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Reactor Fuels Subcommittee

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UNITED STATES OF AMERICA  
NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

REACTOR FUELS SUBCOMMITTEE

+ + + + +

MEETING

+ + + + +

WEDNESDAY,

APRIL 4, 2001

+ + + + +

ROCKVILLE, MARYLAND

+ + + + +

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

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1       PRESENT (Continued):

2                   AUGUST W. CRONENBERG, ACRS Fellow

3       ACRS STAFF PRESENT:

4                   MEDHAT EL-ZEFTAWY

5       ALSO PRESENT:

6                   MICHAEL ALDRICH

7                   SUDHAMAY BASU

8                   EDWARD BURNS

9                   RYAN T. COLES

10                  MARGARET CHATTERTON

11                  SKIP COPP

12                  RALPH CARUSO

13                  DAVID DIAMOND

14                  GARRY GARNER

15                  RICH JANATI

16                  STEVE LA VIE

17                  RICHARD LEE

18                  EDWIN LYMAN

19                  BOB MARTIN

20                  LARRY OTT

21                  JACK ROSENTHAL

22                  HAROLD SCOTT

23                  UNDINE SHOOP

24

25

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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,

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1 and it will be made available as stated in the Federal  
2 Register notice.

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

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

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

12 (No response.)

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

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

20 (Laughter.)

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

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

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(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.

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

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

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

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

24 We have embrittlement criteria in the  
25 regulations for the loss of coolant accident. We'll

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1 talk about those a lot today.

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

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

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

13 DR. MEYER: Okay.

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

18 DR. MEYER: Okay.

19 CHAIRMAN POWERS: Stay with the real  
20 stuff.

21 DR. MEYER: Okay. The safety criteria  
22 that we used for all developed for fresh or low burnup  
23 Zircaloy clad fuel rods. We believe for many years  
24 that low burnup also provided the limiting conditions,  
25 but with the movement to the higher burnup fuels and

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1 the large concentrations of burnable poisons, you can  
2 now see peak powers occurring later, not at the  
3 beginning of life, but as late as end of second cycle.

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

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

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

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

25 DR. MEYER: Yeah. Usually the big actor

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

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

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

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

17 DR. CRONENBERG: Okay.

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

20 DR. CRONENBERG: Yeah.

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

23 Now, the status of where we are right now  
24 is that we have burnups approved to 62 gigawatt days  
25 per ton. This is average for the peak rod in the

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1 core. In Europe they tend to report their license  
2 limits in terms of average for the peak assembly,  
3 which is a lower number by about ten percent. So you  
4 have to keep that in mind. We are not that far ahead  
5 of the rest of the work in our burnup approvals.

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

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

21 And so we've recognized some -- and more  
22 recently, the questions that will be addressed heavily  
23 in this meeting by the later presenters and then in  
24 the summary of the meeting that I'll be describing  
25 about the possible effects of niobium on the

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1 embrittlement criteria for loss of coolant accident.

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

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

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

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

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

24 PIRT is a phenomenon identification and  
25 ranking table. You build tables of phenomena that

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1 occur during the events that you're studying, and you  
2 try and learn something about that by discussing the  
3 importance in each of those, of each of those  
4 phenomena.

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

7 DR. MEYER: Yeah.

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

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

12 DR. KRESS: For the peak rod?

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

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

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

17 (Laughter.)

18 DR. KRESS: Yeah, I would assume.

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

21 DR. KRESS: The peak rod is like 1.4?

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

25 MR. NISSLEY: These are very approximate.

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1 Mitch Nissley from Westinghouse.

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

7 DR. KRESS: Okay. That's --

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

10 DR. KRESS: Okay. Thank you.

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

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

25 TMI-1 was chosen because it had been used

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1 for an international standard problem. There were  
2 input decks. We have done extensive analysis in our  
3 cooperative work with IPSN in France and Kurchatov in  
4 Russia.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

25           And I think that's what made it

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

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

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

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

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

20 DR. KRESS: Not core-wide event.

21 DR. MEYER: Right.

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

24 DR. MEYER: One, seven, seven is for the  
25 rod ejection accident. It's -- I don't know the exact

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1 title, but it's the methods and assumptions for  
2 analyzing a PWR rod ejection accident, specifically  
3 for that event.

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

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

8 PARTICIPANT: Seventy-four.

9 DR. APOSTOLAKIS: Five, six, seven?

10 PARTICIPANT: Those are the same.

11 PARTICIPANT: It's 117.

12 DR. APOSTOLAKIS: Oh, 11?

13 PARTICIPANT: This is an oldie.

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

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

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

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1 lot.

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

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

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

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

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

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1 any good or not?

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

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

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

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

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

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

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

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

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

18 DR. MEYER: If you had a what?

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

21 DR. MEYER: Yeah.

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

24 DR. MEYER: What we did in the end was we  
25 agreed on some -- I forget what we call them -- but

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1 some scoring criteria, and we went back and had a  
2 little formula for deciding two things about a  
3 particular phenomenon. If it was important and if it  
4 was well known because we would address both of those  
5 at the same -- you know, in the same discussion, and  
6 what you're really looking for are things that are  
7 believed to be important and not well known, and those  
8 are the items that you ought to focus on.

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

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

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

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

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

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

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

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

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

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

25 We have believed for some time now that

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1 the 280 calorie per gram number should come down in  
2 the neighborhood of 100 or 80 calories per gram for  
3 high burnup fuel, and so we asked David Diamond to do  
4 calculations of a rod ejection accident where he gets  
5 100 calories per gram deposited in the fuel rod.

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

10 And you hear a chorus of utility people  
11 and others say, "There's no way you can have a control  
12 rod worth two dollars and 50 calories per gram,  
13 \$1.20." Well, maybe.

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

23 And if, for example, you have to have two  
24 dollar control rod worth, and NRR knows for sure that  
25 we don't have two dollar control rod worth out there,

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1 then you're done.

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

5 DR. MEYER: Yes.

6 DR. BONACA: -- pure enthalpy limit?

7 DR. MEYER: Yes.

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

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

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

22 And for the first couple of days we  
23 decided how we could adopt those directly as the high  
24 level criteria for the ranking exercise, and a  
25 conclusion from that discussion was that was going to

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1 be really difficult because neither the codes that we  
2 were looking at, nor the experiments we were  
3 considering would take you all of that distance.

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

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

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

23 And so those were things that could be  
24 addressed with the codes and the experiments that we  
25 were talking about until we settled down to that

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1 level, and we used that throughout.

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

7 DR. MEYER: Yes, that's right.

8 DR. CRONENBERG: For each one of these?

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

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

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

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

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

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

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

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

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

20                  So that's the kind of thing we've see.  
21                  Well, okay. Some other results of the PIRT was the  
22                  majority thought that you needed to run tests in the  
23                  burnup range that you were really looking for because  
24                  part of the action is in the cladding, but part of the  
25                  action is in the pellet, and even if the properties of

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1 the cladding are dominated by oxidation or hydride  
2 distribution, the loading is going to be determined by  
3 the pellet, which is affected by burnup.

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

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

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

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

24 Also, near the end of the discussion of  
25 the rod ejection accident, we realized that there may

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1 be some of the newer alloys which have so much  
2 ductility even at high burnup that they don't fail by  
3 this pellet cladding-mechanical interaction, and in  
4 those cases, then you would be able to go on up to  
5 higher energy depositions before you failed, and that  
6 the phenomena that would come into play would be more  
7 like the high temperature transient effects in a loss  
8 of coolant accident.

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

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

25 I understand what you're doing. You're

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

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

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

15 DR. BONACA: Okay.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

23           DR. MEYER: Got everybody excited, and it  
24 probably is an anomalous result. I think we  
25 understand this one now. The understanding that we

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1 believe we have is not universally accepted, but it  
2 looks like that the precondition of that fuel rod was  
3 at such a high temperature that it caused hydride  
4 redistribution that affected the ductility.

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

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

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

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

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

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

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

8 DR. MEYER: Yeah. This is the --

9 DR. KRESS: Pretty much.

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

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

19 DR. MEYER: That's exactly right.

20 DR. KRESS: Okay.

21 DR. MEYER: That's exactly right.

22 DR. KRESS: Thank you.

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

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

6 DR. MEYER: Yeah.

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

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

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

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1 I don't know if there are any conditions  
2 in the reactor that could do that. What we are asking  
3 ourselves though is if there are conditions during  
4 vacuum drying for storage which could cause this to  
5 happen because this redistribution happens when you  
6 don't have the normal pellet expanding putting stress  
7 on the cladding, and in the storage casks when they  
8 dry them, you get -- I don't know the exact numbers,  
9 but I've heard them talk about numbers in excess of  
10 400 degrees Centigrade sometimes.

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

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

25 So, I mean, the hydride redistribution

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

4 DR. MEYER: That's right.

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

7 DR. MEYER: Right, right.

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

11 DR. MEYER: Yeah.

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

14 DR. MEYER: Yeah.

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

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

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

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1 DR. MEYER: Implies that?

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

5 DR. MEYER: Yeah, well --

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

9 DR. MEYER: Yeah.

10 MR. SCOTT: Is that part of it?

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

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

25 It just looks like you annealed it and

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

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

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

8 DR. KRESS: They are circumferential?

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

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

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

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1 DR. KRESS: You'll input mechanical  
2 properties.

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

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

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

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

21 DR. MEYER: I'm sorry?

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

24 DR. MEYER: Large, but we have --  
25 hopefully we'll have ample mechanical properties

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1 measurements, and we'll have other tests. We have all  
2 of these other tests with Zircaloy.

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

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

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

22 We're going to be on a plateau of  
23 understanding for at least three years, and so we  
24 might as well go ahead and try and go through the  
25 exercise, see if we can finish it off, and then when

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1 we get to the water loop if we see any surprises, then  
2 we'll go back and make an adjustment.

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

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

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

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

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

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

7 DR. CRONENBERG: There's things on the  
8 docket now that are kind of pressing, like the power  
9 upgrade for I don't know if it's Commonwealth Edison  
10 anymore, but the Dresden, Quad Cities. They're going  
11 for 17, 20 percent power upgrades with extended fuel  
12 burnup. I think with the new GE design to above 50 or  
13 55, maybe even 62. So where does research come into  
14 play with NRR that NRR has to review these  
15 applications?

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

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

23 DR. CRONENBERG: Okay. I guess it's more  
24 on the power oscillations when we get to the BWRs, not  
25 this rod ejection, but still I'm sort of seeing how

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1 the research falls into near term licensing, licensing  
2 amendments.

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

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

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

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

25 MR. ROSENTHAL: Yeah, Jack Rosenthal,

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1 Research.

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

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

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

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

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

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

9 We had some interesting -- we had three  
10 pairs of tests. If you go back and look at both the  
11 NSRR and the Cabri test, you can find three pairs of  
12 tests were -- Span 5 and Span 3 were tested, and each  
13 of those three pairs, the Span 5 failed, and the Span  
14 3 didn't fail. They had exactly the same burnup  
15 level, but their oxide thicknesses were quite  
16 different.

17 CHAIRMAN POWERS: Okay.

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

19 CHAIRMAN POWERS: Please.

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

22 CHAIRMAN POWERS: Well --

23 DR. MEYER: But I'll go on.

24 So the next PIRT that we did was for  
25 boiling water reactor and for power oscillations that

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1 were not stopped by a SCRAM, and this is an accident  
2 for which we do not have clear regulatory guidance,  
3 but for which GE has in the past done some analysis  
4 and have used the same 280 calorie per gram limit to  
5 show adequacy in this analysis.

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

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

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

22 It takes about eight seconds for a fuel  
23 rod to transfer its heat out. So this is less than  
24 the time constant of the fuel rod. So if you look at  
25 this part, it looks like the rod ejection accident on

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1 a small scale. These little pulses have about 15,  
2 one, five, calories per gram in them instead of 50 or  
3 100 calories per gram.

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

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

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

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

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1 in the rod.

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

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

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

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

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1 chemical process and how much can you pull away?

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

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

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

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

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

23 So we just focused on this one. I think  
24 that, in fact, a lot of what we learn for the PWR rod  
25 ejection accident in terms of fuel behavior and damage

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1 limits can be transferred, but not all of it because  
2 the Japanese continue to study BWR power pulse events  
3 and have recently looked at some high burnup BWR  
4 cladding in their NSRR reactor and find unusual  
5 behavior that hasn't been seen before that seemed to  
6 be related to the bonding between the pellets and the  
7 cladding, which in the BWR cladding that they were  
8 looking at has this soft zirconium liner on the ID.

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

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

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

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

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

25 Anyway, now --

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

4 DR. MEYER: Not for BWR rod drop.

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

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

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

14 DR. MEYER: Right.

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

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

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

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

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1 DR. BONACA: Okay.

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

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

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

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

20 DR. MEYER: Yes, yeah. We talked at  
21 length about the test, and they don't have to do them  
22 every three seconds. They might do them every three  
23 days. They just do one, leave it in there, raise the  
24 temperature up a little bit and do another one, and if  
25 you do two or three of these, you can probably see

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1 what is going to happen or not going to happen, and so  
2 that's the kind of repeated pulse testing that's being  
3 talked about for NSRR.

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

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

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

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

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

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

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

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

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

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

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

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

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

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1 and you're less vulnerable to the brittle failure.

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

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

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

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

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

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

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

14 DR. MEYER: We do it in house.

15 DR. CRONENBERG: Okay.

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

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

23 DR. MEYER: Gee, I don't know whether  
24 anybody on the staff can run it, but David Diamond at  
25 Brookhaven is doing the rod ejection calculations for

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1 us. PARCS is a Purdue University developed code, but  
2 it's run other places, and David runs it at  
3 Brookhaven.

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

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

8 MR. ROSENTHAL: Yes, sir.

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

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

25 CHAIRMAN POWERS: You don't even need to

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1 understand the momentum equation.

2 (Laughter.)

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

5 CHAIRMAN POWERS: We're desperate.

6 DR. MEYER: They also found that for the  
7 loss of coolant accident that the cladding type was  
8 very important, but the most interesting result of the  
9 discussions on the loss of coolant accident was the  
10 second bullet where George Hache from IPSN in France  
11 got up and gave us a summary of our own U.S. history  
12 of the development of the ECCS criteria and reminded  
13 us that the embrittlement criteria, these numbers  
14 2,200 degrees Fahrenheit and 17 percent oxidation  
15 were, in fact, based on ring compression tests made by  
16 Hobson in the early '70s, and that the quench tests  
17 were only confirmatory because there had been a lot of  
18 discussion about whether the quench tests could  
19 reasonably represent the axial forces or other  
20 constraints that might be on a fuel rod during the  
21 quench.

22 I should say that another way. The  
23 discussion was that the external forces on the fuel  
24 rod, whether they come from the quenching process or  
25 from some other source, including things like

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1 earthquake, could be adequately represented in these  
2 quench tests. It was felt that they could not, and so  
3 the quench tests were used only as confirmatory tests,  
4 and the criteria themselves were derived from these  
5 ring compression tests.

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

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

24 So all of a sudden light bulbs are going  
25 off. Here is some information on a similar alloy that

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1 shows a marked reduction in the amount of oxidation  
2 that can be tolerated during a loss of coolant  
3 accident.

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

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

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

25 The work started with real specimens last

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1 summer when we received the BWR rods from the Limerick  
2 plant, and it's slow going. We have done a number of  
3 the oxidation kinetics measurements, and I can just  
4 give you a qualitative result of that.

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

11 CHAIRMAN POWERS: And do you exceed Baker-  
12 Just?

13 DR. MEYER: I'm sorry?

14 CHAIRMAN POWERS: Do you exceed Baker-  
15 Just?

16 DR. MEYER: I don't think so.

17 CHAIRMAN POWERS: That's harder to do.

18 DR. MEYER: Yeah, it would be harder.

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

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

25 The principal interest in the Halden

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

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

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

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

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

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

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

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

21 DR. MEYER: Yeah.

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

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

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

8 MR. SCOTT: It's about that.

9 DR. MEYER: Sud knows the numbers for  
10 that.

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

18 DR. MEYER: I know that Dana is concerned  
19 about some stressed that might be applied on the way  
20 up. We have, in fact, focused more on the way down  
21 and have given more attention to the cool down part of  
22 this because this is when the oxygen and hydrogen and  
23 distributing themselves in the alpha phase and in the  
24 prior beta phase, and we believe that the cool down  
25 conditions are going to ultimately determine what the

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1 ductility is, and then you do the test when you're  
2 down at a relatively cold temperature. You run it  
3 through the oxidation transient, come down, and then  
4 the ultimate challenge is near the end down at the low  
5 temperature.

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

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

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

25 DR. MEYER: I think if it had a cool down

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1 period prior to the ultimate cool down and quench  
2 following a long period at a very high temperature,  
3 then you might have some interesting effects. It was  
4 my impression that the ups and downs occurred at a  
5 relatively low temperature as you're approaching this  
6 high temperature period, and I don't think those would  
7 have a very big effect because you still have ductile  
8 cladding and a very small amount of the oxidation  
9 taking place.

10 We can continue to --

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

14 DR. MEYER: Yeah.

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

19 DR. MEYER: Okay.

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

25 I think you'll find though --

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

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

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

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

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

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

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

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

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

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

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1 temperature condition is the limiting case.

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

6 MR. NISSLEY: Mitch Nissley, Westinghouse.

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

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

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

23 DR. KRESS: Ralph, the research and the  
24 PIRTs deal with three design basis type of accidents.  
25 Are you planning an additional PIRT to look at severe

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1 accidents and effects on the core melt behavior and  
2 fission product release?

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

6 DR. KRESS: Yeah.

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

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

14 DR. MEYER: Yes.

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

16 DR. MEYER: Yes.

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

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

23 Jack, can you?

24 MR. ROSENTHAL: Yeah, in response to the  
25 ACRS report, et cetera, we're just now planning out

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1 how to take on the iodine spiking issue. So actually  
2 it's a very timely comment, and in my own mind you  
3 make so much iodine per fission, and it's a question  
4 of where is that iodine before the hypothesized event  
5 occurs. Is it in the fuel or the gap, or is it  
6 already outside in the --

7 DR. KRESS: I think that is very relevant.

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

13 DR. KRESS: Yeah.

14 MR. ROSENTHAL: A different project.

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

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

22 MR. ROSENTHAL: Right.

23 DR. MEYER: And finally, I just want to  
24 mention EPRI's cooperation in the big program at  
25 Argonne National Laboratory and to say to you that we

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1 finally have the H.B. Robinson fuel rods in a hot  
2 cell.

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

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

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

12 DR. MEYER: Or some other time.

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

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

23 CHAIRMAN POWERS: When I get around to it.

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

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1 the record at 10:33 a.m.)

2 CHAIRMAN POWERS: Let's come back into  
3 session.

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

6 DR. MEYER: Garry Garner will give the  
7 presentation.

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

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

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

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

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

24 I've pared that down a little bit for this  
25 morning. We had our in-house LOCA man give part of

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1 these results, and I gave primarily the mechanical  
2 test results at the end. It's just me this morning.  
3 I'll, of course, try to answer all of your questions.  
4 If I don't know the answer, I'll defer, and we'll get  
5 it for you.

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

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

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

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

25 And then that is just as a way to set the

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1 table for the LOCA, post LOCA discussion that will  
2 follow, and we'll talk about the oxidation tests that  
3 we did, the quench tests, and the post quench  
4 mechanical testing, and then we'll follow with a brief  
5 conclusion and a summary.

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

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

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

23 So when people talk about M5 being of a  
24 similar nature, similar chemistry to other alloys,  
25 yeah, on the surface, but on the other hand, very

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1 small changes can have very drastic effects.

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

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

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

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

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

25 So just to make sure that we always get

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

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

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

17 The two properties that I would highlight  
18 this morning are the corrosion, and you've seen these  
19 kind of curves before. This curve -- and I apologize.  
20 It's hard to read because it is just so small on this  
21 viewgraph -- but it's the maximum oxide thickness  
22 versus fuel rod average burnup, and you can see that  
23 all of the colored dots are M5 data points. They come  
24 from a wide variety of reactors, from 14-14 to 17 by  
25 17, and the colors are just differentiating those.

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

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

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

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

15 MR. GARNER: Yeah, the red ones.

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

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

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1                   Yeah, when we get more data out here for  
2                   16 by 16s, we'll see what that's doing, but so far I  
3                   would point out that the max oxide there is 41  
4                   microns, and only in that point.

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

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

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

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

25                  As you can see on this curve, it's going

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1 to be less than 100 if this trend continues out here  
2 as we expect it, of course, to do beyond 60 gigawatt  
3 days.

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

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

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

18 DR. CRONENBERG: When did M5 go into use?  
19 '95?

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

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

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

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

8 MR. ALDRICH: Mike Aldrich in Framatome.  
9 I think right around 46 peak rod.

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

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

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

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

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

25 Okay. Now, I would talk about the results

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1 in the high temperature testing, the oxidation quench  
2 test and post mechanical quench test. It was called  
3 the CINOG. That was the facility in Grenoble where  
4 the work was done, and beyond that I don't even know  
5 what CINOG means.

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

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

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

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

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

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

8           For instance, in like I say it was 700 to  
9           1,400. At 1,400, Zirc-4 and M5 oxidation kinetics are  
10          right on top of each other. If you had the time and  
11          inclination to go through this legend, you'll see that  
12          at that temperature they're the same. At 1,250  
13          they're the same. At 1,150 they're they same. At  
14          1,100 they're the same.

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

18          Now, I didn't draw lines through the data,  
19          but the NFI did some independent research on our  
20          alloy, on M5, and got the same results, and that's  
21          what you see right here. The open triangles are Zirc-  
22          4, and the closed triangles are M5. So, again, you're  
23          seeing that behavior.

24          Mr. Lebourhis at the OECD meeting two  
25          weeks ago in France presented the results on this

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1 curve of another French test at 1,000 degrees saying  
2 the same thing.

3 And then down here at 900 again, the  
4 alloys are having the same oxidation kinetic again,  
5 900, 800, and 700. So this area here between 1,100  
6 and 1,050, lower than 1,100 and greater than 900, the  
7 M5 alloy is clearly oxidizing at a lower rate. It's  
8 the only place in that spectrum that that's happen.

9 I put the 17 percent for folks that want  
10 to think about weight gain in terms of the ECR, the  
11 equivalent clad reacted. It's right in there, about  
12 24, 25 milligrams per centimeter squared. So that's  
13 about 17 percent ECR.

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

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

21 MR. GARNER: I don't, no.

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

24 MR. GARNER: Yes, yes. You know, and the  
25 alloy -- we know that the chemistry of the alloy has

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1 to do with what temperature that phase transition goes  
2 in and like that. That's certainly the speculation,  
3 but I'm not an expert on that. I don't know exactly  
4 why that is, but it's very well documented, and it is  
5 confirmed.

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

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

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

16 MR. GARNER: Right.

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

18 MR. GARNER: It's all consistent.

19 DR. CRONENBERG: It's all consistent?

20 MR. GARNER: Yes, sir, yeah.

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

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

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

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

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

24           MR. GARNER: We did have a slide in the  
25 presentation at the end of February where we showed

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1 consistency with the Prater-Cartwright data, yes, and  
2 I can --

3 DR. CRONENBERG: It's less?

4 MR. GARNER: Yes.

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

7 MR. GARNER: Yes.

8 DR. CRONENBERG: Okay.

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

11 CHAIRMAN POWERS: Radiation has no impact  
12 on these?

13 MR. GARNER: Excuse me?

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

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

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

24 And you see for Zirc-4 that you do have  
25 this layer, this flakiness, this layering. It's a

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1 trace amount of it, but it is present, and we saw that  
2 on both the inner layer and the outer layer of the  
3 Zirc-4 samples.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

24           This is the Leistokov correlation points,  
25 lower understandably, and uniformly. And then this is

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1 our data. We're plotting failure points up here, and  
2 as you can see, at the 1,300 degrees temperature, the  
3 red line is the 17 percent linking the criterion, and  
4 so that's a failure point at 1,000 degrees, and it  
5 took four and a half hours to get there.

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

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

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

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

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

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

9 The M5 accident survival is definitely  
10 superior to Zirc-4. At temperatures greater than  
11 1,100 they're about -- they're the same. At  
12 temperatures less than 1,100, it's surviving up to two  
13 times longer than Zirc-4. That's consistent with  
14 those oxidation curves and that small band of  
15 temperatures where M5 has the greater oxidation  
16 resistance.

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

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

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1 again, we agree with the criterion.

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

9 This was the test matrix. We tested at  
10 1,100 degrees C. We did it for times that would give  
11 ECRs from three to 17 percent. This series of tests  
12 was a single face oxidation, and again, we used as  
13 fabricated M5 and compared it to Zirc-4 cladding.

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

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

23 It's not very interesting down here to  
24 test M5 because it takes so long to get anywhere close  
25 to 17 percent. It's really out of the realm of what

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1 we're interested in. So we picked 1,100 degrees,  
2 where the two alloys are oxidizing at a fairly steep  
3 rate, and they're oxidizing the same. A test in that  
4 region might learn something was the thought.

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

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

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

18 MR. GARNER: No.

19 DR. CRONENBERG: No?

20 MR. GARNER: No.

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

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1 weight gain.

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

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

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

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

25 Now, in contrast to that, the M5 oxide

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

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

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

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

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

22 Now, to compare that with what people have  
23 observed in some of E110 alloys, this is a picture  
24 that was not in Mr. Bohmert's paper. It is in a  
25 Russian report, and I can give you the reference of

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1 that if you need that, and in a second, I'm going to  
2 show you a quote from Mr. Bohmert's paper where he  
3 describes in words what he's seeing here, and other  
4 folks have seen this, too.

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

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

14 The point here is that it's a very  
15 stratified and cracked oxide layer, and it has a  
16 completely different morphology than M5. In words,  
17 Mr. Bohmert said that not at a late stage -- that  
18 photograph that I just showed you was taken after like  
19 9,000 seconds -- but Mr. Bohmert and his work said  
20 that at an early stage he found the same thing in  
21 multi-layer oxide scales formed which tend to flake.  
22 We saw that flakiness in the Zirc-4.

23 And, again, we just didn't see that. We  
24 don't see it in M5. We've never seen that kind of a  
25 morphology, and in the quench test, you can see that

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1 when we weighed the amount of oxide that's falling  
2 off, falling off, flaking off, it's not there.

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

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

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

10 MR. GARNER: No.

11 DR. CRONENBERG: No?

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

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

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

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

3 MR. GARNER: They use the one percent.

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

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

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

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

25 When you look at the fracture surface like

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

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

9 MR. GARNER: A LOCA transient.

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

12 MR. GARNER: Yeah. Well --

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

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

20 DR. SHACK: Okay.

21 MR. GARNER: Yeah.

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

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

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1 composition.

2 DR. CRONENBERG: Okay.

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

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

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

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

24 DR. CRONENBERG: But it's also time and  
25 temperature for annealing and so it's not sorted out

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1 then. You said you think it's probably chemistry and  
2 trace.

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

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

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

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

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

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

8           MR. GARNER: Yes, it is.

9           DR. CRONENBERG: Oh, it is?

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

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

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

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

20          MR. GARNER: Right, right.

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

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1 tube material at all.

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

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

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

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

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

25 Every time we tweak something like a

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

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

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

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

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

25 MR. GARNER: Right.

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

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

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

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

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

21 MR. GARNER: Oh, yes, sir.

22 MR. ALDRICH: As far as the deployment of  
23 the alloy and the fuel surveillance section of the SER  
24 for M5, we are required to take additional PIE data of  
25 things like you're referring to, control rod

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1 insertability, as the burnup of the fuel in reactor  
2 exceeds higher and higher levels up to the license  
3 limit, we are required to take PIE data. So that type  
4 of performance would be verified.

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

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

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

17 MR. GARNER: Yes.

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

23 MR. GARNER: Yes.

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

25 MR. GARNER: For a given weight gain we

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

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

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

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

25 Where we differed was where we compared

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

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

15 These two alloys, while nominally Zirc one  
16 percent --

17 DR. CRONENBERG: They are not the same.

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

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

23 So just to conclude just a summary of the  
24 post quench mechanical test, we tested in the Bohmert  
25 range. We tested at that 1,100 degrees temperature

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1 and for the reasons that I tried to explain. That's  
2 where the two alloys, Zirc-4 and M5, are oxidizing at  
3 the same rate so that you can see what's really going  
4 on there with those guys.

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

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

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

18 Now, just one last slide to summarize the  
19 entire high temperature, oxidation, quench, post  
20 quench mechanical test results. I hope that I've  
21 demonstrated this morning that the M5 in reactor  
22 operating performance is clearly superior to the Zirc-  
23 4; that our LOCA/post LOCA oxidation rates are equal  
24 to or a little bit slower than Zirc-4 and  
25 significantly slower in certain temperature ranges.

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

3 The performance is acceptable and is equal  
4 to or better than Zirc-4 of events of equal duration.  
5 For a low oxide it takes an awful long time to get to  
6 17 percent ECR. If you had the ultimately perfectly  
7 alloy that didn't oxidize at all, you'd never get  
8 there. So some consideration of time has to be taken  
9 into consideration, and I think everybody does, and  
10 that's why we agree that the 17 percent criterion is  
11 valid, if you consider how long it takes to do that.

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

14 So that concludes that I had to say.

15 CHAIRMAN POWERS: Thank you.

16 Any other comments for the speaker?

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

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

22 CHAIRMAN POWERS: Okay.

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

25 CHAIRMAN POWERS: Okay.

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

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

4 MR. GARNER: Thank you.

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

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

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

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

23 Our purpose here is to give you an  
24 overview of some of our current work in determining  
25 the properties of both Zircaloy-4 and ZIRLO after high

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1 temperature oxidation and steam. Again, this is an  
2 ongoing program. We started it in late January, early  
3 February as a result of some of the information  
4 discovered by Dr. Meyer. It's an ongoing program. It  
5 has still some time to go to completion, but we do  
6 want to give you an update on what we've discovered so  
7 far.

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

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

24 And the purpose of the criteria was,  
25 again, to insure cladding would remain sufficiently

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1 intact to assure easily coolable geometry, and as a  
2 practical matter, they met that criteria simply by  
3 assuring themselves that after the transient was  
4 completed, the cladding would retain some ductility.  
5 So basically it's a ductility retention after the  
6 LOCA.

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

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

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

24 We have taken extensive in pile  
25 measurements both on the growth, corrosion, creep,

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1 growth, both axial growth of the rods and the  
2 assemblies and lateral growth of the grids. Generally  
3 we find that for equivalent corrosion duties, the  
4 corrosion is probably 60 percent of what we get for  
5 Zircaloy-4. Creep and growth are about half.

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

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

11 DR. UHRIG: One question.

12 MR. LEECH: Yes, sir.

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

15 MR. LEECH: It is one percent nominal tin.

16 DR. UHRIG: What?

17 MR. LEECH: One percent nominal tin, yes.

18 DR. UHRIG: One percent.

19 MR. LEECH: Again, we started licensing  
20 this in 1991. The firm formal licensing process was  
21 initiated, and there was an extensive testing program  
22 that supported that included material mechanical  
23 properties, density, thermal expansion, thermal  
24 conductivity, specific heat, phase changes, high  
25 temperature creep, high temperature oxidation at rod

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1 burst. Plus there was an extensive irradiation  
2 program in the BR-3 reactor.

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

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

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

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

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

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

23 Now, as I say, that gets complicated  
24 because that doesn't necessarily mean the thermal  
25 creep. Out of pile thermal creep is lower. The two

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1 components really interact, and we find must less  
2 irradiation creep.

3 So the overall in reactor creep rate is  
4 much less.

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

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

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

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

16 So because we saw that the mechanical  
17 properties were essentially identical during the  
18 licensing process, we argued that because of the close  
19 similarity of Zircaloy, ZIRLO and Zircaloy-4, which  
20 again has been described to others as simply Zircaloy-  
21 4 with a little niobium added, that we thought that  
22 the 17 percent criteria should continue to apply, that  
23 no additional testing was necessary.

24 The NRC agreed with that, and 10 CFR 5046  
25 was amended to say state that the acceptance criteria

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1 applies to ZIRLO. So that was our licensing history  
2 on ZIRLO.

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

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

19           However, in looking at that, we also  
20 noticed a number of physical differences in the oxide  
21 layers of E110 and Zircaloy-4, and several of the  
22 things that Bohmert mentioned was E110 displays a  
23 heterogeneous appearance to the oxide layer; that  
24 typically if we look at the oxide layer, there were  
25 two separate oxide layers separated by cracks, and

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1 these tend to play -- multi-oxide layers do tend to  
2 play, and his tests, the Zircaloy-4 always had a  
3 glossy black, firmly adherent single layer, relatively  
4 free from mechanical failures, and he noticed a high  
5 hydrogen uptake -- low hydrogen uptake. I'm sorry.  
6 He noticed low hydrogen uptake only if firmly  
7 adherent, crackless oxide layers were formed.

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

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

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

23 So we did. Having said that though, let  
24 me reiterate that one other thing we wanted to look at  
25 was clearly to make the point that ZIRLO and E110 are

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1 not equivalent for a number of reasons, and the number  
2 one reason of course is that ZIRLO also contains tin  
3 here at the one percent level, a substantial amount of  
4 tin. It contains iron. The iron level is a tenth of  
5 a percent, and it does contain oxygen. The spec on  
6 oxygen is about .125 percent, or 1,250 parts per  
7 million, whereas in E110 it's typically 700 parts per  
8 million. So there are some differences.

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

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

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

25 So we're putting both tubing types in and

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1 testing them simultaneously. They're held in here.  
2 Basically there's a sheath thermocoupler that goes up  
3 here. It has a small ring on it, and we sit the  
4 samples on top of that.

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

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

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

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

21 We have, again, I said that there was a  
22 main sheath thermocouple coming up through here which  
23 sits in the middle of the tubes. So we have  
24 temperatures -- both two temperatures on the outside  
25 and then the temperature on the inside, and typically

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1 they're within three or four degrees of each other.  
2 So we are getting fairly uniform heating.

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

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

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

20 MR. NISSLEY: For a large break LOCA,  
21 typical heat-up rates would be on the order of ten to  
22 15 degrees Fahrenheit per second. Small break LOCA  
23 might be as low as two or three degrees Fahrenheit per  
24 second. So that is a little low.

25 MR. LEECH: Okay. So this is somewhat

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

5 The final temperatures when we got the  
6 temperatures ranged from 1,800 degrees Fahrenheit to  
7 2,200 degrees Fahrenheit, which is, I believe, 986  
8 degrees Centigrade to 1,204 degrees Centigrade.

9 We did run another test. We've run one at  
10 1,700 degrees Fahrenheit, which is 926 degrees  
11 Centigrade, because as we'll discuss later, there was  
12 some concern that there was a temperature range  
13 between 950 and 1,000 identified by Bohmert where he  
14 seemed that the E110 alloy was particularly  
15 susceptible to hydrogen pick-up. So we ran that test.

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

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

24 MR. NISSLEY: A pretty good cool-down.

25 MR. LEECH: Pretty reasonable with what we

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1 might actually expect.

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

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

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

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

22 First of all, the number one priority is  
23 oxide layer characteristics. We believe that of all  
24 the things that we've seen with E110 and Zircaloy-4,  
25 that seems to be the biggest difference, and we want

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1 to take care to look at those. We're doing those by  
2 optical metallography and just general observations.

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

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

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

23 There are some other differences. Bohmert  
24 cut his specimens into short sections prior to  
25 oxidizing them, where we oxidize a specimen about that

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1 long, and then we cut the rings out afterwards.

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

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

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

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

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1 percent is going into the metal.

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

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

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

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

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1 the metal.

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

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

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

20 Again, one other thing I might mention,  
21 too, which I haven't plotted, haven't shown you.  
22 We're also doing this at room temperature, and we've  
23 looked at some of the preliminary results that we got  
24 for Zircaloy-4 at room temperature, and they're  
25 reasonably in good agreement with what Bohmert got in

1 his test for Zircaloy-4, which again is another,  
2 probably a second opinion that what he did was really  
3 pretty good work. There was no problem with what he  
4 did. It's just that the E110 seems to be  
5 substantially different than Zircaloy-4 because our  
6 Zircaloy-4 results seem to be consistent with his.

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

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

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

25 Again, we still have some more work to do

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

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

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

19 That really is what I planned to say  
20 today.

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

24 MR. LEECH: Right.

25 CHAIRMAN POWERS: And the previous speaker

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1 showed some micrographs in Zircaloy-4 that had  
2 evidence of delamination.

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

6 After spalling it certainly showed  
7 delaminations.

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

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

12 CHAIRMAN POWERS: Okay.

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

15 CHAIRMAN POWERS: Okay.

16 MR. LEECH: I don't know.

17 CHAIRMAN POWERS: Any other questions of  
18 this speaker?

19 (No response.)

20 CHAIRMAN POWERS: Well, thank you very  
21 much.

22 MR. LEECH: Thank you.

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

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1 OECD meeting after lunch.

2 Thank you.

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

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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.

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

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

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

19 Now, you saw a few of these points on  
20 Framatome's slide, where they picked out the ones at  
21 1,100, and they picked those out from Bohmert's slide  
22 and showed them on their graph.

23 But Bohmert had tested over a wide range  
24 of temperatures, both Zircaloy-4 and the VVER  
25 cladding, E110. And you can see this is the line that

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1 was on the Framatome slide. And you can see it coming  
2 down here around five percent cladding reacted.

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

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

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

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

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

25 And then in addition to that, George

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

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

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

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

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

23 There were actually two papers on the  
24 subject from Russia and I only have a slide from one  
25 of them. The other one was kind of preliminary, and

1 frankly, I was never able to understand the main  
2 results of that paper and have gone back to try and  
3 get clarification.

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

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

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

21 So all of these laboratories appear to be  
22 able to make consistent measurements on Zircaloy, and  
23 we get these two sets of differences for the zirconium  
24 1-niobium, and the difference is remarkable. It's not  
25 just a small difference. I mean, from 17 percent to

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1 six percent is a huge reduction.

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

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

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

25 But this is a failure map for Zircaloy

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

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

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

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

22 Okay. Now, I'm not trying to suggest that  
23 this is the same message, but this morning in the  
24 Framatome presentation we did see numbers that were  
25 close to the 17 percent line which don't have a lot of

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1 margin exhibited. I don't know whether that's  
2 significant or not significant, but I point it out to  
3 you.

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

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

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

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

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

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

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

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

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

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

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1 DR. BONACA: Does it show sulfur there?

2 DR. MEYER: Huh?

3 DR. BONACA: Does it show sulfur?

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

6 DR. BONACA: We heard this morning  
7 about --

8 DR. MEYER: Yeah.

9 DR. BONACA: -- M5, I thought.

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

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

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

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1 know.

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

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

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

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

21 Hee Chung now points out that for  
22 Zircaloy, that there seems to be a threshold around  
23 600 or 700 ppm hydrogen. When you get that much  
24 hydrogen in the specimen, then it also contributes to  
25 the reduction of ductility.

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

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

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

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

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

24           And then there's another process that lets  
25 hydrogen into the cladding associated with the

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1 deformation during ballooning and rupture.

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

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

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

23 And then I was asked to give a brief  
24 presentation on our research program, and these are a  
25 couple of slides that I used. The first bullet

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1 outlines the program that we have at Argonne at the  
2 present time using Zircaloy. There have been some  
3 adjustments to this based on the PIRT process that was  
4 completed.

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

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

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

21 And so, in particular, we're quite sure  
22 that we'd want to do oxidation kinetics measurements,  
23 probably some sort of thermal shock test, look at the  
24 oxidation and the phase relations and measure the  
25 mechanical properties after running the material

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1 through a high temperature oxidation transfer.

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

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

12 DR. KRESS: Okay. This will do that.

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

22 So we want to look very carefully at ring  
23 compression tests, whether that's the right test or  
24 not. These tests have been criticized in the past.  
25 They're not real precise. They're good screening

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1 tests for some purposes, but maybe an axial tensile  
2 test might be a more precise way of looking at the  
3 ductile brittle behavior.

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

8 DR. MEYER: Yeah.

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

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

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

21 DR. MEYER: Yeah.

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

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

5           DR. MEYER:   It's not really connected,  
6           although you come out at about the same place.   The  
7           100 calorie per gram dealt specifically with a rod  
8           ejection type accident.   And that's a accident where  
9           the cladding remains at a relatively low temperature,  
10          and where you haven't gone through a phase  
11          transformation and wiped out its fabrication history  
12          and all of that.

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

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

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

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

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

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

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

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

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

16 DR. CRONENBERG: Okay.

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

24 There's a lot that we have learned and I  
25 think we sill can learn with the unirradiated tubing,

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1 and so if we can make some arrangement with Framatome  
2 and Westinghouse to work on their materials, we'd like  
3 to get started very quickly on the unirradiated  
4 tubing.

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

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

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

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

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

22 DR. MEYER: Yes.

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

25 DR. MEYER: Why?

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1 CHAIRMAN POWERS: -- what other program is  
2 there?

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

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

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

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

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

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

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

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

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

13 DR. MEYER: Okay.

14 CHAIRMAN POWERS: And thank you for your  
15 presentations.

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

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

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

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

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

9 DR. KRESS: We messed up your running?

10 MS. CHATTERTON: You messed up my running.

11 CHAIRMAN POWERS: You should have  
12 protested.

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

17 CHAIRMAN POWERS: That's right.

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

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

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

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1 DR. KRESS: Oh, okay.

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

3 DR. KRESS: They've got you.

4 MS. CHATTERTON: You can't cheat.

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

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

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

19 CHAIRMAN POWERS: Okay.

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

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

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

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

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

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

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

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

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

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

15 MS. CHATTERTON: Yes.

16 DR. APOSTOLAKIS: But it deals with delta  
17 LERF.

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

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

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

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

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

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

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

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

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

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

17 DR. APOSTOLAKIS: Says what?

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

21 DR. KRESS: Absolutely.

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

24 DR. KRESS: That's right. That's exactly  
25 the way I was thinking.

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1 DR. SHACK: The source term is different.

2 DR. KRESS: Yeah, maybe. We don't know.

3 DR. APOSTOLAKIS: The quantitative health  
4 objectives don't change, do they?

5 CHAIRMAN POWERS: We assume those are are  
6 given to us by God.

7 DR. APOSTOLAKIS: Working backwards, you  
8 have assumed certain behavior in severe accidents.  
9 And that's what's going to change?

10 DR. KRESS: Yes.

11 DR. APOSTOLAKIS: Okay. So the LERF value  
12 then may change.

13 DR. KRESS: That's what I was saying.

14 DR. APOSTOLAKIS: Now, the CDF will not  
15 change?

16 DR. KRESS: No.

17 DR. APOSTOLAKIS: Because we lowered it by  
18 a factor of ten arbitrarily. Right?

19 CHAIRMAN POWERS: I mean, I don't know.  
20 I mean, it seems to me --

21 DR. KRESS: Must have had a reason for  
22 that.

23 CHAIRMAN POWERS: Maybe it turns out that  
24 things are more susceptible to core damage.

25 DR. KRESS: Yeah.

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1 DR. APOSTOLAKIS: It's more than a factor  
2 of ten what you be now. I mean there's a problem  
3 somewhere. But anyway, it might be that that --

4 CHAIRMAN POWERS: Factors of ten are not  
5 our of the question here.

6 DR. APOSTOLAKIS: Now, you said some parts  
7 may be risk informed. So you have decided that some  
8 parts may not be?

9 MS. CHATTERTON: No.

10 DR. APOSTOLAKIS: Okay. Just a figure of  
11 speech.

12 MS. CHATTERTON: Yes, a figure of speech,  
13 but basically we're letting the industry propose how  
14 they want to handle -- how they think is the  
15 appropriate way and to provide a justification, and  
16 again this will go and do the review.

17 CHAIRMAN POWERS: I guess, when you raise  
18 the issue of being risk informed, the challenge I see  
19 there has something to do with just what our  
20 discussion was. We typically don't have a great deal  
21 of information on these fuels under accident  
22 conditions, severe accident conditions that will give  
23 you any consequence.

24 Are you saying that the industry can come  
25 in, but they've got to come in armed with information

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1 on fuel behavior under accident conditions?

2 MS. CHATTERTON: That might be an option  
3 if the proposal is to go that direction. The main  
4 point is whatever method they decide to take, they  
5 have to provide the justification for why that's  
6 acceptable, with a great deal of emphasis on lead test  
7 assemblies.

8 That's one thing that we have emphasized  
9 greatly in the last few years, I would say in the last  
10 five years, and that's a result of fuel issues that  
11 we've had in these last five or so years, and I'll be  
12 talking about some of those later, and the things that  
13 we've learned that the fuel -- the lead test  
14 assemblies in the past did not always give us data or  
15 information that was really the most useful.

16 We've also said that a breath (phonetic)  
17 extension program will also have a fuel performance  
18 monitoring program. Somebody said the fuel  
19 performance monitoring; that's in core. I guess maybe  
20 it's really fuel surveillance program.

21 DR. BONACA: If you'd just stay with that  
22 slide, I would like to ask a question.

23 MS. CHATTERTON: Sure.

24 DR. BONACA: Clearly, the first bullet I  
25 can see that you are concerned about how long the

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1 cycle is going to be or the burnup.

2 MS. CHATTERTON: Yes.

3 DR. BONACA: And the issues that we  
4 discussed this morning. At the bottom there, I see  
5 fuel performance monitoring program. Now, currently,  
6 I mean, although it may be a concern to have fuel  
7 failures, the one percent for the fuel assumptions in  
8 analysis allow for 50 pins probably are going to be  
9 failed.

10 Okay. So I'm curious about what would  
11 this fuel performance monitoring program mean. I  
12 mean, for example, some of the Westinghouse plans have  
13 exhibited at times maybe four or five 17's failed in  
14 some batches. Okay. That's really an operational  
15 concern.

16 Is it also a regulatory concern right now?  
17 Is that what it's focusing on?

18 MS. CHATTERTON: This isn't focusing just  
19 on fuel failures.

20 DR. BONACA: Yeah.

21 MS. CHATTERTON: This is focusing on  
22 things like corrosion, growth.

23 DR. BONACA: Okay. I understand.

24 MS. CHATTERTON: It's focusing on all the  
25 types of parameters that -- I want to characterize it

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1 fairly by saying in the past many times the fuel has  
2 been burned, taken out, put in the spent fuel pool,  
3 and never looked at.

4 DR. BONACA: I understand.

5 MS. CHATTERTON: As a result we had some  
6 problems that might have been eliminated had the type  
7 of program that I'm talking about --

8 DR. BONACA: So for example, oxidation  
9 rates because those also go in estimation of  
10 performance under accident conditions.

11 MS. CHATTERTON: Yes.

12 DR. BONACA: Okay. I understand.

13 MS. CHATTERTON: Yes. And if you're not  
14 measuring your oxidation levels, you don't know if  
15 your inputs to your accident analysis are correct.

16 DR. BONACA: Okay. Thank you.

17 MS. CHATTERTON: And that's the main point  
18 in that.

19 DR. CRONENBERG: Margaret, on that, the  
20 NRC used to publish a fuel performance summary. Every  
21 year PNL used to do the work.

22 MS. CHATTERTON: Yes.

23 DR. CRONENBERG: They used to summarize  
24 it. That's no longer in effect.

25 MS. CHATTERTON: That's correct.

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1 DR. CRONENBERG: Are you going to  
2 reinstitute this type of summary like the PNL used to  
3 do, but EPRI or industry or somebody will be -- will  
4 it be a formal, published monitoring program?

5 MS. CHATTERTON: I don't think we're far  
6 enough along to really be able to say exactly how  
7 that's going to work. How I envision when we will  
8 come up with a reg. guide will be it listing the types  
9 of testing that needs to be done, giving some ideas as  
10 to the frequency and when.

11 For instance, if you fuel goes beyond 62,  
12 but it's only to 63, and it's ten years down the line,  
13 it probably doesn't need to be measured again.

14 On the other hand, the different type of  
15 fuel, the slightly different power history, some of  
16 those, it's going to be difficult to come up with  
17 exactly how we handle this. There's going to be a lot  
18 of thought into that such that it provides enough  
19 data, but it doesn't totally hamper the industry such  
20 that they have to measure everything because that's  
21 not the intent.

22 It's going to have to be set up with  
23 controls such that after so much data, there's not  
24 need. If, on the other hand, if results aren't  
25 turning out to be good, then you need more. And it's

1 going to have to have triggers in it for when you do  
2 more results, when you do more testing and also when  
3 you would need to do a hot cell.

4 Most of the hot cell exams, most of this  
5 is going to be pool site exams. The types --

6 DR. BONACA: That's what it was. The PNL  
7 was mostly pool site exams.

8 MS. CHATTERTON: Yes. Oh, yes.

9 DR. BONACA: But we don't have that data  
10 anymore.

11 MS. CHATTERTON: We don't have that data  
12 anymore.

13 DR. BONACA: I would hope that if you're  
14 going to push it to 70-75, that type of program goes  
15 on for a few years until you've had that --

16 MS. CHATTERTON: Yes. And that's the type  
17 of thing that I think we're thinking about. Yes, I  
18 miss having those reports.

19 DR. BONACA: Yeah.

20 MS. CHATTERTON: Those are great.

21 DR. BONACA: There was a lot of data.

22 MS. CHATTERTON: Where are we in this  
23 whole plan?

24 Well, in the last year or so there's been  
25 some progress. I would say not a tremendous amount.

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1 Although the industry is working on it and it's slow,  
2 sometimes it comes in big steps.

3 We had a draft submittal in March of 2000,  
4 and the staff provided comments, and we had another  
5 meeting with NEI December 6th. They outlined their  
6 approach for RAA, and the staff gave them comments  
7 saying that it looked fairly reasonable.

8 And basically what they're doing is  
9 proposing a clad failure and coolability limits that  
10 are a function of burnup. They are based on enthalpy  
11 increase, and we've seen the preliminary work on this.  
12 We haven't seen all the details; we haven't reviewed  
13 all the details.

14 What they presented looks like a  
15 reasonable approach. Again, once it's submitted we  
16 will do a complete review of it.

17 We expect a submittal late summer. Again,  
18 sometimes work takes much longer than they think.  
19 Originally that was an early 2001 date, and it's been  
20 changed.

21 CHAIRMAN POWERS: If you get a submittal,  
22 say, in August, when do you think you'd have your  
23 review finished?

24 MS. CHATTERTON: This submittal I expect  
25 in August will not be a complete submittal. This will

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1 be a partial submittal and it will depend on the  
2 amount that's in it.

3 I think a submittal like this is going to  
4 take us a considerable amount of time, six months or  
5 so I would say on half of it, possibly a year or more  
6 on the complete package.

7 If it comes in in pieces, which is I think  
8 the intention, we will kind of review it in pieces so  
9 that, one, we can get feedback that they're headed in  
10 the right direction in a given area. But also so that  
11 we can keep the process moving.

12 There's going to be a lot of data needed,  
13 and some of this will be actually showing what data  
14 needs to be taken, needs to be obtained so that they  
15 can --

16 DR. BONACA: Excuse me. RIA stands for  
17 what, rod ejection accident?

18 MS. CHATTERTON: Yes.

19 DR. BONACA: Okay.

20 CHAIRMAN POWERS: Reactivity insertion.

21 MS. CHATTERTON: Right. I'm sorry.

22 DR. BONACA: It's more general.

23 MS. CHATTERTON: Well, I'm sorry. Were  
24 there anymore questions on this one?

25 (No response.)

1 MS. CHATTERTON: The next thing I wanted  
2 to get into a little bit was lead test assemblies just  
3 because, as I said, that's been an area that we really  
4 want some emphasis on, and we've stated all along that  
5 we think they should be prototypical, up to the  
6 proposed burnup with reasonable power histories that  
7 are similar to what's being used.

8 In the past we'd always said we wanted  
9 them in very nonlimiting locations, and it was very  
10 common to burn a lead test assembly to 50 or 60  
11 gigawatt days, but to do it in six, seven cycles. And  
12 then when you put the fuel in and burned it in three  
13 cycles, you may not get the same -- exactly the same  
14 results.

15 So that's why there's going to be a  
16 real -- we're really emphasizing lead test assemblies,  
17 and we also know the type of cladding makes a  
18 difference, the flow conditions, the water chemistry.  
19 Lead test assemblies need to be characterized, of  
20 course, before irradiation.

21 And they will need pool site, and or hot  
22 cell exams after. Hot cells exams are probably going  
23 to be relatively infrequent, but there will be some  
24 need. Certainly there'll be -- full site will be  
25 needed certainly after each cycle, final burnup on

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1 assemblies that are designated as lead test assemblies  
2 before they start irradiation.

3           However, there may be assemblies that  
4 become LTAs after they've had some burnup on them.  
5 And so sort of to address that, to encourage lead test  
6 assemblies, to encourage the gathering of data, we've  
7 taken on a program to try to look for lead test  
8 assembly guidelines, something that we haven't had in  
9 the past.

10           Sometimes we had a submittal that we  
11 reviewed and approved. Many times there was not an  
12 actual regulation or any restriction. So under the  
13 50.59, under the test parts they were able to do lead  
14 test assemblies. It leads to a lot of things that we  
15 hope by coming out with some guidelines we can  
16 improve.

17           The purpose, basically, to get a  
18 consistent approach, to get consistent database, to  
19 obtain data. There's a real benefit to the industry,  
20 too, in that they will know what we expect and know  
21 that if they follow these guidelines, that it's  
22 certainty.

23           I'll give you the outline topics and  
24 things in another slide, but that's basically it.  
25 We've made some progress on this. We met with WOG in

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1 May of 2000. They put a lot of work into it, got the  
2 whole industry together. They submitted a topical  
3 report, which we looked at, and then we met with them  
4 in December.

5 We gave them our comments on that document  
6 in January, and we expect to hear from them again  
7 soon. Some of the things that are covered and need to  
8 be covered is a definition. Exactly what are we  
9 talking about as a lead test assemblies? What are the  
10 conditions?

11 Characterization, the type of  
12 characterization of the rods, full site, hot cell;  
13 when are hot cell exams needed?

14 Characterization will have to address both  
15 pre-characterization and after final burnup.

16 The guidelines will address the number of  
17 LTAs that can be in any one core. Also the location.  
18 That's what I mean by placement. Location in the  
19 core, what restrictions we think are necessary.

20 Safety requirements. The biggest thing  
21 here is in almost all cases the LTAs are designed such  
22 that they meet all the fuel design limits that the  
23 current core is meeting.

24 However, they meet them using a code  
25 that's been verified to 62. If we're now talking

1 about burnups that are going higher, we're taking a  
2 step in saying that the code is valid beyond.

3 On the other hand, if you don't get the  
4 data you can't validate the code. So this isn't an  
5 area that we're working on, how to write that up, how  
6 to address it such that it's covered conservatively.

7 Part of the way that it's covered, of  
8 course, is the few number of pins that would be LTAs  
9 and given the whole number in the core.

10 DR. UHRIG: There's been reports the last  
11 three or four years of a control rod binding and  
12 sticking, and the general, as I recall, the exposure  
13 was about 43-44,000 megawatt days per ton, in the  
14 vicinity of the assembly.

15 MS. CHATTERTON: A little higher.

16 DR. UHRIG: Little higher. What's going  
17 to happen when you get the higher limits here? Are  
18 there going to be more problems of that sort, or is  
19 this something that has been addressed?

20 MS. CHATTERTON: That is one thing that  
21 will have to be addressed, and you're right. I didn't  
22 have it on the slide. But all the current type  
23 problems that we've seen, like the incomplete control  
24 rod insertion, some of these crud and oxidation type  
25 problems, all of those things are going to have to be

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1 addressed in a program to go to higher burnup,  
2 absolutely.

3 CHAIRMAN POWERS: When you think about it,  
4 people can go up to the 60 gigawatt days per ton.  
5 Now, somebody comes along and says, "Gee, I want to go  
6 to 70." That's what, 16 percent extrapolation?

7 It doesn't sound an outrageous  
8 extrapolation to me. Do we have evidence that we  
9 would expect changes in physics of the kind we saw  
10 between going from 30 to 60 when we go from 60 to 75?

11 MS. CHATTERTON: Do we have hard evidence?  
12 I don't think we have evidence.

13 CHAIRMAN POWERS: I mean, there are fuel  
14 rods around that have gone up to 75.

15 MS. CHATTERTON: That's right. There are  
16 fuel rods that have gone around. I know of some in  
17 Europe that have gone as high as 100.

18 CHAIRMAN POWERS: That's right.

19 MS. CHATTERTON: Do we have evidence?

20 No, we really don't. But I think this is  
21 a point that the -- we said there was an extrapolation  
22 at one point in the past, and then there were some  
23 things that happened that maybe weren't thought were  
24 going to happen. And that it's time to stop and  
25 really examine all the criteria before we move or

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1 leave forward.

2 CHAIRMAN POWERS: I guess what I'm asking  
3 is -- I don't know whether I'm asking -- we're closing  
4 the barn door or we're making up for the sins of the  
5 past on the backs of the people that are guiltless.  
6 And we're talking about relatively small changes here  
7 and asking for a heroic amount of work it looks to me.

8 And I'm wondering is there really merit in  
9 that? I mean if we sort out the issues in 60 and say,  
10 "Okay, everything's fine here," and that, quite  
11 frankly, looks the direction it's going with these  
12 superior clads. You know, things look like they're  
13 moving along fine. Do we really want to create an  
14 enormous burden?

15 I mean, clearly moving the lead test  
16 assemblies out of the benign locations and into more  
17 prototypical location, that's something that's been  
18 needed for a long time. But after you go much beyond  
19 that, do we really learn risk significant information  
20 from LTAs?

21 MS. CHATTERTON: I think we gain a good  
22 deal of information. I think we also gain some  
23 confidence in reproducibility and uncertainty on --  
24 you know, how uncertain are the measurements to take  
25 when you take them only once? You asked the question

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1 and --

2 CHAIRMAN POWERS: Oh, yeah. Ralph's good  
3 at that. He knows how to do that.

4 MS. CHATTERTON: And that is -- to me that  
5 is one of the things. This is an opportunity that you  
6 have to do that. That's not a difficult one. You  
7 can't do that on the accidents that Ralph is talking  
8 about. I mean, my goodness, the cost of the tests,  
9 you couldn't possibly do that.

10 But on this, these are some areas that you  
11 can.

12 CHAIRMAN POWERS: There are those of us to  
13 take the vote that say you absolutely must do that,  
14 especially because of the test are so expensive.

15 MS. CHATTERTON: Well, I don't look at it  
16 as -- it sounds like a lot but let me -- maybe I  
17 didn't characterize some of it exactly correctly.

18 I think there's a lot of areas that the  
19 industry is going to be able to right off very  
20 quickly.

21 CHAIRMAN POWERS: Okay.

22 MS. CHATTERTON: With the state -- going  
23 to higher burnup makes no difference and here's why.

24 We look at this, too, as this will help  
25 not only us, but the industry have a really good

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1 documentation of what is important and, you know, how  
2 things change.

3 I expect there'll be an awful lot of  
4 things that are written off very quickly, and they do,  
5 too. They're working on the major ones.

6 DR. CRONENBERG: Then maybe it's not so  
7 small. It's longer burnup, higher burnup, longer fuel  
8 duty times, 20 percent power increases. I thought you  
9 were asking a rhetorical question.

10 CHAIRMAN POWERS: No, I don't think I was.  
11 I agree with you. Some of the things -- it's more  
12 than just an increment in burnup because we're doing  
13 an increment in --

14 DR. CRONENBERG: I mean, Commonwealth  
15 Edison has come in on the docket with a 17 percent  
16 power increase, one step.

17 CHAIRMAN POWERS: There's a lot more going  
18 on here, none of which is really designed to coddle  
19 the fuel at all. It's going to put this fuel under  
20 some pretty heavy stress.

21 But the question then comes back to is it  
22 a risk significant issue that we're getting into.  
23 They can have all the operational difficulties that  
24 they want to volunteer for, and that's their business.  
25 Is it -- what we're asking about are -- our concern is

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1 more with the risk issues.

2 And, you know, I think we have to be  
3 careful not to close the barn door and put the burden  
4 on -- that's all I'm concerned about.

5 MR. CARUSO: I'd just like to make the  
6 observation -- this is Ralph Caruso from Reactor  
7 Systems Branch.

8 Dr. Powers, you had asked if there was a  
9 regulatory requirement for us to gather this sort of  
10 operational data, and I would make the observation  
11 that we are less interested from a regulatory point of  
12 view in this operational data than in the knowledge  
13 point of view.

14 One of the reasons why we're encouraging  
15 people to do lead test assemblies is to share the data  
16 with us. In the past they've been reluctant to do  
17 that, but what we're trying to do is we're trying to  
18 make the process easier for them so that they can do  
19 more testing which we believe benefits them.

20 And by trying to make the process easier  
21 and being a bit less threatening from a regulatory  
22 perspective, we hope that they'll share the  
23 information with us. We'll understand what they're  
24 doing, and we will therefor feel more confident that  
25 they know what they're doing.

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1                   So there's quite a bit of working together  
2                   on this, and we're not necessarily going to change any  
3                   regulations. We're just trying to understand what's  
4                   going on. I don't know if that helps any.

5                   CHAIRMAN POWERS: Sure.

6                   DR. KRESS: I see two places where  
7                   operational testing could shed light on or that has  
8                   risk significance. One of them is on the rod  
9                   insertion issue.

10                  And the other one is that it's true that  
11                  the iodine spike is due to failed pins, which are few  
12                  and far between in a core, but that may be where  
13                  that's -- may be where that spike comes from. I would  
14                  perceive that if higher burnup increases the failure  
15                  rate of those pins, it would increase the iodine  
16                  spike, and you might be able to see that during the  
17                  operational -- that's where it comes from anyway --  
18                  during your operational observations.

19                  CHAIRMAN POWERS: You've got to convince  
20                  me that an iodine spike is risk significant.

21                  DR. KRESS: Yeah, it falls more in the  
22                  category of design basis accidents.

23                  CHAIRMAN POWERS: Design basis accidents.  
24                  I mean I think there are risk -- there are interesting  
25                  risk significant features here in the high burnup

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1 fuels. I'm not sure that LPAs get to them.

2 DR. KRESS: Yeah, that's -- I think that  
3 was your point.

4 CHAIRMAN POWERS: Yeah.

5 MS. CHATTERTON: The LTAs do provide you  
6 with the rods you need for something like Ralph's  
7 program and for really --

8 CHAIRMAN POWERS: Now, there's where you  
9 get it. Now, Ralph's program's got to be extended to  
10 75 gigawatt days per ton; right, Ralph?

11 MS. CHATTERTON: Actually, we've said the  
12 industry has to then pick up the tab beyond 62.  
13 That's as far as the agency program. We said we do  
14 confirmatory work to 62 and then beyond that --

15 CHAIRMAN POWERS: I know what you said.  
16 Now, we just won't hold you to it. We'll let you  
17 backtrack on that one.

18 (Laughter.)

19 MS. CHATTERTON: The last point on my lead  
20 test assembly guidelines thing is we don't have  
21 reporting in there. Basically, hopefully it would be  
22 a template. It would be very easy to fill out, but it  
23 would give us -- it would provide the data. Then we  
24 would be able to know exactly what's happening with  
25 LTAs.

1 CHAIRMAN POWERS: Nothing that you are  
2 legally bound to is easy to fill out.

3 MS. CHATTERTON: I just finished my taxes.

4 CHAIRMAN POWERS: That's right.

5 DR. CRONENBERG: You know somebody was --  
6 that wasn't very expensive, that annual -- that kind  
7 of pool site inspections, and I think it was a good  
8 thing, and we don't do it any more.

9 CHAIRMAN POWERS: Yeah, I mean there's not  
10 question it's a good thing, but the idea that a  
11 licensee is going to have an easy report to fill out,  
12 I mean, it just doesn't exist. There is no report  
13 that the licensee prepares that's easy to do, because  
14 they are --

15 MS. CHATTERTON: Easier?

16 CHAIRMAN POWERS: Easier is possible.

17 MS. CHATTERTON: Okay, easier.

18 DR. UHRIG: What happens to the lead test  
19 assemblies? Do they remain with the utilities?

20 MS. CHATTERTON: Yes.

21 DR. UHRIG: And are they usually sent for  
22 examination in detail or is this just sort of a --  
23 what kind of data comes out of them?

24 MS. CHATTERTON: At the end, we would  
25 expect an all lead test assemblies to do pool site

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1 exams, and that --

2 DR. UHRIG: Okay.

3 MS. CHATTERTON: -- that would consist of  
4 oxidation measurements, probably growth  
5 measurements --

6 DR. UHRIG: Growth rate, yeah.

7 MS. CHATTERTON: -- growth rate, visuals,  
8 get an awful lot from visuals. And then depending on  
9 if anything was found, it would determine what  
10 further --

11 DR. UHRIG: They don't do a destructive  
12 examination though. Metallurgy --

13 MS. CHATTERTON: No. If something really  
14 is shown, then we would think that a hot cell exam --

15 DR. UHRIG: Would be in order.

16 MS. CHATTERTON: A constructive hot cell  
17 exam would be in order.

18 CHAIRMAN POWERS: How many cells in the  
19 country are available to do full length rods?

20 DR. UHRIG: One. Two.

21 CHAIRMAN POWERS: Two.

22 MS. CHATTERTON: Yeah. A number of hot  
23 cell exams, few and far between.

24 So moving on, why do we really want a lot  
25 of that?

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1 Well, part of it is because of some of  
2 these recent fuel issues. Oxidation higher than  
3 predicted. We have several cases where, as I said,  
4 the LTAs behave beautifully. If the fuel is burned as  
5 the LTAs were, it behaves beautifully. But if it's  
6 burned at a higher rate, at faster duty, they've  
7 gotten very different results.

8 I think you're all probably aware of axial  
9 offset anomalies that still tend to be -- that's a  
10 problem that's still not completely understood.

11 DR. UHRIG: Isn't that pretty much boron  
12 chemistry?

13 MS. CHATTERTON: It's a chemistry issue,  
14 but it's also a fuel duty issue. And it's a very  
15 strange --

16 DR. UHRIG: Well, it does depress the flux  
17 in the area and reduces the load on the fuel.

18 MS. CHATTERTON: That's correct.

19 DR. UHRIG: But it would force it to be  
20 somewhere else for the same power level.

21 MS. CHATTERTON: Yes, it forces -- it's  
22 usually the precipitate at the top of the fuel forcing  
23 the power to the bottom. You end up with a shutdown  
24 margin problem. Had one utility that had to operate  
25 at 70 percent power for four of five months as a

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1 result of that, and it's continued through other  
2 cycles.

3 Several other utilities have seen it, not  
4 anywhere near to that extent.

5 DR. UHRIG: This is --

6 CHAIRMAN POWERS: Do we understand -- I  
7 mean this is an inverse chemistry thing. Inverse  
8 solubility issue, and you don't ordinarily think of  
9 that arising with boron. Do we understand why boron  
10 suddenly has an inverse -- boron becomes less soluble  
11 at high temperatures.

12 MS. CHATTERTON: Actually, it's a sub-  
13 cooled boiling. Basically, what you've done is you've  
14 precipitated some crud onto the control rods, you're  
15 in a region of sub-cooled boiling, and in the process  
16 of sub-cool boiling with the boron, you precipitate  
17 boron into that crud.

18 CHAIRMAN POWERS: And that's the step I  
19 don't follow.

20 MS. CHATTERTON: You don't follow.

21 CHAIRMAN POWERS: Why does the boron  
22 suddenly say, "I want to precipitate"?

23 MS. CHATTERTON: Well, with the sub-cooled  
24 boiling you've got a mechanism there to -- you want to  
25 give me a little --

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1 MR. NISSLEY: I'm not an expert on this  
2 but some of the theories are that when you have crud  
3 and corrosion in the presence of sub-cooled boiling,  
4 that the boiling mechanism is coming off as pure steam  
5 and leaving the boron behind.

6 CHAIRMAN POWERS: And then it gets flooded  
7 right back up with water and --

8 MR. NISSLEY: It's thought to -- it's  
9 sometime referred to as boron hide-out where it's not  
10 on the -- completely on the outer surface. It's  
11 somewhat within the structure of the crud and the  
12 corrosion.

13 MS. CHATTERTON: You get kind of like  
14 little chimneys in within the --

15 CHAIRMAN POWERS: This sounds like on of  
16 the things that if you tried to do it, it would be  
17 impossible.

18 MS. CHATTERTON: Probably so. But it's  
19 certainly been a problem that --

20 DR. CRONENBERG: But it's real. I mean,  
21 they've measured crud with a high boron content.

22 MS. CHATTERTON: Yes.

23 CHAIRMAN POWERS: Well, I'm still asking  
24 why.

25 DR. CRONENBERG: Yeah, I don't know, but

1 it's there.

2 MS. CHATTERTON: Everyone has spent a lot  
3 of time on this issue, and it's still around. We've  
4 had some fuel failures in a couple different plants  
5 due to high fuel duty. Again, a combination of crud  
6 and high fuel duty.

7 In all these cases, we've seen the effects  
8 of water chemistry, high crud build-up, and we've seen  
9 some accelerated growth of rods in assemblies.

10 That's much more the IRI issue that is  
11 pretty much under control. I think I could say that  
12 very easily in all plants, or at least appears to be  
13 up until very recently.

14 The last thing I wanted to talk a little  
15 bit about is some of the current fuel reviews that  
16 we're doing. We have two reviews on cladding types  
17 that are in house now. One is the duplex cladding  
18 developed by Siemens, used extensively in Europe.  
19 That's the one that has rods up to 100 in the Goesgen  
20 plant in Switzerland.

21 CHAIRMAN POWERS: Wow.

22 MS. CHATTERTON: The review on that  
23 cladding will be to 62. And we're just beginning that  
24 review right now.

25 We're also reviewing the use of ZIRLO for

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1 CE plants. We have some CE plants that have fairly  
2 high duty that have been using a low tin Zircaloy  
3 that's not been standing up to quite what they would  
4 like.

5 And so the use of ZIRLO in those plants  
6 would be extremely advantageous. And that's the  
7 reason the timetable is they really want this by the  
8 end of the summer. So we've got a large review.

9 The issue is basically a lot of it will be  
10 making sure that the interfaces are done correctly on  
11 the computer codes, that you get the right properties  
12 in, and it's handled in each way. So there's a lot in  
13 there.

14 And basically that's what I had as far as  
15 issues.

16 DR. UHRIG: What do you mean by duplex  
17 cladding?

18 MS. CHATTERTON: Duplex is -- it's a  
19 double type of cladding. It's almost the reverse of  
20 the BWR liner cladding.

21 DR. APOSTOLAKIS: The barrier cladding.

22 MS. CHATTERTON: It's got its corrosion  
23 barrier on the outside, and it's a Zircaloy on the  
24 inside.

25 DR. APOSTOLAKIS: Okay. It's a double

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1 cladding.

2 MS. CHATTERTON: It's essentially a double  
3 cladding. Very, very --

4 DR. UHRIG: They're both Zirc?

5 MS. CHATTERTON: Pardon?

6 DR. UHRIG: Both are Zirc?

7 MS. CHATTERTON: No.

8 DR. APOSTOLAKIS: Different material.

9 MS. CHATTERTON: The outside is a -- I  
10 have to think.

11 PARTICIPANT: It's a proprietary material  
12 to Siemens.

13 MS. CHATTERTON: Yeah.

14 DR. UHRIG: Oh, okay.

15 MS. CHATTERTON: The strength part, inner  
16 part is Zirc-4. And as I said, they've had -- they've  
17 used that extensively in Europe. The data from it as  
18 far as corrosion and performance is absolutely  
19 excellent.

20 CHAIRMAN POWERS: I hope that once you get  
21 through your duplex cladding review, you come down and  
22 talk to us a little bit about that.

23 MS. CHATTERTON: Sure.

24 CHAIRMAN POWERS: Because I think that  
25 would be interesting for us to see.

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1 MS. CHATTERTON: Good. Thank you very  
2 much.

3 CHAIRMAN POWERS: Thank you, and good luck  
4 in Boston.

5 MS. CHATTERTON: Oh, thank you. It'll be  
6 a slow run. It will be fun, but it won't be a fast  
7 run.

8 CHAIRMAN POWERS: Don't care how slow it  
9 is. the fact that you're there is just amazing.

10 DR. KRESS: We're going to look for you on  
11 TV.

12 CHAIRMAN POWERS: We'll watch for you on  
13 TV.

14 MS. CHATTERTON: I'll be the last one.

15 CHAIRMAN POWERS: We're going to switch  
16 gears and Dr. Lee fresh from a vacation of over --  
17 almost a week duration in Italy is obviously going to  
18 be in fantastic spirits to talk to us about the MOX  
19 research program.

20 Yes, you're in a good mood when you come  
21 back.

22 DR. LEE: I'd like to briefly tell you  
23 something about the MOX research that Office of  
24 Research undertook, just started last November.

25 How about now? Thank you.

1 And you know why our interest in mixed  
2 oxide fuel. In February 2nd, the NMSS team came  
3 before the full Committee and briefed you on the  
4 certification plan and what is the activity related to  
5 your mixed oxide fuel, MOX fuel use in U.S. That is  
6 basically the disposal of up to 33 metric tons of MOX  
7 fuel in using it in our commercial reactors, and the  
8 two plan, four units targeted is McGuire and Catawba.

9 CHAIRMAN POWERS: What I have never  
10 understood is why ice condenser plants are  
11 particularly suited for using MOX fuel.

12 DR. LEE: And you were told that they  
13 really did not target ice condenser, remember?

14 (Laughter.)

15 DR. LEE: There was two Virginia power  
16 plants that was involved with it, but they drop out of  
17 it, but it happened in the two plants that's left.  
18 There are four units left now, ice condenser plant,  
19 under Duke Power, and I'm sure your concern has to do  
20 with the severe accident issues about fuel dispersal.

21 CHAIRMAN POWERS: Yep. Comes to mind.

22 DR. LEE: I think, one, we did the DCH for  
23 ice condenser plan. We found the McGuire plant is  
24 slight a bit higher than the cutoff point that we use,  
25 like .1 conditional failure probability. It came up

1 to .14, but the Duke Power took issue with us that if  
2 you take into the real design of the plants, that  
3 number will come down.

4 So when the whole overall evaluation for  
5 MOX use in the McGuire come in, those numbers will  
6 be --

7 CHAIRMAN POWERS: Yeah, I think I would  
8 have responded by saying, "Yeah, and when I take the  
9 degradation of the containment into account, the  
10 number goes up again."

11 DR. LEE: Well, the research activities  
12 really focus on supporting a user request that came in  
13 back in late '99, and at that time we didn't have any  
14 budget to address it, but we just had budget this year  
15 to address the technical assistance requested by the  
16 regulations, nuclear reactor regulations.

17 They are interiors (phonetic) neutronics,  
18 fuel and source terms. The neutronics, they want to  
19 modify the codes that were used for MOX and also, of  
20 course, goes with it all the fuel behavior, monitoring  
21 assessment for the fuel behavior for design based  
22 accidents and under normal as well as design based  
23 accidents need to be corrected before we can use it.

24 And then in the source term area that we  
25 also need to validate that the source term that we

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1 used for UO-2 fuel (phonetic) is approximate for MOX.

2 DR. KRESS: How much, what percentage of  
3 the fuel would be MOX in these?

4 DR. LEE: It's normally one third of the  
5 core would be MOX.

6 DR. KRESS: Oh, as much as one third?

7 DR. LEE: Yeah.

8 DR. KRESS: Okay. I --

9 PARTICIPANT: Forty percent.

10 DR. KRESS: Okay.

11 DR. LEE: Or even more than that. Thirty  
12 to 40 percent.

13 Now, as I mentioned to you, we started  
14 this activity not too long ago, but at that same time  
15 before that, Ralph Meyer was doing a PIRT on the high  
16 burnup fuel. So since we know the MOX is going to be  
17 coming into play, we attached to ask our experts to  
18 tell us something about what do we have to do for the  
19 LOCA and reactivity accident, and that PIRT has been  
20 completed.

21 And on that Web site you will see the  
22 reports related to LOCA as well as the RIA accident.

23 The source term PIRT now is going to be  
24 starting very soon. It's not just for MOX. It's also  
25 for high burnup fuel as well, and we expect to finish

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1 by this year.

2 The composition for the experts have not  
3 been -- selection not been completed because we're  
4 waiting for a response from French and from Japan and  
5 also from the industry selecting experts to  
6 participate in this panel.

7 The NRC internal one has suggested some  
8 members, and we're working on that.

9 Now, in the neutronics area, there are  
10 three areas that we have initiated. The first one is  
11 the PARCS code that we have at Purdue University.  
12 That has been used for many years. This PARCS code is  
13 a neutronics code being interfaced with our thermal  
14 hydraulics codes like TRACK M or RELAP, and we have  
15 used it, and we have used it very successfully for RIA  
16 type analysis.

17 And we initiated the modification for this  
18 to make it more usable for MOX. That is started in  
19 November, when we initiated these activities.

20 At this time, we extended the number of  
21 group of energy that can be handled by the code from  
22 two groups to n group because it's very easy to make  
23 it general. One time we can use seven groups, four  
24 groups, two groups, because if the industry comes in  
25 with analysis with two groups, we have to be able to

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1 collapse it to two groups so we can analyze it on the  
2 same base.

3 The cross-section because of the isotropic  
4 between the UO-2 bundles and the MOX bundles, there  
5 will be very sharp gradients of neutron flux. So we  
6 have to handle the scattering correctly. So we have  
7 expanded the cross-section angle of dependency with P3  
8 approximation. That has been completed as well at  
9 this time.

10 DR. APOSTOLAKIS: Is there a report where  
11 I could go and find more about these observations  
12 like, you know, why you need to go to the P3  
13 approximation, and so on? The motivation for the  
14 research, in other words.

15 DR. LEE: I think the motivation if you  
16 look at the Europeans, the way they analyzed the MOX  
17 code, they usually use a high order scattering to do  
18 the approximation.

19 DR. APOSTOLAKIS: So I should look to  
20 Italy as well to find that?

21 CHAIRMAN POWERS: Well, I think there's --

22 DR. APOSTOLAKIS: There must be a report  
23 somewhere.

24 CHAIRMAN POWERS: Yeah, one of the authors  
25 of PARCS put out a document that went through all of

1 these things, and it was given to this subcommittee a  
2 couple of years ago, I guess. I can't remember the  
3 exact title, and I mean, I am sure we could find that  
4 for you.

5 DR. APOSTOLAKIS: Okay.

6 DR. KRESS: It was a pretty good document  
7 as best I remember.

8 CHAIRMAN POWERS: It was a pretty good  
9 document. I mean, it raised the scattering and the  
10 group issue. It also raised the delayed neutron  
11 fraction issue.

12 DR. LEE: Yeah, the delayed neutron  
13 fraction.

14 DR. MEYER: Was this a Commission paper  
15 that you're referring to?

16 CHAIRMAN POWERS: No, no. Actually it was  
17 a Purdue report.

18 PARTICIPANT: It was critical, a bit  
19 critical, right?

20 CHAIRMAN POWERS: Well, I wouldn't say it  
21 was critical. I would say that he came back and said,  
22 "Look. My PARCS code right now can't do the MOX fuel  
23 because of these things," and he listed down what he  
24 had problems with using PARCS for that.

25 So I mean if it was critical, it was

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1 critical of his own codes.

2 MS. SHOOP: This is Undine Shoop with  
3 Reactor Systems.

4 You can find more detail in the Commission  
5 paper that we wrote. We've authored two of them at  
6 this point. One would be from '99, and one would be  
7 early 2000, and I'm sure we can get copies of them for  
8 you.

9 DR. APOSTOLAKIS: Good.

10 DR. LEE: In addition, at this time there  
11 was a researcher from Saclay, is stationed at Purdue  
12 University assay change for about a year, and you can  
13 reduce in the French code CRONOS, and this code has  
14 been benchmarked against many of othe plant data in  
15 France that use MOX code. So we like to compare that  
16 with the developed PARCS that we're going to be using  
17 for MOX code analysis as well.

18 Tom Downer from Purdue University is the  
19 one who is the PI for this, principal investigator for  
20 this work. He's also working with the OECD and NEA to  
21 develop a theoretical benchmark for reactivity  
22 transient. This is quite a lot of work to do because  
23 now you need to develop an exercise that go from  
24 steady state and looking at some transient, how would  
25 the parts compare with other codes?

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1           Of course, you would be using a Monte  
2 Carlo code calculation, and so forth, but this is a  
3 code-to-code comparison.

4           We also initiated a very small activities  
5 at Brookhaven under Dave Diamond. He has been helping  
6 us for many years, helping us to do independent  
7 assessment of PARCS, and we intend to use him to  
8 continue this activity.

9           It provides feedback to code developers,  
10 and we try to make it also more user friendly, too,  
11 because a couple in between the milars (phonetic)  
12 continue to be a problems in setting up the problem,  
13 but we are making it better now.

14           And then also, in terms of if there is any  
15 technical issues that we require his assistance to  
16 review, the licensee will submit to us and we will ask  
17 them to do so.

18           At the same time we also initiate a  
19 lattice physics code develop at Oak Ridge. It's a  
20 routine called NEWT, and this is part of the scale  
21 code, the whole suite of codes that Oak Ridge use for  
22 shielding, heating, decay heat, and also analysis.

23           And this will enable us to generate the  
24 cross-section, assembly-wise cross-section that we can  
25 feed into PARCS and that PARCS can use for steady

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1 state, and as well sa depletion, as well as transient  
2 analysis, especially IA type.

3 In the fuel area, of course, we started to  
4 update the material properties for the FRAPTRON and  
5 the FRAPTRON codes to be used for the MOX analysis.  
6 Then, of course, we have to assess the experiment  
7 against data.

8 There is a Halden exercise, blind test,  
9 the CS&I test completed, and at that -- actually the  
10 rig is still inside the reactor. They continue to  
11 monitor, measure the build-up of the fission gas, and  
12 the temperature. So you can get those probably as a  
13 function of burnup.

14 The exercise they did was allow 14  
15 gigawatt days per ton. At that point they asked all  
16 the participants to do the calculations. That was  
17 back two years ago, and they just finished that.

18 So those are the information that we would  
19 like to revisit, and of course, in this area, I didn't  
20 mention, of course, the Cabri test for the IRA. We  
21 would like to look at the gigawatt behavior, as well.

22 In the source term area, we are  
23 negotiating with the France to get VERCORS  
24 experiments. The VEGA from Japan, they will not be  
25 doing any MOX experiment until like 2003.

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