR. APOSTOLAKIS: Okay.

DR. KRESS: It was a pretty good document as best I remember.

CHAIRMAN POWERS: It was a pretty good document. I mean, it raised the scattering and the group issue. It also raised the delayed neutron fraction issue.

DR. LEE: Yeah, the delayed neutron fraction.

DR. MEYER: Was this a Commission paper that you're referring to?

CHAIRMAN POWERS: No, no. Actually it was a Purdue report.

PARTICIPANT: It was critical, a bit critical, right?

CHAIRMAN POWERS: Well, I wouldn't say it was critical. I would say that he came back and said, "Look. My PARCS code right now can't do the MOX fuel because of these things," and he listed down what he had problems with using PARCS for that.

So I mean if it was critical, it was

critical of his own codes.

MS. SHOOP: This is Undine Shoop with Reactor Systems.

You can find more detail in the Commission paper that we wrote. We've authored two of them at this point. One would be from '99, and one would be early 2000, and I'm sure we can get copies of them for you.

DR. APOSTOLAKIS: Good.

DR. LEE: In addition, at this time there was a researcher from Saclay, is stationed at Purdue University assay change for about a year, and you can reduce in the French code CRONOS, and this code has been benchmarked against many of othe plant data in France that use MOX code. So we like to compare that with the developed PARCS that we're going to be using for MOX code analysis as well.

Tom Downer from Purdue University is the one who is the PI for this, principal investigator for this work. He's also working with the OECD and NEA to develop a theoretical benchmark for reactivity transient. This is quite a lot of work to do because now you need to develop an exercise that go from steady state and looking at some transient, how would the parts compare with other codes?

Of course, you would be using a Monte Carlo code calculation, and so forth, but this is a code-to-code comparison.

We also initiated a very small activities at Brookhaven under Dave Diamond. He has been helping us for many years, helping us to do independent assessment of PARCS, and we intend to use him to continue this activity.

It provides feedback to code developers, and we try to make it also more user friendly, too, because a couple in between the milars (phonetic) continue to be a problems in setting up the problem, but we are making it better now.

And then also, in terms of if there is any technical issues that we require his assistance to review, the licensee will submit to us and we will ask them to do so.

At the same time we also initiate a lattice physics code develop at Oak Ridge. It's a routine called NEWT, and this is part of the scale code, the whole suite of codes that Oak Ridge use for shielding, heating, decay heat, and also analysis.

And this will enable us to generate the cross-section, assembly-wise cross-section that we can feed into PARCS and that PARCS can use for steady state, and as well sa depletion, as well as transient analysis, especially IA type.

In the fuel area, of course, we started to update the material properties for the FRAPTRON and the FRAPTRON codes to be used for the MOX analysis. Then, of course, we have to assess the experiment against data.

There is a Halden exercise, blind test, the CS&I test completed, and at that -- actually the rig is still inside the reactor. They continue to monitor, measure the build-up of the fission gas, and the temperature. So you can get those probably as a function of burnup.

The exercise they did was allow 14 gigawatt days per ton. At that point they asked all the participants to do the calculations. That was back two years ago, and they just finished that.

So those are the information that we would like to revisit, and of course, in this area, I didn't mention, of course, the Cabri test for the IRA. We would like to look at the gigawatt behavior, as well.

In the source term area, we are negotiating with the France to get VERCORS experiments. The VEGA from Japan, they will not be doing any MOX experiment until like 2003.