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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS)

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SUBCOMMITTEE ON REACTOR FUELS

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WEDNESDAY,

JULY 27, 2005

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ROCKVILLE, MARYLAND

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The subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T-2B3, 11545 Rockville Pike, at 8:30 a.m., Dana A. Powers, Chairman, presiding.

COMMITTEE MEMBERS:

DANA A. POWERS, Chairman

RICHARD S. DENNING, Member

THOMAS S. KRESS, Member

WILLIAM J. SHACK, Member

ACRS/ACNW STAFF:

RALPH CARUSO, Designated Federal Official

PANELISTS:

MIKE BILLONE, Argonne National Laboratory

BERT M. DUNN, Advisory Engineer, Framatome ANP

JEAN-PAUL MARDON, EPRI

ROBERT MONTGOMERY, EPRI

ODELLI OZER, Fuel Reliability Program, EPRI

NRC STAFF:

FAROUK ELTAWILA, RES/DSARE

RALPH MEYER, RES

HAROLD SCOTT

A-G-E-N-D-A

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

8:30 a.m.

CHAIRMAN POWERS: The meeting will come to order. This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee on Reactor Fuels. I'm Dana Powers, chairman of the subcommittee. Subcommittee members in attendance are Dr. Tom Kress, who I'll note is the member with the longest tenure on the ACRS, Rich Denning, who I'll note is the member with the shortest tenure on the ACRS, and Bill Shack, who is the vice chairman of the ACRS.

MEMBER DENNING: Did you say he's the --

CHAIRMAN POWERS: No, he's on the left wing, as you well know. The purpose of the meeting today is to discuss the results of activities in the Office of Research related to the development of revised LOCA criteria for reactor fuel. Tomorrow we will hear about the behavior of reactor fuel during reactivity-initiated accidents. The subcommittee will hold discussions with representatives of the NRC staff and with the industry regarding these matters. The subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberation by the full committee. Ralph Caruso is the designated federal

official for this meeting.

The rules for participating in today's meeting have been announced as part of the notice of the meeting previously published in the Federal Register on June 29, 2005. For those of you that regularly review the Federal Register, you will know that a transcript of the meeting is being kept and will be made available as stated in the Federal Register notice. It is requested that speakers first identify themselves and speak with sufficient clarity and volume so they can be readily heard. We have received requests from one member of the public, Mr. Shadis of the New England Coalition for time to make oral statements. And Mr. Shadis, if you will be kind enough to look at the agenda and let me know when it would be appropriate for you to speak we will make the arrangements to give you time then.

Do members of the subcommittee have any opening comments they would like to make?

MEMBER SHACK: Yes, Mr. Chairman. I'd like to note that I have a conflict of interest today since some of the work that's being discussed is being performed at Argonne National Laboratory.

CHAIRMAN POWERS: And the subcommittee will then be utilizing you as a resource for

clarification, but not for generation of opinions and tentative actions. Right? Yes. Feel free to join in for the technical discussions. Historically meetings of the Reactor Fuel Subcommittee have been highly technical in nature.

With no other comments to be made, I'll turn to our first speaker who is Mr. Farouk Eltawila of the Office of Nuclear Regulatory Research.

MR. ELTAWILA: Good morning and thank you Mr. Chairman. As you know, for several years we have been investigating the effect of burnup on fuel behavior during pulse related directivity and shaft accident, and loss of coolant accident. This has been done according to a program plan that has been endorsed by NRR, and was most recently updated in August 2003.

In September 2003, we met with this subcommittee, a new member here, Dr. Denning, and updated the provided -- described the detail, how we are going to address the regulatory criteria for reactivity insertion accident and loss of coolant accidents. A few months later the ACRS in its report, NUREG-0365 I believe, wrote a very favorable comment about the research, and we appreciate the feedback that we got from you on that.

In the staff requirement memorandum, and that's the mechanism that the Commission interact with the staff, dated March 31, 2003, the Commission approved the staff recommendation to proceed with modification to 10 C.F.R. 50.46 to provide for more performance-based approach to meeting ECCS acceptance criteria. Our research is complete enough now to initiate this modification, so we asked for this meeting today to get early ACRS input in this progress. This modification is intended to accommodate the effect of high-burnup on fuel cladding behavior, and to generate a rule to apply to all zirconium alloy cladding. As you know, the rule right now is written for zirconium, and ZIRLO, and M5 is not specified in the rule, and we have to apply every time for a licensing amendment or exemption.

At the present time exemption from the regulatory is needed for the M5. We are working on a tight schedule now to try to rectify this situation. And the current schedule called for publication of an advance notice of proposed rulemaking in early January of next year. Therefore we would like to get a letter from the ACRS after the full committee meeting in September so those comments can be factored into the internal discussion before we issue the rulemaking.

CHAIRMAN POWERS: Dr. Eltawila, is the schedule such that you need the letter in September? October would not do?

MR. ELTAWILA: The technical basis on the operating plants is supposed to be provided to NRR at the end of September. But, you know, getting a letter in September or October will not cause any problem because we are planning to, you know, just preparing for the rulemaking and things like that will take all the way up to January or something.

CHAIRMAN POWERS: I mean, I guess the situation is if we're going to write you a letter that says it looks good, then you can get it in September or October. If we have any substantive comments on it you'd like to get it in September.

MR. ELTAWILA: Definitely September. You are going to hear a different story from the industry, I can assure you that. So I'm giving you a heads-up, you know, because we received a letter from Westinghouse indicating that they would like to see a delay in the rulemaking, and we are planning to respond to that letter. Not Westinghouse, the chairman of the program just happens to be from Westinghouse.

Also last year we prepared a research

information letter, and we forward to NRR to provide the technical basis for reactivity insertion accident. NRR used that, and based on that evaluated an EPRI supplemental on proposed criteria for the reactivity insertion accident. And EPRI is planning to make a presentation on that issue today.

CHAIRMAN POWERS: On the general thrust, I mean, the general strategy without speaking to the specifics of the letter, this evolution of a fairly detailed scientific research into a research information letter that then goes into the regulatory process strikes me as an area that RES should be proud of, and that the Commission should be aware of that progression. Do you share that view? We might want to think about commenting on that history in some report to the Commission, just so they're aware how this program has progressed.

MR. ELTAWILA: I think we'd appreciate that, definitely.

CHAIRMAN POWERS: We might.

MR. ELTAWILA: It would be very good for the Commission --

CHAIRMAN POWERS: I'd work with you a little bit to try to put that together as distinct from the September issue, just so we understand how

these things go. Because as you are aware, in a couple of reports that the ACRS has prepared on research, one of which was written just when you started your RIA work, and one about three-fourths of the way through. They were fairly complementary on the planning and whatnot, and it would be nice to round that out, just to say, well, and here's what the product was that came out.

MR. ELTAWILA: In fact, some of the comments that the ACRS committee made on the reactivity insertion accident in the NUREG report helped us as ammunition that we interacted with RSN about the way the test program should be, and we said if you don't really have a test program that challenging the fuel, and things like that, we will not be able to participate. And we used the report as a weapon. So that was very good.

CHAIRMAN POWERS: Well, our intention is not to write things that are weapons, but I think it might be useful for the subcommittee to consider drafting a letter that just ties that up. Because as you recall, I think it was four years ago that we wrote in our research report when this work was just being undertaken. And we subsequently wrote a report that Dr. Eltawila mentioned in which we suggested some

course corrections. And then to say, okay, here's what came out of it would be useful, I think, especially since the Commission now has two members that did not track that history.

MR. ELTAWILA: And actually you have two new Commissioners who will appreciate this kind of mention. Just, that takes me to my final comments. As you know, this research program is a cooperative research program, and we work very closely with EPRI and Westinghouse, Framatome. They have provided us with the fuel, and provide good comments on the research program. We also have international cooperation with RSN as I mentioned. We have cooperation with Russia, and Halden, and JAERI of course in Japan. And the information is flowing freely among this organization, but because, as you are aware, that we have to interpret our results independently from the industry, you will see some difference in interpretation. The way we interpret our results, the same results actually, the same experimental data, is being interpreted differently by NRC versus the industry. And we hope by the end of these couple of days we will try to shed some light about these differences, and if they are a big problem or something that we can deal with. That's all the

opening remarks that I have. I would like to see if you have any questions before I turn it to Dr. Meyer to open the meeting.

CHAIRMAN POWERS: I'm sure we'll have a lot of questions. I'd like to work with your staff in trying to put together a little history on this RIA, especially the ability to do things jointly with the industry and with international cooperation, yet retaining this independence of interpretation. My own feeling of course is independent interpretations, and differences of opinion in interpretations is a healthy sign for the field. I think that that's not a detraction. It may create challenges for our friends in NRR, but it is -- in the scientific basis, this is a healthy thing. And I think it's an area that the Commission needs to know about.

Now we move on to --

MR. ELTAWILA: I agree with you. I believe definitely at the end of the day our rule, our regulation, the result of that research will be much more stronger once we reach the understanding of the other point.

CHAIRMAN POWERS: That's right.

MR. ELTAWILA: So I don't -- I agree with you. We are not thinking about it as a detraction or

anything like that. So, Ralph?

DR. MEYER: Well, this is really a special moment for me because we're bringing to completion two large projects that I've been working on for about 10 years. And of course there will be some follow-on work in each of the areas, but we're now going to try and make use of the major results from these programs. I'm especially proud of the LOCA work that is being done at Argonne National Laboratory. In 1995, while most of the fuel world's attention was still focused on reactivity accidents after the test in France and Japan that we all know about, I initiated the project at Argonne to look at the fuel damage criteria used for LOCA analysis. Others soon realized that possible deficiencies in the LOCA criteria were more important than those in the RIA criteria because of the greater risk significance of a loss of coolant accident.

The industry then joined us in the Argonne program, and within a couple of years, as Farouk will underscore, we were spending big bucks on real experimental research. The laboratory also pitched in with refurbishing of some aging equipment. We soon had a world-class research program that had taken the lead on the safety testing. Spending big bucks in the 21st century is relative, though, and this program is

nothing like the big LOCA research programs that we had in the late '70s and early '80s. We have only got about 10 people, some part-time, working at one laboratory and a 40-year-old hot cell. Nevertheless, under the first-class leadership of Mike Billone we're getting really high quality results, but we're not able to pursue all avenues and answer all questions in this modest program. And we're not going to keep asking for more money to extend the program year after year in pursuit of more answers when we have enough to make good decisions now.

Here's what to expect. You'll find this to be true in today's presentation and in tomorrow's. The number of data points that we have is smaller than you'd like. You can ask some questions that we won't be able to answer. And you'll see us trying to do best estimate work, but sometimes having to make a choice between assumptions, and if one assumption looks conservative and one looks non-conservative we'll choose the conservative one. In the end, our result might be a little conservative, but our conclusions are sound, and I don't think you can do better with the available data.

Now, I'd like to talk about our LOCA work, and after two very brief background slides I'll give

you the bottom line, and then proceed to fill in the blanks. Okay. So, the criteria we're going to talk about are called embrittlement criteria, and they came about in this way. We start with the requirement to maintain coolable geometry, which is embedded in our general design criteria. And then, after a lengthy hearing in the early 1970s the Commission concluded -- I'm going to need a pointer, Theron, sometime -- the Commission concluded that if you step down from the general requirement of maintaining coolability to keeping fuel pellets inside the cladding, and to do that, not letting the cladding fragment, and on down the logic chain you end up with putting limits on oxidation and temperature.

CHAIRMAN POWERS: When I look at this, I mean the coolability seems very plausible, keeping the fuel pellets inside the cladding seems a plausible criterion. Don't let the cladding fragment, break into several pieces seems. This next step, to go from there to retain some ductility in the cladding. It seems plausible. What I wonder is do we have anywhere information that says, gee, when I re-flood hot cores I get fragmentation when I have this much ductility, and don't get fragmentation when I have another amount of ductility. I mean, do we know what re-flooding of

cores do and loss of ductility?

DR. MEYER: Well, yes we do, not from re-flooding whole cores, but from doing tests on individual and multiple fuel rods where we in fact re-flood the test apparatus and produce quenching conditions that are very similar to what we expect during the LOCA. Now, your -- okay, ask it again.

CHAIRMAN POWERS: Where do I go to find these experiments?

DR. MEYER: The experiments?

CHAIRMAN POWERS: Yes.

DR. MEYER: Well, Mike will talk about some of them today, and historically the original experiments that were -- there were some done by not Hobson, but who were some of the? Mike, help me here. The early quench tests. I don't think Hobson did quench tests, did he?

DR. BILLONE: Not of -- I'm sorry, Mike Billone. I was going to address the question a little differently, Dana. There's no complete analysis done of a degraded core that would include every possible load mechanism. There are experiments that were done in the '70s and in the early '80s in which sometimes non-irradiated material was ballooned, and burst, and quenched. And very limited work would have been done

with lightly irradiated rods. So we don't have -- in my opinion, and Ralph can disagree, we don't have a classical stress analysis of a degraded core during quench. So there's a multiple number of possible loads. The idea about cladding retains some ductility historically was because there was some skepticism on the Commission's part that you could quantify what those loads were for a degraded core.

DR. MEYER: There were quench tests prior to the hearings in 1972. And they were discussed. There have been a lot more in the late '70s and early '80s. And we've done some ourselves recently. And this is a major point. And going from this step to this step is one that is of interest to the industry right now. But this is a historic fact, that the Commission debated this, that the parties debated it at the hearing. And the Commission then, based on the findings of the hearing went in this direction. And they expressed their skepticism at being able to analyze the loads and strength responses to the LOCAs because they didn't know what loads would arise. And so they backed down to the position that they said -- where they said just give us some ductility, any ductility, and that will be okay. And in fact, I think it's true in all of the quench tests that are

done, if the material has ductility it survives the quench. In fact, all of the specimens that -- well, maybe that's not true. Maybe you slow-cool some specimens. But we test specimens -- the matter of ductility is post-quench ductility. So you've survived the event and have something else left. And just simply the historical origin of the oxidation and temperature limit.

CHAIRMAN POWERS: Here's where I get a little bit confused. If we go back to the title of your slide where you say we're going to revise criterion, we're not looking back at something that's now 30 years old. And quite frankly, a lot of water has gone over the technical dam in 30- 35 years. On the other hand, I am aware of experiments conducted in Germany with boron carbide control rods in which the quenching process did not progress in a classic quench and freeze process, but rather resulted in a rather exothermic excursion in the facility, and did significant damage even to the facility. So this step, it seems to me that don't let the cladding fragment, and going then to ductility seems to be a step that needs some attention, at least to establish the basis for going on through the ductility step.

DR. MEYER: Well, I guess I want to

respond two ways. First of all, we did not attempt to start from scratch, and start up at this line, and re-derive LOCA criteria. We attempted to stay down here and find a minimum change in the regulations that would account for the effects of burnup, and the effects of alloys that have been introduced to achieve the burnups. And that's where we're going with this presentation. But on the other hand, I have to say that there's been an awful lot of testing with quenching. The Japanese are doing a lot right now. And the retention of post-quench ductility always is sufficient to make sure that the fuel rod survives the quench process. It doesn't shatter. It doesn't oxidize rapidly during the quench process or anything like that. It's already in the steam. It's already oxidizing at the higher temperature.

CHAIRMAN POWERS: Can you explain Quench 8 then?

DR. MEYER: Hm?

CHAIRMAN POWERS: Can you explain that at Quench Test 8?

DR. MEYER: Can I explain?

CHAIRMAN POWERS: Quench -- the Number 8 test in the quench program.

DR. MEYER: This is a severe accident test

that I think is conducted under conditions more severe than we're talking about here because we put limits on temperature and oxidation in order to stay in this rather benign regime.

CHAIRMAN POWERS: But I think that raises questions on whether you're in the benign regime universally, doesn't it?

DR. MEYER: Well, I think we have ample demonstration that the regime is benign. If we limit the temperatures to about 1200 degrees Centigrade, and limit the oxidation, or as you'll see I'm going to take time at temperature, limit the time at temperature such that you don't embrittle the cladding to the extent that it would be subject to a lot of brittle --

CHAIRMAN POWERS: Remind me, what's the minimum eutectic in the boron carbide steel system?

DR. MEYER: I'm sorry?

CHAIRMAN POWERS: The boron carbide steel system, where's the minimum eutectic in that system?

DR. MEYER: In boron carbide? I have no idea.

CHAIRMAN POWERS: For some reason, 1100 degrees Centigrade sticks in my mind, but I could be wrong about that.

DR. MEYER: So you're thinking of control rods now?

DR. BILLONE: That's preferable, yes.

CHAIRMAN POWERS: Say that again, please?

DR. BILLONE: I was -- Farouk was going to speak. I was going to ask you if you wanted an example of when cladding does fragment, or are we past that point. And how it's related.

CHAIRMAN POWERS: That would be interesting.

DR. BILLONE: Okay. Generally, if I go back to the experiments of Hee Chung in the early '80s, what he found is that for any single rod test, or portion of a rod, it's very easy to survive quench if there are no other loads, until you get to very high oxidation levels. But if you oxidized at 1260 degrees C, which is above the limit, in let's say 17 percent ECR, you not only totally embrittle the material, and whether a brittle material fragments or has a clear break depends on the loading, but he subjected his loading to a pendulum impact test. Those samples did shatter. So generally in any experiment you do something else beyond just the quench. Either you're restraining it axially to put axial loads, or you literally after the experiment run

a series of mechanical tests, whether they be impact tests or not. But you generally -- you lose ductility first, then you go farther than that, and then you fragment.

So, it's sort of like being a little conservative. You want the material to be forgiving, and able to withstand --

CHAIRMAN POWERS: Yes, I think my understanding of the point that Dr. Meyer was making was that if he's got ductility at room temperature after the quench, he certainly had ductility at the higher temperature, and that therefore he has some conservatives in here. What I'm asking is do we know that this is really the right criterion in light of things like quench tests. I mean, these are tests that have taken place since this era of the '70s when this general strategy was developed. I'm just asking. I mean, it may be fine. I'm just asking if you're looking at it.

DR. MEYER: Well, we haven't looked at the severe accident test.

CHAIRMAN POWERS: Well, I mean theirs are not what I would call very severe accidents. They are -- I mean, they definitely don't melt fuel. They may melt control rods. But that's because there's a deep

eutectic in the boron carbide steel system, and I quite frankly don't remember where it is. And like I said, 1100 degrees sticks in my mind, but that could be off by 200 - 300 degrees. I simply don't remember.

DR. BILLONE: I'm sorry.

DR. MEYER: Should I go on?

CHAIRMAN POWERS: Yes, please.

DR. MEYER: Okay. So, this is the -- I'm going to call it the procedure that we believe is currently being followed. Some of it is in the regulation. Some of it is interpretation. The first part is in the regulation. The embrittlement criteria are in the regulation. They're in subparagraphs (b)(1) and (b)(2) of 50.46. There's a well known peak cladding temperature limit of 2200 degrees Fahrenheit, 1204 Centigrade. And there's a maximum cladding oxidation limit of 17 percent calculated on the basis of the total oxygen absorbed, and related to the thickness of the cladding with wall thinning taken into account if you're running this calculation for a region inside of the balloon. I'll show you what the balloon looks like in a little bit.

In Appendix K of Part 50 of the regulations you have ECCS models that are -- some required, some others acceptable, and in that it's

prescribed that two-sided oxidation be calculated within 1.5 inches of the location of the burst. In addition to that, in 1998 we issued an information notice with subsequent clarification by letter saying that total oxidation, which are the words that appear in the regulation, is interpreted by NRC to include both the pre-accident oxidation or corrosion, and the oxidation that takes place during the transient. So that's where we are right now. And what we want to do is see what needs to be done to that in order to accommodate burnup, and alloy effects.

Now, this is the bottom line, so I'm going to give you the bottom line now and then try and go back and explain how I got there. So if you can maybe save your questions till we talk about them. So at Argonne, we have data being obtained right now on unirradiated and high-burnup cladding to give us the technical basis. We've developed some -- what I think of as minimal modifications, trying not to totally revise the logic and conclusions of this hearing, but to modify the criteria in a way that would be least disruptive to existing licensing models and calculations. The criteria would be different from the current criteria inasmuch as it would not involve a fixed oxidation limit of 17 percent, but rather a

limit to be determined for each planning alloy, and we have determined that limit for all of the cladding alloys that are currently in use. And we'll mention those to you. Because the rule as it's written would require testing on each particular alloy. It's performance-based, and the criteria therefore would apply to all zirconium-based alloys, because you're going to test each one of them. For modern Zircaloy, and I'll make a distinction a little later between modern Zircaloy and old Zircaloy, and ZIRLO, and M5 cladding, which are the current types in operating reactors now, these limits would turn out to be 17 percent minus the corrosion thickness, that's the current practice. A limit on the time spent at high temperatures of 45 minutes from rupture to quench, which is -- the analyses that we're familiar with are all well below this, so it's not a problem, but I'll show you why such a limit is necessary. All calculations done with the Cathcart-Pawel correlation. If the compliance calculations have been done with the Baker-Just correlation that's no problem, it would be conservative in this application. But you're going to see that we're going to use this Cathcart-Pawel correlation as what I refer to as a time scale rather than anything else. And I'll explain that as we get

going.

MEMBER KRESS: Are the first two sub-bullets under there redundant?

DR. MEYER: No.

MEMBER KRESS: No?

DR. MEYER: They're not redundant. I'll tell you why. At the lower temperature, say 1000 degrees Centigrade, which is a very relevant temperature for small break LOCA, where you might sit there for a long time. You cannot get 17 percent oxidation at 1000 degrees before -- in some alloys, before you start getting breakup of the oxide, and high hydrogen absorption, and rapid loss of ductility. And so you need a time limit. So it's got to be less than both of those. In light of all this, we don't expect any re-analysis, and I hope to be able to show you that we haven't raised any safety concerns about the operating plants, because although they might not -- the licensing analysis might not have been done exactly this way, I think we can see that in fact if one had looked at these things they would have fit these criteria.

MEMBER KRESS: On the third bullet, Cathcart-Pawel gives you an amount of oxidation and energy release, but it doesn't tell you what

temperature.

DR. MEYER: It doesn't tell you what?

MEMBER KRESS: What temperature you're running at. It says 45 minutes from rupture to quench. Isn't that related to the temperature you're going to have? Isn't that related to what kind of LOCA you have?

DR. MEYER: It's related to the behavior of the cladding, regardless of what kind of LOCA you have. If you hold the cladding at a fairly low temperature, like 1000 degrees is kind of the prime temperature for looking at this because at lower temperatures the oxidation is slower.

MEMBER KRESS: That's sort of a limiting, though, value.

DR. MEYER: I'm going to show you some pictures, and you're going to see what happens here. But if you -- you simply can't get to 17 percent with some of these alloys before the oxide starts breaking up. And so we put this other limit on there. And you see --

CHAIRMAN POWERS: There seem to be a couple of assumptions here. One is that you anticipate all future alloys will continue to be a zirconium base. Is that correct?

DR. MEYER: All future alloys will what?

CHAIRMAN POWERS: All future cladding alloys will be zirconium based.

DR. MEYER: The staff requirements memorandum has specifically instructed us to write this for zirconium-based alloys. So the rule would say zirconium-based alloys. So if you had something other than zirconium-based alloys, then you'd have to go figure what criteria you needed for the ECCS.

CHAIRMAN POWERS: The other assumption that seems to be inherent here is that you come up to a temperature and kind of hold.

DR. MEYER: Yes.

CHAIRMAN POWERS: Is that what happens --

DR. MEYER: No.

CHAIRMAN POWERS: -- to fuel rods in a real accident?

DR. MEYER: We're well aware of your concern about the ragged temperature histories that could be experienced. We can address those. You will see that as with most diffusion-related experiments, that in order to determine the correlations, you need to do a set of isothermal tests. And then you put it together in such a way that you can reproduce the ones with varying temperature conditions.

CHAIRMAN POWERS: I have to admit I went through your derivation -- not yours, but someone's derivation on that, and that is perplexing.

DR. MEYER: It's what?

CHAIRMAN POWERS: Perplexing. Certainly not the way that people do it when they analyze things like DTA and TGA types of effects for thermal kinetics.

DR. MEYER: Well, let me encourage you to keep this level of question for Mike Billone, and I'll do the best I can when we get to it. I know one of the concerns that you have about spalling oxide during a temperature transient, and I can address that when we get down to it.

CHAIRMAN POWERS: Okay. Because I have not seen anybody address that question.

DR. MEYER: I'm sorry?

CHAIRMAN POWERS: I have not seen anything that addresses that question yet.

DR. MEYER: Yes. Well, you know, slides are just slides. They don't have all of the words that are going to flow here. I hope I can do that. Okay, so this is where we're going, and now let's start back at the basics, and see if I can tell the story in a comprehensible way.

So for the uninitiated, which are somewhere behind the table here, loss of coolant accident looks like this to the cladding. The power drops off, and the cladding starts to heat up. You've lost the coolant. Somewhere around 800 degrees ballooning takes place, the cladding gets soft, there's a big pressure differential. It balloons. It pops. That relieves the pressure differential. Coincidentally, at almost exactly the same temperature that this ballooning and rupture process is going on, the cladding -- the Zircaloy, or ZIRLO, or M5, or E110, all of these alloys are going through a phase transformation. At the lower temperatures the crystal structure is a hexagonal close pack, and we refer to that as the alpha phase. At the higher temperature the crystal structure is a body-center cubic, and we refer to that as the beta phase.

CHAIRMAN POWERS: What causes the temperature to drop?

DR. MEYER: Well, when you get ballooning, you get a thin cooling effect. And so if you're looking in the ballooned region, you would see that kind of. I don't know, there may be something else going on there. It's just typically there's a little blip there. I don't want to dwell on it.

CHAIRMAN POWERS: Probably not important. It's probably not an important blip.

DR. MEYER: Well, it's where the ballooning and rupture takes place. And this disturbs the thermal hydraulic conditions. And so you do get this kind of a little drop. This curve is not to scale. I simply drew this freehand, and later laid these temperatures in because I needed some calibration on the temperature scale since we're going to be talking about temperatures that are not higher than 1200 degrees Centigrade.

Once you get past the ballooning, then the oxidation process picks up. Prior to that, it's extremely slow. Even more important than the oxidation process is the diffusion process. And I hope if I succeed at nothing else during this presentation that I succeed at convincing you that what we're really looking at here is oxygen diffusion into the metal, rather than oxidation on the surface. And we're simply using oxidation on the surface, and it's kinetics equations to give us a time measure. Because what does the deed is getting oxygen from the surface into the metal, and embrittling these ductile metal phases

MEMBER KRESS: Well, my question earlier

about this temperature transient being related to the different kinds of LOCAs you may have, related to that because the diffusion of oxygen in a metal is related to this temperature transient, the rate at which it goes in, it seems to me.

DR. MEYER: Temperature and time. It's all about temperature and time.

MEMBER KRESS: Yes. But I was trying to rationalize how you could put a temperature and time on a number of LOCAs, how you could put a time on a number of LOCAs, because you've come down on just a time.

DR. MEYER: Are you referring back now to the 48-minute timeline?

MEMBER KRESS: Yes. You've eliminated the temperature.

DR. MEYER: Yes. Give me a few minutes.

MEMBER KRESS: Okay.

DR. MEYER: Give me a few minutes. Okay. So now you oxidize rapidly, and oxygen moves deeply into the metal. The ECCS coolant systems come on, and you cool in a manner something like this. You initially get steam cooling when the water is down low, and then you reach a temperature around 800 degrees where it wets and it quenches.

MEMBER KRESS: Is this a small break LOCA, or a large break LOCA?

DR. MEYER: Doesn't matter. It doesn't matter. We're talking about materials here. As long as you get temperatures above the 800 region, and you hold them up there for awhile, these processes are going to take place, and I don't think the cladding could care less whether a small pipe did it or a big pipe did it.

CHAIRMAN POWERS: Let me ask you a couple of questions that may not be appropriate for this slide, and may be appropriate later, but I'm not sure where to ask them. We did, what, two years ago we had a speaker come to us from France and discuss some experiments he was thinking of doing dealing in his case with large break LOCAs and clad ballooning. And what he discussed in that was the question of not just single rod ballooning, but multiple rod ballooning, and where it occurred, and whether it was all in a plane, or up and down, and whatnot. And he also discussed the fact that as we move up in higher burnup fuel we get more fragmentation, so when the clad balloons up, presumably you get some collapse of the fuel then to the ballooning and things like that? And he had little sketches of that.

DR. MEYER: Yes.

CHAIRMAN POWERS: And argued that that changed the heat in the balloon, the decay heat that you had in the balloon region. Is that taken into account when you conceptualize this figure, or talk to me about that stuff.

DR. MEYER: Some of it is and some of it isn't. And let me try and make the distinction for you. Multi-rod effects were studied by NRC 25 years ago. We were looking to see if the ballooning process on one rod would affect that on another rod, looking to see if the balloons all lined up in a coplanar manner which would be a worse situation than if they were staggered axially. The tests showed that they were staggered axially. We made licensing decisions on that basis, approved ECCS models on that basis, and so far as I know, there's nothing in the burnup phenomena that would alter that conclusion. And we have not opened up that area to reinvestigation in this program. There was a proposal from IRSN in France to run large tests in the Phebus reactor to revisit that situation, and that proposal is still pending. But we have not raised that as an issue, and I don't see any strong reason at this time to go into the multi-rod question again when the question that

we're pursuing is one of the effect of burnup and the effect of alloys.

CHAIRMAN POWERS: I think the more important aspect of high-burnup is actually the fragmentation of the fuel and its collapse into those balloon regions.

DR. MEYER: Okay, now that's another part of the question, and that is being included, but it's being included in single rod tests. And these tests are not only being done up at Argonne, but there's also an active program on this at the Halden project, where testing of longer sections under somewhat different conditions are being done with that as the principal objective. And we will bring that information into the seam here. But that kind of information itself does not affect the metallurgical response of the cladding in these particular criteria that we're trying to address here. It would affect ECCS models. It would affect the models that are being used to demonstrate compliance because it alters the heat source in the axial extent. But it wouldn't affect the criteria. It wouldn't affect the conditions under which the cladding would start behaving in a way that we didn't want it to.

Okay. Some of these I plan to move

quickly over, and don't mean to do anything but just show you an example. Here is a real fuel rod that's undergone a LOCA simulation. You can see it's swollen in the middle, and it ruptured right there.

CHAIRMAN POWERS: And it demonstrates that RES will never get to an SI unit system?

(Laughter)

DR. MEYER: This is a 40-year-old hot cell, and these --

CHAIRMAN POWERS: Oh, I see, 40-year-old hot cells, so our rulers have to be 40 years old too.

DR. MEYER: Well, steel tools last forever, right? So I know these steel rules that are laying in the hot cell up there, and you don't want to take that out.

MEMBER KRESS: What exactly do you mean by high-burnup here?

DR. MEYER: What do we mean by high-burnup. What we mean --

MEMBER KRESS: Is it 45, or 60?

DR. MEYER: It's very rough, but we mean something above about 40.

MEMBER KRESS: Okay.

DR. MEYER: At the present time, NRC has a burnup limit of 62 gigawatt days per ton average for

the peak rod. So we tend to test up at that limit. The sections that we test generally are a little above that limit because after all that limit was average for the peak rod, so if we go after some peak rods for specimens, then locally you can find segments that might be a little higher.

MEMBER KRESS: Where did you get your fuel? Are these all HB rods?

DR. MEYER: This one happens to be a BWR rod from Limerick. We have high-burnup --

MEMBER KRESS: Did you pull it out of the pool or what? Pull it out of the spent fuel pool?

DR. MEYER: Yes, EPRI did this for us for which we are eternally grateful. It was a big project to get fuel rods out of two plants. We're hoping to repeat this with M5 and ZIRLO clad fuel rods. We do have some small specimens of those that have come via an EPRI program, and we'll mention those later on.

Now, I'm going to have to talk about this complicated figure, because we simply can't make any progress unless we deal with some stuff here.

CHAIRMAN POWERS: It's not that complicated, is it? It's a cross-section of a piece of oxidized clad.

DR. MEYER: I should have expected that

from Dana. This simple figure then. So this is a cross-section of a piece of Zircaloy that has gone through a LOCA-like high temperature transient, been brought back to room temperature, sectioned, polished, put on a microscope, and this is what you see. Here is the oxide, this dark gray layer is the oxide that accumulated during the transient. The piece of tubing was fresh.

MEMBER KRESS: But first, what do you mean by room temperature ductility equals 6 percent?

DR. MEYER: Okay. You go through an oxidation process so we will -- in the testing, we will always -- and we generally try and do these isothermal tests. So we will mention the temperature plateau on which we did the isothermal anneal. And then we bring it down to room temperature.

MEMBER KRESS: Okay.

DR. MEYER: Sometimes we test at room temperature, but sometimes we test at a higher temperature. In fact, the regulation in 1973 was built on tests that were done at 135 degrees Centigrade. And we are doing that as well sometimes. And Mike will explain why we sometimes do it. I can explain that, but let me try and get on with it here.

This one had a very high amount of

ductility left, even when tested at room temperature. It would have had more than that, may have had more than that if tested at 135. 135 degrees is the -- what you call it temperature -- help me with the word, Harold. Saturation temperature right after quenching in a loss of coolant accident when you still have some containment pressure to give you a back pressure on the system. And so the ductility in the regulations for the last 32 years had been based on testing at that temperature. Okay. And that's what we're doing now.

Okay, so you have the oxide. Now, all of this material was initially in the beta phase after it got up to high temperature. But then oxygen started pouring into the metal at the high temperature. The beta phase, which here is labeled ductile metal, could only hold a small amount of oxygen. And once you got more oxygen in there than it could handle, it would convert back to an alpha phase, even at high temperature. We'd sometimes call it an oxygen-stabilized alpha phase. So here you had two distinct varieties of this oxygen stabilized alpha phase. One is a fairly well defined layer with a very high oxygen content, and then you had these fingers protruding into the ductile beta regions that have oxygen

concentrations higher than the solubility limit of oxygen in the beta phase. You cool it all back down. It's all alpha phase now, but it retains this distinctive appearance of the phases as they existed at high temperature. And as you can see, this one, this dark prior beta phase, is the only one that's ductile. And as that disappears, you're going to lose ductility of the material as a whole. Or, if it's ductility, it's spoiled. And each of those is possible. We can come back if you --

CHAIRMAN POWERS: It probably would be useful to inject a phase diagram at this point so that people know that the alpha phase becomes stabilized as oxygen absorbs into the material. And that raises this question. You're going to explore multiple different kinds of alloys with different alloying agents, and try to get something general here. And yet at no point do you discuss the rather well developed rules on how the band structure of the two phases of zirconium metal respond to alloying agents. And that's surprising. Why doesn't that appear in this discussion?

DR. MEYER: Well, the reason I didn't do it was because there are differences. Mike will show you some and can talk about them. But the general

character is the same. I will say that this almost looks more like an M5 or a ZIRLO structure where they're very prone to these protrusions into the ductile layer. Sometimes the Zircaloy will have just three distinct layers where you only see a nice uniform prior beta layer in there. But I chose this Zircaloy slide because it exhibits the features that we see in all of the alloys that we've looked at. Now, the temperatures at which the transition takes place are slightly different, and there are variations. But the general character is the same.

Now this is the thing that makes the subject so complicated. We're looking for embrittlement, and there's not one cause of embrittlement, but there's several causes of embrittlement. And here's where you have to have that concept of different phase layers. I'm just going to glibly refer to them as layers in mine. Because the beta phase has such a limited oxygen solubility, it keeps -- as you try and put too much oxygen in it, it keeps converting portions of it to this oxygen-stabilized alpha phase. And so the true beta material keeps shrinking. So it gets thinner and thinner. Eventually it gets so thin that the sandwich of several phases just appears to be brittle. And that

point is determined empirically by testing macroscopic pieces of the material.

CHAIRMAN POWERS: If we get these intrusions -- When you've got distinct layers, oxide, embrittled metal, ductile metal, you say, gee, all my ductility is in this layer of ductile metal, and it's fine, you can get that down fairly thin and still retain the integrity of the metal structure. But now if you get these intrusions coming in, doesn't that change how you look at these things? Don't you have to worry about something like percolation phenomenon, or something like that?

DR. MEYER: Sure, if we were trying to model this mechanistically. But what we do is just a macroscopic empirical test on a piece of material, and use a screening test to determine if it's ductile or brittle. It works very well, and we don't make any attempt to mechanistically model the mechanical behavior of this tangled mixture of phases.

CHAIRMAN POWERS: Criteria will come out to be about the same. Remarkable.

DR. MEYER: I'm sorry, I can't --

CHAIRMAN POWERS: The criteria all come out to be the same, yet if you were looking at a percolation across that, you would expect the amount

of oxidation you'd have to get through would be less than if you just had to thin the layer down. But they come out to be about the same. Interesting.

DR. MEYER: Let me go on. You can ask those kinds of questions again to Mike, and maybe he can answer them. But you do have situations where the beta layer can get so thin that the macroscopic material appears to be brittle.

CHAIRMAN POWERS: Say if we were talking about, I don't know, maybe steam generator tubes, we would be talking about ligaments retaining strength and things like that. And here you're getting up those ligaments because of these protrusions. I mean, it's just interesting.

DR. MEYER: Okay. There are also conditions under which the beta layer itself, where you can -- the solubility is increased to the point that you can put so much oxygen in the beta layer that it gets brittle. So you've got to look at that. We have this matter of breakaway oxidation which occurs, and I'll show you examples of that. And when the breakaway oxidation occurs, then you have a rapid absorption of hydrogen. And the hydrogen then quickly induces embrittlement. How that takes place mechanistically you can -- Mike.

CHAIRMAN POWERS: Let's see. You put oxygen in, and it sucks electrons out of the conduction band, so when you put hydrogen in it injects electrons into the conduction band that makes it solute more oxygen?

DR. MEYER: Well, the first thing that hydrogen does, and Number 3 and Number 5 may be closely related, but the first thing that hydrogen does when you get hydrogen into the metal is it increases the solubility of oxygen in the beta phase. I don't understand the mechanism for its doing that, but it does that. And when it does that, then the beta phase itself can absorb enough oxygen to become brittle.

CHAIRMAN POWERS: Well, bear with me a little bit and assume that it affects on electrons in the metal structure. Alloying agents also affect it.

DR. MEYER: I just didn't hear.

CHAIRMAN POWERS: Well, if indeed the effect, the hydrogen enhancement of oxygen solubility is due to the electronic structure of the metal, and my point is that alloying agents also affect that electronic structure.

DR. MEYER: Yes.

CHAIRMAN POWERS: And you could make a

more generalized description of this, it seems to me, to cover who knows what alloying agent may come along by focusing on that electronic structure.

DR. MEYER: Well, we might be able to if we went that deeply into it. And we haven't done that. It's a modest program. We're looking empirically at the results of these phenomena, and we haven't tried any modeling of the electronic structure, or binding energies, or anything like that. This is simply beyond the scope of the work that we've tackled. We've simply tried to look at what the consequences would be by making practical measurements that we can interpret.

We have one other cause of embrittlement that we in fact talked about the last time we were here, and I'll bring it up again, and that's in the balloon itself, where you have a rupture, and you have two-sided oxidation because now steam can get on the inside of the balloon, there's a tendency to trap the hydrogen that's freed from the dissociation of water that participates in this ID oxidation process, because it's not being swept away by steam. And then we find that you get enhanced bands of hydrogen absorption in the cladding above and below the opening of the balloon. And this leads to hydrogen-induced

embrittlement in the balloon. And it also may be related to the oxygen solubility, or it might be something else. I can't tell you. All I know is that the phenomenon exists, we measure it, and we can show it to you in detail in Mike's talk. But we have to look, if we're going to have an effective set of criteria, we've got to look at all of these things to make sure that none of them leads to embrittlement within the bounds of the criteria that we establish. And we're trying to get this all simplified to the point where it can be done in a practical sense in an ECCS calculation where you only have a few parameters available.

So, now I'm going to go through these more or less one by one, or a couple at a time, and point out that then and now the 1200 degree temperature limit had to do with the embrittlement of the beta phase in the material. At temperatures above 1200 degrees Centigrade, or 2200 degrees Fahrenheit, the solubility limit of the beta phase, which is temperature dependent, it simply goes up. And so the beta phase itself starts becoming brittle at temperatures above 1200 degrees Centigrade. As a practical matter, if you're doing tests at 1000, 1100, 1200 degrees, and you get up around 1200 degrees, and

you look at 1170, 1180, at 1200 you see that the ductility is starting to fall off fairly rapidly. And this is why the 2200 degree Fahrenheit limit was put on there in the first place. And we don't have any plans to change that. It's true then, it's true now, and we don't plan to tinker with the 2200 degree Fahrenheit limit.

CHAIRMAN POWERS: It's only true for the alloys you looked at. Is there no alloying agent that I could stick in there that would stabilize beta to the point that you could get enough oxygen solubility to embrittle it?

DR. MEYER: I don't know. What I can say, though, is we can work within this construct. If we choose 1200 degrees Centigrade, we choose to stick with that as a limit, then we can explore the other parameter that affects the embrittlement, the time at temperature, which is reflected in the oxidation limit, and explore that, and map out ductile and brittle domains. I told you you're going to be able to ask us questions that we couldn't answer.

CHAIRMAN POWERS: And I wanted to live up to your expectations, Ralph. That's all I'm trying to do. I didn't want to disappoint you.

DR. MEYER: Okay. Now, so we've discussed

most of this. Oxygen diffuses into the metal. The beta layer shrinks. If you limit the time at each temperature, then you can keep the beta layer from shrinking too much and retain an effective ductility of the whole sample.

Now, I think I'm about to try and make my big point, but the -- I'm going to try and convince you that there's enough oxygen as a source on the surface of these materials that it's really not -- the thinning of the beta layer is not a direct consequence of how much additional oxide you pile up on the surface, but how much time you allow at each temperature. So, we're going to choose the Cathcart-Pawel correlation as our temperature scale. And what this does for us is gives us, you know, if we put a number, 17 percent, which is the old comfortable number, on the Cathcart-Pawel correlation, what this gives us is a time scale for different temperatures, 2400 seconds at 1000 degrees, and smaller amounts at 11 and 12. So we're going to try and use this and see if it works.

And so here is an oxygen concentration diagram from a report by Hee Chung, which I have annotated to show it in the way I'd like to think of it, is that you have an oxygen source sitting on the

side of your metal, and when the temperature is high enough, oxygen diffusion will pump oxygen into the metal itself. Now, on this slide I want to try and digress, and comment on your concern about oxygen spallation during some temperature transient. I think if you chiseled off half of that oxide thickness, it wouldn't make a wit's difference in how much time it took to embrittle the material. And so if you're using an equation which is fixed, like we are. For our time scale we're calculating the time that it takes for oxygen to move in and thin the beta layer. And although that calculation was derived from an oxidation process, we're really not using it for that. And if you lost some of the oxygen source, you would quickly replenish it. And the amount -- the true amount of oxygen that was taken from steam and put onto this system here, that might be different in the region of some spallation, but I don't think it would make any difference on what was going on inside of the metal because it found ample oxygen on the surface, and all we're waiting for here is enough to arrive in the beta phase to alter its properties. And so that's both the point that I want to make about time at temperature rather than oxidation on the surface, and hopefully an answer to your longstanding concern about

the flaking of -- or spallation of oxygen during a temperature transient that might -- a non-uniform temperature transient that might kick off some spalling, some oxide that we would miss by doing tests under uniform heating conditions.

CHAIRMAN POWERS: I understand your point. I'll have to think about it for oxygen driving. I think that you've raised a different question now, is do you not now get an enhanced hydrogen uptake in this system?

DR. MEYER: What about hydrogen?

CHAIRMAN POWERS: If I spall the oxide, don't I get an enhanced hydrogen uptake in this system?

DR. MEYER: I don't think so, because I mean we have -- we do a lot of testing with bare cladding, and we don't get enhanced hydrogen. In fact, I don't think we get significant hydrogen uptake during -- help me, Mike -- during the high temperature oxidation. We don't get a lot of hydrogen, do we?

DR. BILLONE: No, very little. Maybe 10 - 20 ppm of hydrogen. Except for E110.

DR. MEYER: Yes, which had the breakaway.

DR. BILLONE: Which has a breakaway.

DR. MEYER: And when you get to -- and you

can get that in any alloy, and I'll show you a figure on that, just one or two more slides down the line here.

CHAIRMAN POWERS: I guess what I'm struggling with is what's the difference between breakaway oxidation and spalling the oxide?

DR. BILLONE: You have to -- breakaway means the oxide layer is literally breaking up, cracking. And that's followed by device-lamination, where it actually separates from the surface, and then spalling. So those are related, but there are different kinds of oxide layers. So if you're really talking about -- let's call it the corrosion layer, the thing that grows in-reactor. That's what's on the metal as you start the transient. And if you're asking does it matter if some of that flakes off, if that was your question, that's a different question than asking --

CHAIRMAN POWERS: Get halfway through this --

DR. MEYER: Also, mention the form of the oxide, because the normal form of the oxide that we're working with is a shiny, black tetragonal oxide, and the breakaway stuff is white in color, and I think it's monoclinic.

DR. BILLONE: Right.

DR. MEYER: So, when the breakaway phenomenon takes place, other stuff's happening that you don't see in this situation.

CHAIRMAN POWERS: That's fine. You get halfway through your story there. And now you go through a sudden drop in temperature. The stuff spalls, it fractures, it looks just like your breakaway stuff. I mean, I defy you to tell the difference if I showed you two specimens.

DR. BILLONE: No, no.

CHAIRMAN POWERS: I don't defy you. I defy him. Let me be clear on this.

DR. BILLONE: If you want help, Ralph, just ask.

DR. MEYER: Okay. Point taken. So, the proof of the pudding is in the eating, and so now I've gone back to plotting things in a fairly familiar manner. I've plotted what we call offset strain, where we reckon zero ductility to be at 2 percent.

CHAIRMAN POWERS: I'm going to ask you --

DR. MEYER: And there's a lot of details here. I'm not going to describe these details. If you want them --

CHAIRMAN POWERS: I looked with some

diligence to understand what you mean by offset strain.

DR. MEYER: I know exactly what it is, but I'm going to let Mike talk about it. He's going to show you the data.

DR. BILLONE: Deviation from plastic strain. Non-linear.

CHAIRMAN POWERS: The strain where you deviate from the Hook's law?

DR. BILLONE: Yes. You deviate from Hook's law, and it's permanent, meaning you unload, it's still there.

CHAIRMAN POWERS: You might want to make sure you define that on your nice -- you have a nice little plot.

DR. BILLONE: In my first 10 minutes I'll show it.

CHAIRMAN POWERS: And whatnot, because I looked for it, and said I'm not sure what they mean by offset. I know what I mean, which is deviation from Hook's law. And but it's not defined. And you might want to define it.

DR. BILLONE: I have it in my presentation, exactly the slide you want.

DR. MEYER: Okay. Now, keep in mind this

is unirradiated. So we're going to show you some results here for unirradiated cladding, and then we'll get to the irradiated stuff. Unirradiated new Zircaloy, 17 percent line is right here. So all of these retain their ductility for the time it took to get to 17 percent with the Cathcart-Pawel equation.

This is ZIRLO. You see the same result. It's got a little more than the Zircaloy, but close. And here is M5. I don't think -- we don't have another data point down here on the M5 I don't believe, but you can see where it's heading, and you can see that for practical purposes, the 17 percent limit, which is the one we're using to date, works for all three of these unirradiated alloys.

Now, it doesn't work for everything. And here's an example where it doesn't work. Here's old Zircaloy. It comes down around 13 percent. And back in the '70s and early '80s, when we were using this kind of cladding, we were also using the Baker-Just correlation because it was required by Appendix K, and that requirement wasn't lifted until 1988. And the 17 percent calculated by Baker-Just is the same as 13 percent calculated by Cathcart-Pawel. So what I'm showing you here is a reproduction, or a confirmation of the early work that Hobson did, and the criteria

that were derived from it. The difference between old Zircaloy and new Zircaloy has largely to do with the surface finish. And you'll see that surface finish can have some pretty dramatic effects. It's one of the two fabrication processes that we've identified as having a fairly strong effect on the behavior of the cladding under these high temperature LOCA conditions.

CHAIRMAN POWERS: I looked at that particular note at the bottom, and at first I said, oh yes, I know exactly. Then I thought about it. What you mean is the time required for Baker-Just to get you to 17 percent is the same as the time required for Cathcart-Pawel to get to 13 percent? Is that what you mean?

DR. MEYER: That's what I mean.

CHAIRMAN POWERS: At some particular temperature, 1200 degrees I guess.

DR. MEYER: If I could have thought of those words, I would have put them on this slide.

MEMBER DENNING: Ralph, would you go back to the previous slide. There's something I want to question you about.

DR. MEYER: Before that?

MEMBER DENNING: It was the one on the unirradiated cladding for the ZIRLO and the new Zirc.

Those figures.

DR. MEYER: The figures for?

DR. BILLONE: Go back two more slides.

MEMBER DENNING: That's fine.

DR. BILLONE: That's ZIRLO.

MEMBER DENNING: Now, as we look at the data there, there's not a consistency in terms of as a function of temperature.

DR. BILLONE: The test temperatures are different, so make sure Ralph clarifies that.

MEMBER DENNING: Yes, the test temperatures are different, okay. But there's not a --

DR. MEYER: Oh, you're looking at that rather than that?

MEMBER DENNING: I'm looking at those, right. And then you see that, you know, there's not a pattern where the curve is dropping down with, you know, the curve is lower for the next temperature, and then lower for the next temperature. Is that an indication of the uncertainty in the data there, or is that really something phenomenologically where there's a minimum that one's going through?

DR. MEYER: It's an indication of the monoscope of the program. What we did, and we did

this deliberately, when we could run a test at room temperature and get ductility, we did it because it's cheaper to run a test at room temperature than at 135 degrees. When we didn't get -- and that was sufficient. So you knew that if you tested at room temperature you got ductility, you would have ductility at 135 degrees. It was only in the cases where the ductility was near zero when tested at room temperature that we switched to the more expensive testing at 135. And we haven't filled out this whole matrix. I mean, there's a lot of testing that goes into getting a curve like this.

DR. BILLONE: But to directly answer your question, if that red line were done at room temperature -- it was really done at 135 -- you would see your trend. In other words, you would lose ductility for ZIRLO at about 10 percent, and you would have your trend. But you had two room temperature tests, and one test at 135 degrees C, and that's why you're not seeing what you're looking for.

DR. MEYER: In the beginning we thought we could get by with doing room temperature tests.

MEMBER DENNING: I see.

DR. MEYER: But we had to switch.

MEMBER DENNING: I see. On the 1200,

that's -- I understand. Okay.

MEMBER KRESS: Each one of those points is a different test.

DR. MEYER: Yes. Each point is -- this is a short piece of tubing, a couple of inches long, that was oxidized at 1000 degrees until a time that corresponded to a Cathcart-Pawel prediction of about 12 percent ECR.

MEMBER KRESS: I was trying to rationalize --

DR. MEYER: And then you cut a ring from that, and you --

MEMBER KRESS: You subject it to the --

DR. MEYER: And you squeezed it.

MEMBER KRESS: I was trying to rationalize the blue curve. It's just the data is uncertain, probably. Rather than being smooth.

DR. BILLONE: When material is extremely ductile it's very subjective and difficult to determine how ductile. The test is better, easier to interpret the data as you approach embrittlement. So some of that could be judgment, what's your seeing there, judgment in interpretation of a curve that has no sharp load drop indicating a significant crack all the way through the wall.

MEMBER DENNING: And if you were to put an uncertainty bound on that from your best judgment, what would they look like, if you put an uncertainty bound or a reproducibility for the same conditions. What would that look like?

DR. BILLONE: We've done a lot more testing with the 15 x 15 cladding, because we had a lot more of it. And we got very reproducible results. I think the bigger -- the broader question that could be asked is if you go lab to lab to lab with different testing techniques, do you see the same data trends. And the answer for this -- go the next slide, please -- is for the M5 and the Zirc-4 we have the same data trends for unirradiated cladding as CEA-Framatome-EDF have, using very different testing techniques. And so we don't run a lot of tests with these alloys, but once we get a set, and once we get a pattern for screening tests, we tend to look at what other people have gotten, and are we consistent. And that's how we sort of look at uncertainty.

MEMBER KRESS: The line going straight up from the first point on the green one, we do know that point. The 12-minute -- the 13-minute point.

DR. MEYER: On the green curve?

MEMBER KRESS: Yes.

DR. MEYER: That one?

MEMBER KRESS: No.

DR. MEYER: That one?

MEMBER KRESS: The line above that going straight up. How is it derived?

DR. MEYER: Oh, there's another point up here.

DR. BILLONE: There's two more points, at 10 percent and 5 percent and zero. Three more points.

MEMBER KRESS: Okay.

DR. MEYER: We really didn't want to plot those points because they're -- it's a screening test to try and find out where zero is. And when you've got 20 or 30 percent ductility the test isn't that accurate nor interesting.

MEMBER SHACK: I thought you needed your 45-minute limit for something like M5 where to get 17 percent oxidation at 1000 C, you're running presumably for a fairly longish time. You're doing everything here just with your 17 percent. Where would my 45 minutes catch me?

DR. BILLONE: Forty-five minutes is, let's see. For this particular curve, if I went -- Ralph, I can't read your legend. What is 1000 degrees C curve?

DR. MEYER: The blue.

DR. BILLONE: Blue is 1000 degrees? Okay. At approximately 20 percent ECR you're at about 3400 seconds, and none of these alloys that you've seen have broken up in our tests. The breakaway oxidation has --

MEMBER SHACK: So these --

DR. MEYER: The breakaway here is going to be about 5000 seconds.

MEMBER SHACK: So for these three alloys, the 45 minutes is --

DR. BILLONE: Conservative.

DR. MEYER: This is double-sided.

DR. BILLONE: Yes, but the 1000 degrees C test was close to an hour, was run for close to an hour. As a matter of fact M5 was run for 4100 seconds, which is more than an hour, and in France it was run for 5000 and greater. It's all these alloys, all zirconium-based alloys, the oxide will break up after a certain amount of time, Zirc-4, ZIRLO, M5. It's not a particular M5 issue as far as breakaway oxidation.

MEMBER SHACK: Okay, but that would make the 45-minute rule look quite conservative, then, for M5.

DR. BILLONE: And for Zirc-4.

MEMBER SHACK: And for Zirc-4.

DR. BILLONE: I mean, what do you call quite conservative? One of these alloys starts picking up hydrogen at 3400 seconds, it hasn't shown dramatic --

MEMBER SHACK: Well, it just seems that if I did it on my Cathcart-Pawel 17 percent, I'd be conservative enough.

DR. BILLONE: Oh, okay, I'm sorry. This is -- Ralph's right. This is two-sided oxidation. The time is relatively short to get to 17 percent. If you did one-sided oxidation you increase the time by a factor of four.

MEMBER SHACK: Four. Okay.

DR. BILLONE: And that's what we're trying to --

MEMBER SHACK: That's where you would find it.

DR. BILLONE: I'm sorry, I missed the point.

DR. MEYER: Okay, I think you may have seen some of these pictures before. But these are the zirconium-1 niobium alloys, the Russian E110 at the top and the French M5 at the bottom. And you can see

that the E110 has gone south quite early at 290 seconds. This is at 1000 degrees Centigrade. It's worse at 1400 seconds. At 2400 seconds M5 is still shiny and hasn't developed any spots on it at all. But very easy to see the difference when you run the test, and of course you could cut a ring and do a mechanical test and you would find the stuff that's brittle, and this is ductile.

CHAIRMAN POWERS: And the only differences are texture effects?

DR. MEYER: Are what?

CHAIRMAN POWERS: Texture effects?

DR. MEYER: No. I'm glad you asked the question. I would -- how bad am I on time? I'm supposed to finish in 15 minutes? Or now?

CHAIRMAN POWERS: Fifteen minutes.

DR. MEYER: I've got 15 minutes. I'll give you the 2-minute version of the E110 story because it's fascinating. At the end of the day we found two fabrication parameters that made the difference between this and this. And neither of them were what you would have expected. One of them is surface finish. You grow a crystal on a substrate, the condition of the substrate matters. And that's true here. The Zirc-4 oxide is a crystal also, and an

ionic one, and it's sensitive to that. So the E110 has a very rough surface finish, and the M5 has a very smooth polished surface finish.

That's one factor. That's not the whole story. The other one was the one that was equally surprising. And it has to do with the reduction process that's used to go from the Zircon or to the zirconium metal before you even make the ingot. It turns out that the Russians were using an electrolytic process that produced very pure zirconium. And everyone in the west, including the French and the U.S. use a kroll process which doesn't produce such pure zirconium. It has some impurities in it like calcium. Calcium has a valance of three. Niobium has a valance of five. Zirconium has a valance of four. Five and three kind of balance out, and so if you get some impurities from the kroll process into the zirconium, it kind of acts as the antidote to the niobium that you're going to put in, which seems to increase the sensitivity of the process. So if you use sponge zirconium in the fabrication process, you generally get nice behavior. If you use electrolytic or iodide zirconium, you tend to get this. The Russians had some specimens that were made earlier with sponge zirconium. They bought some sponge

zirconium from CEZUS in France and made some E110. It behaved quite nicely. They produced some of their own sponge zirconium and made some E110, and it behaved nicely. But the commercial stuff is made with electrolytic and iodide zirconium and it behaves like that. And who would have guessed that that step and the surface finish would have produced the kind of sensitivity that we see.

CHAIRMAN POWERS: Why does calcium have a valance of three?

DR. MEYER: Hm?

CHAIRMAN POWERS: Why does calcium have a valance of three?

DR. MEYER: Doesn't it? In the oxide? I think it does.

CHAIRMAN POWERS: I don't think it so. I think it's two. But I can always be proved wrong.

DR. BILLONE: Don't look at me for chemistry.

CHAIRMAN POWERS: When you say the advantage of looking at the electronic structure of these alloys is tremendous, because --

DR. MEYER: I shouldn't have got into that discussion anyway, so we'll have to -- it's something like that. Hee Chung is the proponent of this

hypothesis. The fact is that sponge zirconium and a good surface finish seem to produce cladding that behaves very nicely under high temperature LOCA conditions, and other starting zirconium materials and rough surfaces produce poor behavior.

Here is a slide from a recent paper, Jean-Paul Mardon who is here, going to make a presentation I think, is the principal author of this. And it shows for Zircaloy and M5 the breakaway process beginning out here around 5000 seconds. And 45 minutes is 2700 seconds. And typical LOCA times are 1800. So we've just pegged that number in the middle there. And for these three -- this number is not proposed for any regulation or anything. It's just a number that says for these three alloys, Zircaloy, M5, and ZIRLO, if the length of time is less than 45 minutes you're okay, and the typical LOCA times are less than that so I think we're okay.

Okay. One other feature -- we talked about this a year and a half ago when we met, and this has to do with the hydrogen absorption in the balloon. So here is a plot with the burst location in the center, and going in both directions you see a symmetric behavior. The blue is the hydrogen concentration and you see it increasing quite

dramatically up to 3000 to 4000 ppm, which is quite high, at distances on the order of 6 or 7 centimeters from the center of the balloon where the rupture is. These produce what I think of as bands of embrittled material in the balloon, even when the 17 percent criteria and the temperature criteria are met, according to a calculation in the balloon. This is not a new observation. This was observed and reported in 1980 by Argonne National Laboratory and by the Japan Atomic Energy Research Institute. So it's an old observation, and we simply recognized it and go on with it.

So what we propose to do in the balloon section is basically leave the regulation as it is. We recognize that you won't have ductility everywhere in the balloon. There'll be a couple of local regions. In our previous meeting I referred to them as singularities. Maybe they're a little broader than singularities. But by retaining the current rule where you calculate the oxidation in the balloon region, assuming double-sided oxidation, taking account of the thinning from the ballooning process, all of which is described in the regulation, that what this will do for you is it will protect areas within the balloon that are ductile. And this will mitigate

any fracturing behavior that might take place in the balloon, and tend to keep it from shattering. So that if you had fracturing, you are likely to have just clean breaks. And we've seen some of those with material that's gone through this process, and they do tend to be clean breaks. And if they're clean breaks, then the pellets can't get out. You might lose some granulated material, a small amount, but I don't think there's any threat to coolability from that small loss. The rods can't displace an offset enough for much material to go out. In fact, with the order of magnitude of ballooning that we see, the balloon will nearly touch the neighbor rods so that lateral movement of the balloon in this location is going to be restricted anyway.

Now, I think I have very little on what is the most important subject for the high-burnup effect, and that is the hydrogen enhanced beta layer embrittlement, the last of the causes that I listed. Now we're talking about hydrogen that comes in during the corrosion process. So during normal operation, at operating temperatures where the cladding is around 300 degrees Centigrade, over the lifetime of the fuel if you get 20, 40, 60 microns of corrosion or oxide built up on the surface, you may get 20, or 50, or

200, or 400 ppm hydrogen absorbed into the metal. And it's this hydrogen that is available in the metal to alter the solubility limit of oxygen in the beta phase at high temperature. And so that's what it does. And we therefore have to do something to account for this. And the interim requirement of a few years ago was to say that total oxidation meant pre-accident oxidation plus the transient oxidation.

So what we did was look to see if that would work. This is done in a very pragmatic, empirical manner. We take the corrosion layer and convert it to this ECR equivalent cladding reactive percentage, subtract it from the 17 percent, and see if that is an adequate accounting for the hydrogen. Now, there's a phenomenological link here. It's not a mechanistic model, but the logic is there. The more corrosion you have, the more hydrogen you get into the metal. The more hydrogen you get into the metal, the higher the solubility for oxygen in the beta layer and the lower the embrittlement. So it hangs together logically. And in the one series of tests that we've done on high-burnup cladding it seems to work. This is 1200 degrees Centigrade with the post-quench testing at 135 degrees Centigrade. I can't read that. What's that.

DR. BILLONE: The red are the data points.

DR. MEYER: Oh yes, and these are -- and these are -- yes. The red are the data points --

DR. BILLONE: For steam oxidation alone.

DR. MEYER: I shouldn't have gotten tangled up on this slide.

DR. BILLONE: Your legends are so small.

MR. SCOTT: We added the corrosion thickness to the layer --

DR. MEYER: There you go. There you go. Thank you Harold. Thank you. So we did the reverse here. We added the corrosion thickness to the test results and compared that with the 17 percent, or the 13 percent in this case because it's old Zircaloy. It works. I'm sorry I didn't present this slide in a more elegant manner.

In the program plan, and up until very recently, this is all we expected to have on the burnup effect. That is, test results for Zircaloy from high-burnup fuel. Because we did not have on this time scale access to high-burnup ZIRLO and M5 runs. So the plan was laid out. It was endorsed by everyone that what we would do was to investigate the effect of alloys on unirradiated specimens, and investigate the effect of radiation on Zircaloy

specimens, assume that the radiation effect played out the same with M5 and ZIRLO. Fortunately we're going to be able to do better. We haven't done it yet, but we will.

Here are the data for ZIRLO. There are no points on here because the tests are in progress right now. And M5. We were able to get some small pieces of irradiated M5 and ZIRLO cladding shipped back to us from Studsvik in Sweden, cladding that was over there, the ZIRLO cladding at least, on an EPRI program. And so we have these cladding specimens in the laboratory at this time. They're being moved into position in the hot cell, and we will do this kind of testing on the M5 and the ZIRLO, and the next time that I show these slides I hope that we have data points on here. And I have full confidence that they're going to come in above this line.

So, the criteria. The criteria that could be used, and we think are justified by this research would require some input. The applicant, or vendor, or whoever would have to determine an oxidation level in unirradiated cladding at which the ductility disappears. Easy test to do because it's unirradiated material. You don't need a hot cell for that. You'd have to take some additional segments at a lower

temperature and find the time at which breakaway oxidation occurred, and declare a time that would prevent that from happening, and use that as a limit. And then you also have to know what the corrosion thickness is on the cladding of interest. And so you put those all together, and in the LOCA analysis then, in your ECCS calculations, you calculate the cladding oxidation during the LOCA and show that it doesn't exceed that limit that you just found. This one. I'll tell you, that number is 17 percent for the alloys that are being used, but it could be different. And then you show that the calculated time from rupture to quench is less than the time for breakaway oxidation, and you do all of this with Cathcart-Pawel. And that's the proposal.

And the conclusions are that we've looked at old Zircaloy, new Zircaloy, M5, ZIRLO. All of them seem to fit. We've determined what the oxidation value is, where ductility is lost. We've determined where the breakaway oxidation process begins. We've put it all together in that manner, and we haven't found any problems. I don't think any re-analysis of operating reactors would be needed. You don't have to change any ECCS models. The criteria would work for small break and large break, the cladding really

doesn't care.

The change that we made to add the time limit is especially appropriate for small break, so I would make that estimate. I also think that these criteria would be valid for modest burnup extension because we really didn't see a burnup effect per se. It was a corrosion effect, which is a consequence of the burnup process. And because we have tested specimens with a lot of corrosion on them, I think we probably covered the corrosion range that would be experienced with newer cladding that were taken up there. So my expectation is that you could justify this at higher burnups than the current limit of 62 gigawatt days per ton. And I think because these criteria as they would be written in a regulation would be performance-based, they would apply to all zirconium-based alloys. And this would avoid the need for exemptions from the rule for not only the current alloys, M5, but newer alloys such as low tin, ZIRLO, and other alloys that might be coming along.

I'm finished. He didn't see this coming. This is Mike Billone who's sitting over here. And I just wanted to mention that Mike recently got a very nice award at the University of Chicago. And here he is with this important body himself, the president of

the university, his important vice president, and the director of Argonne National Laboratory. So I hope you will let me sit down now and congratulate Mike.

(Applause)

MEMBER SHACK: He even got enough of a bonus he could buy another tie.

CHAIRMAN POWERS: Seems to be a tradition of conservation of ties at Argonne. Members have any questions of Dr. Meyer? Dr. Meyer, could you put up your slide in which you showed the layers and argued that spallation made no difference?

DR. MEYER: Okay. You've got to read to do this. I thought you just had to look at pictures. Oh, when I do that it's going to put me to the beginning.

CHAIRMAN POWERS: This'll do.

DR. MEYER: Do what?

CHAIRMAN POWERS: This'll do fine. Okay. What I would argue is that if you're at a fixed temperature, you're having a quasi-steady state across each one of those layers, because there's a fixed oxygen potential at each interface, right?

DR. MEYER: Yes, probably.

CHAIRMAN POWERS: Okay. If I thin one layer, the flux across that layer has to increase.

DR. MEYER: If you thin the layer?

CHAIRMAN POWERS: Sure. Cut that layer in half, the flux across it's got to go up. So if I spall off a little of the oxide, the flux across the layer's got to go up because the oxygen potential at each interface is fixed.

DR. MEYER: Oh, I don't know.

CHAIRMAN POWERS: How else could it be? The oxygen potential --

DR. MEYER: If you thin the layer the flux has to go up?

CHAIRMAN POWERS: Sure. The oxygen potential at each interface is fixed.

DR. BILLONE: But at the instant you take it off, it hasn't changed. The oxygen profile hasn't changed. Then it grows. That's true.

CHAIRMAN POWERS: It has to go faster too because the oxygen flux across the layer went up.

DR. BILLONE: Then it catches up.

CHAIRMAN POWERS: Yes. And that's usually called oxidation, and it's more rapid, and things go faster.

DR. BILLONE: That is correct.

CHAIRMAN POWERS: Yes.

DR. BILLONE: You're correct, and Ralph

may also be correct, that as long as you have enough oxygen at that alpha/beta interface to pump into the beta layer, you're going to embrittle the material, with or without that spallation. You might both be correct.

DR. MEYER: Certainly the oxidation rate would speed up.

CHAIRMAN POWERS: It has to.

DR. MEYER: Oh, I agree with that. But I don't think the rate of diffusion into the metal is going to change much. If anything, it's going to slow down a little, which is the opposite of what you were thinking, I think.

CHAIRMAN POWERS: It's going to slow down a little? I would be fascinated to listen to that argument.

DR. MEYER: If I took half of the oxygen source off of there, I think --

CHAIRMAN POWERS: No, there's no oxygen source going off there. The oxygen source is exactly the same. The oxygen source is steam.

DR. MEYER: Not for the metal. Well, for the oxide. I mean, steam, an oxygen atom from the steam is going to stick right out here. It's not going to weasel its way into the metal surface.

Eventually.

CHAIRMAN POWERS: The transfer is by ionic diffusion, but.

DR. BILLONE: Yes.

CHAIRMAN POWERS: Okay. And if I thin that layer, it goes a lot faster.

DR. BILLONE: That's correct.

DR. MEYER: The reaction process. Yes. But not the diffusion into the metal. If anything that's going to slow down.

CHAIRMAN POWERS: I don't understand how it slows down.

DR. BILLONE: I don't know, but I need a break.

MEMBER SHACK: He's looking at the oxide itself as the source of oxygen.

DR. MEYER: Yes.

MEMBER SHACK: And then the transport from the corrosion.

DR. MEYER: Exactly.

MEMBER SHACK: I'm not sure I agree.

DR. MEYER: Well.

CHAIRMAN POWERS: But spallation screws up his time scale.

DR. MEYER: Let me just recite -- I hope

I can get this correct -- a statement that Brachet, Jean-Christophe Brachet made at our meeting last time. He pointed out how they put oxygen into the metal when they went to alloy. They went, you know, you put oxygen in the Zircaloy as one of the alloying things. And in the laboratory if you want to do this you oxidize the specimen on the surface, and then you put it in an inert atmosphere, and you heat it up. And the oxygen goes in.

CHAIRMAN POWERS: And okay. I'm at a total loss as to what that has to do with what spallation does. It seems to me what spallation does, at the very minimum it screws up your time scale.

DR. MEYER: In the case I just described, if you spalled off a piece of the oxygen that had been laid on there before it was put in this inert atmosphere and heated up, you wouldn't get as much oxygen diffusing into the metal, because you had taken away some of the oxygen source.

CHAIRMAN POWERS: Your inert atmosphere seems to have nothing to do with the problem at interest here.

DR. MEYER: Well, in a way it does. I realize this is not an inert atmosphere, and as soon as you take some away it's going to grow back mighty

quick.

CHAIRMAN POWERS: And that changes your time scale.

DR. MEYER: Doesn't change the time scale if you're using an equation. By the way, we always use this equation as if the material were bare. We don't put a corrosion thickness on this equation when we use it. It's being used as a time scale, not as an accurate predictor of how much oxygen has grown on the surface. We apply the equation as if the metal started out bare, and ---

CHAIRMAN POWERS: If it starts out bare you don't have any choice in that matter usually.

DR. MEYER: Well, you know, if it started out with 30 microns of corrosion I guess you could start the Cathcart-Pawel equation there.

CHAIRMAN POWERS: Yes. How much difference would it make?

DR. MEYER: I don't know.

CHAIRMAN POWERS: Epsilon.

DR. MEYER: Okay.

CHAIRMAN POWERS: But if you come along, and you're saying look, I'm looking at the point of oxidation -- maybe not. Maybe it doesn't screw up your time scale. I don't know. Are there any other

questions? Then we will take a break until quarter of. Thank you Dr. Meyer.

DR. MEYER: Thank you.

(Whereupon, the foregoing matter went off the record at 10:28 a.m. and went back on the record at 10:46 a.m.).

CHAIRMAN POWERS: Let's come back into session. If the members will look on Dr. Meyer's viewgraphs, I think it's Viewgraph 28 and 29, it is my understanding that these are the viewgraphs -- maybe it's 27, 28, and 29 -- that these are the viewgraphs that really contain what RES would like to get some sort of feedback from. And so I'll ask you to bear those in mind as we go through the rest of the discussions, and when we get to 7 o'clock, I will probably come around and ask the members if they have any comments pertinent to these three viewgraphs based on what they've heard up till then.

MEMBER SHACK: 27's interesting.

CHAIRMAN POWERS: It's 28 and 29? Definitely 28 and 29. 27 is blank? If they have any comments on that as well. I mean, that has to do with the proposed testing. Testing going on. And so if you have comments on that I think it would be useful too. Probably there will be at least one or two words

about thermal transience.

With that we will turn to the award-winning speaker, Dr. Billone.

DR. BILLONE: That's a tough act to follow, you know. I have a rather lengthy presentation. I have to apologize for it. And what I did was this morning I pulled out about 40 viewgraphs for my discussion session later. So you have a combined -- if you have the package, it's like 108 viewgraphs.

MEMBER SHACK: You didn't number them either.

DR. BILLONE: No, I numbered them in my presentation this morning. I apologize for that. But before we get into that, I thought it would be a good idea just to tell you what kind of tests we run, and just to tell you what we mean by "ductility". Basically, we run two types of tests on a 25 millimeter cladding sample. And this could be as received, pre-hydrided, or it could be high burn-up. We expose it to steam on both sides, the various oxidation levels, and then we cut -- from this piece we cut 8 millimeter rings. This is as fabricated. I'm using 15 x 15 because it's a little bit bigger and easier to see. So the advantage of this kind of test

is when you get to radiated material where the real estate is very precious, you can run a lot of tests with small segments like this, cut rings from them, and bend the rings.

Before we do that, let me just, since I want to give you an idea of ductility as we define it, and brittle as we define it, and then what is the difference between a clean brittle failure and fragmentation is the last part of my demonstration, because that's highly dependent on how you load the sample, what the loading rate is on the sample. So basically, if you take the as-fabricated material, and you put it in an Instron, and you squeeze 2 millimeters, it springs back about 1 millimeter, and you've got about 1 millimeter permanent displacement. And this is something you can determine from the load displacement curve through the offset strain, or you can simply measure the diameter before the test in the loading direction, diameter after the test, and you measure about a millimeter of permanent plastic strain. So, going from a circular shape to an oval shape before you crack is an indication of ductility in the ring compression tests. If I go all the way, there's no sense even looking at the load displacement curve. You can essentially squeeze the sample flat.

And it means as-fabricated cladding has very, very high ductility.

All right, let's oxidize it two-sided to 10 percent ECR. It doesn't matter whether you're at Cathcart-Pawel or actually measure at ECR. They're both about the same. And now when I squeeze that ring, I go from a continuous ring to a split ring, a very, very sharp load drop of about 40 - 50 percent. We stop the test. We take the sample out. We have to look at it under a microscope. You get a crack that is so thin and tight that you need a microscope to see it. That's what we're calling failure in our screening test, is when you go from a continuous ring to a through-wall crack along the whole length of the sample. So this, again, it's too small for you to see, it started out circular, it ends up about circular. You get essentially no plastic deformation.

CHAIRMAN POWERS: Excuse me, Mike. If you lay it down somewhere, the cameraman can show it to everybody.

DR. BILLONE: Oh, I'm sorry. Okay, let me contrast those two pieces. I'm sorry. Anyway, it's a very simple concept. It's a very simple test to run. And the ring compression test, as much as you may malign it, is used for brittle materials,

composite materials, because you don't need to cut or machine gate sections, and you don't need to grip. You just simply load it between two flat plates.

I showed you the results of doing a room temperature test after 10 percent oxidation of 1,200 degrees C. If you run the test at 100 degrees C, surprisingly, and it's hard to see, you do get a little ductility. You do get a little bit of ovality in the sample. And if you run the test at 135 degrees C, you might be able to see, you get considerable ovality. You've got very high ductility.

All right. So those are basically our ring compression tests. We run those, as I say, on a lot of materials. You can do them very fast. The two-sided oxidation's very reliable. It's very easy to determine weight gain by simply measuring the sample before and after. Now, there's a different kind of test that we run for what we call our LOCA integral specimens. And those specimens are about 300 millimeters long, 12 feet to those of us in the old units. And for the high burn-up stuff, they're essentially filled with cladding. They're welded end caps with a pressure tube coming down at the top. So, think of our LOCA integral test as pre-pressurizing the inside of this material, which is basically filled

with fuel, ramping and steam at 5 degrees C per second, and we'll show that, and holding at 1,200 degrees C for various amounts of time. Slow cooling, 3 degrees C per second, and then quenching at 800 degrees C.

So, what happens if I do that for non-irradiated cladding, because I can't really bring Robinson cladding in here and hold it in my hands. We take that material, and Ralph showed you actually what happens in the more extreme case, if you go all the way to 20 percent ECR in the burst region, he showed you the oxygen profile, and he showed you the hydrogen profile, and that sample is extremely brittle in the balloon region. It's a sample that two years ago I took in my hands, and I did a four-point bend test for one of our technical advisory groups, and I got a very clean break across the burst region. What I would like to show you today is, again, if you took the as-fabricated cladding, and you did a four-point bend test like this, it has extreme ductility, and it has a bending moment of over 2 kiloNewtons per meter. It's not the failure bending moment, but it's capable of not breaking at a very high bending moment, and it's capable of much more deflection than this, we just happened to stop the test at this point. So this

I think they can see without the camera. This is what we mean by ductility. It's a permanent change in shape of the material after the test is over.

All right. That's not very interesting. The question really I want to address is I already know that our samples that we let stay at 1,200 degrees C for five minutes, that's an ECR of about 20 percent Cathcart-Pawel, about 20 percent Baker-Just. They were very brittle at room temperature in the balloon region. So we finally got more sophisticated than my hands, which are not calibrated, so we developed the four-point bend apparatus, the fixtures, and it's now done in an Instron where the -- if you think of it, this is upside down compared to the Instron, my thumbs are moving at a prescribed displacement rate, and are recording the load. So you end up with a load versus displacement at where my thumbs are. Now, what happens for the LOCA integral samples, if I now cut back the time from five minutes to two minutes at the high temperature, because you're picking up all this oxidation on your ramp up, and some on your ramp down, the ECR only goes down from 20 percent to 14 percent. So what I'm going to show you is what happens when I do a four-point bend test on a 14 percent ECR sample in the balloon region. And what

happens is essentially what happened two years ago. You get a very, very clean break across the weakest part of the material, which is the burst region of the material. So this happened, if you look at the load displacement curve, this failed in the elastic regime, indicating at room temperature it's brittle. I would like to know very soon whether or not you get the same hydrogen peak. And it's a little expensive between Thursday and now to cut a bunch of little samples, and meticulously measure oxygen concentration and hydrogen concentration. So we'll save this sample, and we're just going to tap it with a hammer, because what I found a couple of years ago is if you start tapping with a hammer, you can pretty much map out the hydrogen profile. And it also illustrates that the idea of fragmentation, you can have a brittle material that behaved this way in a bending test. The brittle material would also behave that way if I pulled on it, the tension test. They're both axial type loading. If you whack it with a hammer, that's when the brittle material may fragment, or may not fragment.

So the idea of fragmentation, and also you all have the experience of taking a hot plate that's a little too hot and putting it under cold water, and what I've seen is pie-shaped cracks instantaneously

forming, with a loud burst of sound. So you can fragment things with thermal shock. And if you take the combination of thermal shock, and a little bit of impact, a little bit of pulling, it's possible to fragment if you have extreme and brittle material. But fragmentation is not a material property necessarily. It has to be in conjunction with loads. So to the extent you either know or don't know your LOCA loads you can deal with fragmentation. And that's why we're assuming you don't know all the loads in the degraded core, we're backing off from fragmentation to just simply brittle behavior, to trying to avoid brittle behavior. So let's hold this sample.

Now, there must be some time or test conditions in which you finish this test and have ductility at room temperature. So we went to the extreme of only holding at 1,200 degrees C for one second. The ECR is 8 percent because we're ramping in steam at 5 degrees C per second, picking up oxidation. We hold for one second, slow cool to 800, and then quench. And it's kind of interesting when you subject it to the four-point bend test, because the sample's still intact. In the elastic regime, you get a very sharp load drop of about 80 percent, which is the

burst region failing, and part of the circumference failing. After that, you get a very low load drop, because the back of this burst region -- and remember this is thin wall. It gets thicker as you go 180 degrees. The back of that region is relatively ductile. And because I want to do the same hammer test on this, we'll complete the test. This was the limitation of our Instron machine. We couldn't bend it any farther. And you don't really have to. You would call this sample -- as a structure, you would call this sample ductile. So if I complete the process, which wasn't too hard, I now have another sample which was oxidized much less, may have a lot less hydrogen in it, and that will lead me to my next step.

But before I whack these samples, and by the way, these two I'm bringing home for analysis so I'm going to put these away so I don't get a little bit happy with the hammer. Does anyone have any question on the two basic tests we run? We run a four-point bend test on the LOCA integral sample because it's highly non-uniform, and oxygen concentration, the axial direction, a diameter burst or non-burst, and in hydrogen. And what the four-point bend test does is it gives a uniform bending

moment between my thumbs. So you're not biasing any region of non-uniformity. And it fails where it's basically the weakest. All right. The idea here --

MR. ELTAWILA: Mr. Chairman, can I ask a question please?

DR. BILLONE: Yes.

CHAIRMAN POWERS: Sure.

MR. ELTAWILA: Why you're oxidizing the inside of the tube?

DR. BILLONE: Because from experimental point of view, we're talking about -- I'm sorry. Are you talking about these small ones?

MR. ELTAWILA: I think, yes. You were talking about the large for all your tests that you have to oxidize the tube from the inside and the outside, pre-oxidizing the tube.

DR. BILLONE: Well, the answer --

MR. ELTAWILA: So why the inside?

DR. BILLONE: Because based on worldwide experience, some of which was shared to us as late as May 11, there's all kinds of problems in doing the one-sided oxidation test, particularly of short samples on the radiated samples. You get very reliable experimental results, and you can determine weight gain very reliably by doing the two-sided test.

You have good temperature control, and you have good control of the oxidation. If there's hydrogen in the sample, you don't risk losing the hydrogen to the inner surface of the material.

MR. ELTAWILA: Would that affect the mechanical properties of the cladding?

DR. BILLONE: No, because as I'll show in my presentation, CEA for non-irradiated materials, CEA meaning CEA, EDF, Framatome. I'm just going to simplify it and call it CEA. They do one-sided oxidation tests on the same materials that we're using. We do two-sided. And our test results as far as ring compression agree extremely well for the material that's not pre-hydrided. So it doesn't seem to make a difference from that point of view. So we do it because we get much more reliable results, we have much better temperature control. The two-sided oxidation naturally occurs with the ballooning and bursts.

MR. ELTAWILA: But only in the balloon region.

DR. BILLONE: Only in the balloon region. Having just lost track of my samples -- well, you know what, this is more interesting. Because it's more interesting not to know which sample is which. All

right. What I'd like to do is, again, my experience is light tapping in the region that's highly embrittled and thin. Highly embrittled with hydrogen and oxygen tends to cause fragmentation. As I move out of that regime, and I won't do it I hope. You can pound as hard as you want, the material's so tough that you can't even get a crack to grow in the material. Hopefully I will not do that.

So let's take one of these samples. And let me take the other sample. Okay. I now know which sample is which. This is the sample that was held for one second. You essentially had very little hydrogen embrittlement outside the balloon region. This is the sample that was held for two minutes. And if I pounded a little bit harder, what would happen is I would continue to fragment. So the issue of fragmentation, I know the material is brittle in that regime, I know it would snap cleanly in a bend test, it would snap cleanly and not disperse fuel in an axial test. But there are other loads that could cause a brittle material to fragment, and that's one thing we're backing away from.

That's all I really intended to do, is to say that, as Ralph showed, you've got oxygen and hydrogen embrittlement, and there's various ways you

could test for that. The simplest I can demonstrate to you is just tap with a hammer, increase the tapping of the hammer, and eventually as I get out here, outside the balloon and burst region -- I've done it in my office when OSHA has not been around -- you can bang it you know from up here as hard as you can, and the material is really, really tough. So that ends the demonstration part. And I hope I've demonstrated everything I'm about to present to you in this. So thank you for that.

We're going to switch off of the camera. Okay, thank you. This has the 108 slides numbered, but I'm going to try to skip to the ones I'm going to use for discussion. It takes a little while to load because it's huge. All right, what one step do I have to do to get it on the screen? Oh, you did it for me. Thank you. Before I start, I'd like to acknowledge Dr. Yong Yan, the principal investigator for this work, Tanya Burtseva who does the very detailed oxygen and hydrogen analysis, along with the ring compression tests. Hee Chung is our senior technical advisor. He's now retired, but as Ralph knows, he's very accessible for last minute questions before big meetings. Okay.

All right. Let me try to explain the

purpose of our work, although from Ralph's presentation it should be clear, but to generate steam oxidation kinetics and post-quench ductility data for cladding. And as I mentioned, we deal with as-fabricated cladding alloys. There are the four that are in our program. Pre-hydrided cladding alloys, and so far we've just pre-hydrided Zirc-4. You'll see some excellent data, or read excellent data done by CEA-EDF-Framatome, and please just let me say CEA, that the lab has done. M5 and Zirc-4. The Argonne work, we've concentrated only on pre-hydrided Zirc-4 because we have high burnup Zirc-2 and Zirc-4 currently at the hot cells. We also have -- these are fuel segments, fuel rods cut into segments. We also have 8-centimeter long tubes of defueled high burnup ZIRLO in M5 for our testing program. That's what we currently have in the hot cell.

In addition to just generating data, we're trying to develop some mechanistic understanding of cladding behavior, with the emphasis on effects of high burnup and effects of fuel on the cladding at high burnup during LOCA events. We kind of added additional responsibility, and that is to develop a rate correlation for ductile or brittle transitions, something we'd integrate over a time/temperature LOCA

trace as a function of hydrogen content and the particular time history that you have in steam. And I'll have a presentation on that probably after lunch.

The other advantage -- the nice thing about this program. It doesn't make our work any better than anybody else's work, but because it's sponsored by NRC we try to document and distribute all of our data and correlations to NRC, industry, and our technical advisory group for their independent assessment. We don't wait until we've, you know, crossed the last "t" and dotted the last "i". We actually issue the data pretty close to when we generate it. It may be in the form of a PowerPoint presentation, letter reports, eventually journal articles and NUREG reports. And for us, this has worked very well because we get some very good feedback from technical experts all over the world. So hopefully that ends up being a better final product.

Okay. A little more specific about the scope of the work in terms of steam oxidation kinetics meaning weight gain versus time, if you will. And post-quench ductility, as I showed, we concentrate on 1000, 1100, 1200 degrees C oxidation of those short segments. And these are all samples quenched at 800

degrees C. We've done ring compression tests to determine post-quench ductility at room temperature, 100 degrees C, and 135 degrees C. We're pretty much now focusing on the 135 because as soon as you add hydrogen to the material, jack the temperature up to 1200, you embrittle quite quickly at room temperature. And if you're going to look for any ductility, you're going to be looking for it at that temperature.

Those are all essentially ring tests. In addition, as I mentioned, samples approximately this long with high burnup fuel, or with zirconium pellets if you're doing out of reactor work. Those are our LOCA integral tests. And those are followed by four-point bend tests, and a lot of characterization to determine what the effects of the fuel are in the cladding.

Our advanced alloys, we're using 17 x 17 Zirc-4 as a basis modern belt polish, 17 x 17 Zirc-4, and comparing that, the behavior of ZIRLO and M5 to the 17 x 17 Zirc-4. We also have 10 x 10 Zirc-2, which is in our program and will be tested in the fall. That came a little later than the other alloys.

E110 is really added to the program because you really want an alloy that behaves badly to test your test techniques. I mean, is post-quench

ductility a good screening test? Well, if what you saw in the E110 pictures, if that stuff comes out ductile there's something wrong with your test. So it's in there for really two reasons, understand a little bit about how bad alloys perform with these tests, and try to gain some insights into maybe why it behaves so badly at the same time. That's kind of a bonus.

I already mentioned that we looked at pre-hydrided 17 x 17 Zirc-4 rings, high burnup defueled, ZIRLO and M5, are at ANL. We'll be doing the same kind of ring double-sided steam oxidation tests on those. LOCA integral tests, eventually we'll get some fueled M5 North Anna to INL, eventually to IANL. How we're going to get fueled ZIRLO is yet to be determined for our program.

The high burnup Zircaloy program. We have Limerick BWR cladding, fuel cladding, and we've conducted four tests with that. We're now switching our focus to the Robinson PWR Zirc-4 cladding. We've completed some tests with short rings, and we're trying to move on to the LOCA integral tests with the materials. So, for Zirc-4 15 x 15, we had a lot of baseline material. Framatome was generous with the material it gave us. So we did a lot more tests than

even our advanced alloy tests in terms of as-fabricated material duplicating tests, seeing we got the same results, pre-hydrided from 5 to 800 weight parts per million, and of course the high burnup hydrogen levels are at 400 - 800 ppm.

So, tests that have been completed are tests that I'll show you data. This is another shot of the alloys that we have. And this is really just put in there for your records. But we have cladding thicknesses from 0.57 millimeters to 0.7 -- well, throw this one out. This is really tubing. And one thing we learned in this is you don't want to test tubing. You want to test whatever the final steps are, it could be etching, it could be belt polishing. You'd like to test the cladding in its final form before fuel is put in it. And so throw out that one. So let's say 0.57 to 0.76 wall thicknesses, and diameters ranging from about 9.2 if you count E110 millimeters to about 11.2. So we have a range of alloys in terms of composition, a range of geometry, and a range of surface finishes. And the last number on the right is the surface roughness ranging from about 0.1 micron mean square roughness for belt polish material up to about 0.3 to 0.35 for material that hasn't been belt polished.

Okay. You should be familiar from previous presentations, we're just going to concentrate on these two high burnup fuel types. The Robinson Zirc-4, and with up to 800 ppm of hydrogen, and up to 100 micron oxide layer. Excuse me, it's called a corrosion layer for this talk. That's the in-reactor stuff. And the Limerick, and please notice in the column on the far right that the Limerick after 57 gigawatt days per metric ton only has about a 10 micron oxide layer, and about 70 ppm of hydrogen. It makes these -- it's not that this is Zirc-2 and this is Zirc-4, this is what makes these alloys extremely different, the hydrogen content. And you would expect different test results as a result of it.

Okay, this is one I pulled out for my discussion. Well, you know, this is too much fun. I'm not going to do this. I have to go through this. Ralph talked about embrittlement, and Ralph actually showed you, and I have to contradict you on this, Ralph. Sorry. He showed you E110 breaking up after about 300 seconds, and the surface looking rather wretched. It appears that we just caught the beginning of this process. And if you take this grayish white oxide, which is, we call it "bad oxide". It's the monoclinic oxide that tends to crack and

break up. And next to it is the black oxide, it doesn't show up that well in this contrast, which tends to be an adherent layer that maintains its integrity, and is a barrier for -- a traditional barrier for oxidation.

Anyway, if you look under this region, this is the oxide layer, and it has delaminated from the metal layer. Question is, is that ductile or is that brittle? And I'll show you in a second. Now, if you go to the extreme case, 1,400 seconds, Ralph showed that picture. That's got 4,000 ppm of hydrogen. That's clearly brittle. This only has about 120 ppm, which means we caught this early in the process. So the answer to the question is it ductile or is it brittle, that E110 that looks so terrible because we caught it at the beginning of the process happens to have about 60 percent ductility, meaning if I look at load and displacement, I can essentially squeeze that E110 flat, even though the surface looks like a nightmare. That's about 300 seconds. If I go another additional 300 seconds at 1000 degrees C and cool, it's extremely brittle, and it's picked up about 300 ppm of hydrogen. So this is the E110 ring compressed, and again, this green line is the offset strain. You get essentially offset strain within the

noise at about 7 percent ECR. So about 300 seconds after the picture Ralph showed you, the alloy -- "goes to hell" is not a proper thing to say at a meeting. The alloy behaves badly.

All right. Ralph showed you this picture. Is it brittle or ductile? He gave you the answer, essentially. And that's the load displacement curve. This is the offset strain, which is about 5 - 6 percent. Five percent for me, 6 percent is the number I gave Ralph.

Okay. This picture looks really nice, so I'm obviously setting you up. This is 17 x 17 Zirc-4, 13 percent ECR at 1200 degrees C. Is it brittle or ductile at room temperature? Is it brittle or ductile at 135 degrees C? And the answer is that 1200 degrees C is a very embrittling temperature, and at room temperature this material is brittle, and at 135 degrees C it happens to be ductile. Hopefully I've got the results. This is the measure at ECR in terms of weight gain is shown on here. But this is the load deflection curve. At room temperature you get essentially displacement, permanent displacement that's within the noise. And this is the load deflection curve. You get about 10 times the displacement when you test at 135 degrees C.

Okay. This is an even nicer picture, which I have the punchline just cued up in front of you. What happens if you add 600 ppm of hydrogen, drop the ECR or the oxidation down to 7.5 percent of the wall thickness, and you oxidize at 1,204 degrees C? This is your classic oxide layer, your classic oxygen stabilized alpha layer. This is your prior beta layer. It is loaded with oxygen, even though your oxidation time is not that long. It's loaded with oxygen because you've jacked up, at this interface you've jacked up the boundary condition from about 0.6 percent to about 1 percent oxygen. You've increased the steepness of the initial gradient of oxygen across the material, and your pumping oxygen in rather fast. So, the question at room temperature isn't worth asking. It's extremely brittle at room temperature, so brittle I didn't even ask Tanya to draw this up in the fancy way. And if you test that -- sorry. This test was performed at 135 degrees, our highest test temperature. Material is extremely brittle at 135 degrees, with 600 ppm of hydrogen, and that level of ECR. Okay.

CHAIRMAN POWERS: In this particular sample, you loaded it with hydrogen artificially?

DR. BILLONE: Pre-hydrided in hydrogen, 4

percent hydrogen plus argon gas.

CHAIRMAN POWERS: And you'd anticipate that real cladding would acquire its hydrogen during normal operation.

DR. BILLONE: Right.

CHAIRMAN POWERS: So when Ralph sets up his criteria, his proposed criteria, and says look for embrittlement based on un-irradiated cladding, you're taking issue with that?

DR. BILLONE: Well, the un-irradiated cladding has to be both fresh, as-fabricated, because that's what you're putting in the reactor. But in looking at the burnup effect, and I thought he had that in there, you have to pre-hydride the un-irradiated cladding at a level that you expect based on your corrosion layer. And it may not have been said explicitly enough. He may have said corrosion layer, because the two are correlated. But certainly if it's M5 and you expect the maximum of 200 ppm of hydrogen at your highest burnup, you would want to know how the non-irradiated material, M5, behaves up to that hydrogen level. So yes, it's essential that the non-irradiated test, the inexpensive tests with hydrogen be done. But they need to be done within a realistic range of expectations.

CHAIRMAN POWERS: Well, I mean, nothing in this criteria seems to mention that pre-hydriding it.

DR. BILLONE: I'll have to sit down with Ralph and look at it. I thought he had something in there about corrosion layers.

CHAIRMAN POWERS: He said the oxide, the corrosion oxide should be determined and expressed as a percentage of cladding thickness for fuel rods and burnups of interest. I mean, okay, list it down.

DR. BILLONE: All right, then Step 2 from that, once you've done that, you have a correlation that tells you how much hydrogen -- well, maybe I need to talk to Ralph. Maybe we need to talk. There's one more step after what he said, and that is if M5, you expect 20 microns of corrosion at high burnup, and 120 weight parts per million of hydrogen, that go hand in hand, then you need to know how your non-irradiated material's going to behave up to that hydrogen level.

DR. MEYER: This is Ralph. I did not have a test proposed for pre-hydrided material.

DR. BILLONE: Oh, I know why. Because you had the corrosion.

DR. MEYER: Because we used the corrosion sickness as a surrogate for that. But he can explore the effects of hydrogen with those kind of tests. So

he's doing more tests than we have skimmed off for the proposed criteria.

DR. BILLONE: Yes. Let me back off. I'm thinking in a laboratory timeframe, looking for mechanisms, and definitely you want to do this test. In terms of what regulation requires, and what their final criteria, they do criteria, I do tests and correlations. I've got to keep remembering this. It's not a disconnect, it's just the flow of information from us to RES, and then their reinterpreting the information in licensing language. For us it's essential to do these tests because you can do a lot of them very cheaply, and before you go into a reactor with a Robinson sample containing -- a high burnup sample containing 600 ppm of hydrogen, you darn well better have these results because you don't know what test time to run at if you're trying to bracket the ductile to brittle transition. So these tests for us are critical in running our high burnup tests. But I'm sorry, Dana, I keep interpreting you. Did you want to finish?

CHAIRMAN POWERS: Well, just, I mean, maybe you've succeeded in really confusing me.

DR. BILLONE: I talk too much.

CHAIRMAN POWERS: You seem to -- I mean,

this seems to say that there is a synergism between hydrogen acquired during operation --

DR. BILLONE: Right.

CHAIRMAN POWERS: -- and the subsequent embrittlement by oxidation.

DR. BILLONE: Which was in the words in Ralph's slides, yes.

CHAIRMAN POWERS: But I don't see in the criteria that he's laid out here any recognition of that, save that he wants you to measure the thickness of the corrosion layer during normal operation, which presumably comes from a lead test assembly.

DR. BILLONE: And Ralph's answer to that was he's trying out the idea of if you subtract that from the allowable ECR, he thinks, he hopes that you're essentially accounting for the effects of hydrogen.

CHAIRMAN POWERS: But if I look on your previous slide, was that the case?

DR. BILLONE: You know, if I could remember my previous slide.

CHAIRMAN POWERS: Well, you showed a list of various sources of Zircaloy, you showed a list of oxide thicknesses, and you showed a list of hydrogen thicknesses. And at least operating from memory, I

did not see a linear correlation between the two.

DR. BILLONE: Well, that's because -- of course, in a boiling water reactor, once you get to the mixed regime, about 20 inches above, then your temperature is flat at about 288 degrees C, and you don't get much axial variation due to the coolant temperature. So your oxide is like 10 microns, and your hydrogen level's about 70, for our particular material. In the PWR, of course, you have this temperature gradient coolant, which has a huge effect. So you have -- I just listed maximum values of oxide and --

CHAIRMAN POWERS: But what I'm not seeing is how just subtracting off the corrosion layer compensates --

DR. BILLONE: Oh, but you've got to take that up with Ralph. Really.

CHAIRMAN POWERS: Well, I get to do that kind of simultaneously --

DR. BILLONE: That's what he did. He kept pointing to me when you asked.

CHAIRMAN POWERS: And that's fair. You can point to him and explain how his criterion accounts for this.

DR. BILLONE: From a mechanistic point of

view, I am not interested in that. I'm interested in providing the data, and Ralph is free to use the data to check an information letter from 1998 to see if it happens to hold. If it doesn't, then he's got to move off that position. So Ralph's using something from 1998, am I correct? '98? Information letter, and testing it against the data that I'll be showing.

CHAIRMAN POWERS: I'm still --

DR. BILLONE: This material had no corrosion layer to begin with. So obviously Ralph's scheme would not work for bare cladding.

CHAIRMAN POWERS: I understand that.

DR. BILLONE: You don't have bare cladding in a reactor.

CHAIRMAN POWERS: What you're showing us is the results of a test somewhat different than what's specified in his candidate criteria.

DR. BILLONE: All these tests were done long before he came up with those specifications, yes.

CHAIRMAN POWERS: Okay. And it's showing an interesting physical phenomena that's pertinent. And so all I'm asking is how did the criteria account for this interesting physical phenomenon that looks pertinent.

DR. BILLONE: Ralph?

DR. MEYER: Let me comment here that there is a direct relation between the corrosion thickness and the hydrogen absorption. It's one that's been measured before. It's in our codes. And although we don't have it explicitly factored in, because we're not generating a mechanistic model here, it is -- I think it is linear, and it's just a factor. It's something. And so we're taking advantage of that. If I could go ahead and jump to a good variety of the question, a good question would be is the fraction of absorbed hydrogen the same for the different alloys. And we had essentially assumed that that would be the case at the outset when we decided that we would test the burnup effect in Zircaloy, and assume that the burnup effect played out the same in all of them. That may not be correct. We have the test that will show if there's some gross discrepancy. The initial test with the M5 and the ZIRLO using the Stusvick material should reveal that. But for the moment I think it's still a reasonable assumption to say that using the dull pencil that we're using here for regulation, that it is a good chance that this is going to work out.

CHAIRMAN POWERS: I must be very, very slow. You're perfectly willing to specify the

oxidation model to be used, but you will hide and obscure the hydriding model you want to use. I mean, you have some knowledge of how this corrosion layer relates to the amount of hydrogen in the alloy.

DR. MEYER: Yes. About 15 percent of the freed hydrogen is absorbed into the metal cladding. And that's a number we use in our computer codes for relating hydrogen to the corrosion process. We simply don't introduce that number explicitly into this process. It's more empirical. We simply test the specimens with a given corrosion level, and check to see if they're brittle or ductile, and look and see if the method works. It appears to work.

CHAIRMAN POWERS: But I guess what I'm asking -- every answer seems to get me a little more confused. Here we see something, an alloy that is taken to 7.5 percent effective oxidation here. It's got a bunch of hydrogen in it. Okay, and it's brittle at room temperature. It's not brittle at 135 --

DR. BILLONE: I'm sorry. It's brittle at both.

CHAIRMAN POWERS: It's brittle at both?

DR. BILLONE: I made a slight --

CHAIRMAN POWERS: Okay, it's brittle at both.

DR. BILLONE: It's gone.

CHAIRMAN POWERS: Okay. This is a long ways away from 17 percent. It had zero corrosion thickness, but I suppose that 600 weight parts per million hydrogen corresponds to a pretty healthy --

DR. BILLONE: About 70 to 75 microns of oxide layer, which Ralph converted to ECR.

DR. MEYER: This is pre-charged with hydrogen. This is not a --

CHAIRMAN POWERS: I understand that.

DR. MEYER: Okay.

MR. SCOTT: Dana, this is Harold Scott. Six hundred ppms hydrogen is probably 70 - 80 microns.

DR. BILLONE: That's right.

MR. SCOTT: So 7 and a half -- 70 - 80 microns is 7 and a half plus 7 and a half's 15. We're pretty close to 17.

CHAIRMAN POWERS: Okay. Well, you can do these calculations in your head faster than I can. Why don't you just show that? I mean, why not say look, my corrosion layer will account for this phenomenon, and then I don't have to worry about it.

DR. BILLONE: Well, it was too small on Ralph's graph. He has red data points, and he has blue points that do that on his graph for high burnup

material. And that was material with 550 +/- 100 weight parts per million hydrogen naturally from in-reactor, and about 70 micron corrosion layer. He needs to enlarge that graph, I think, and make it jump out at you.

DR. MEYER: I botched up the presentation of that graph, too, because I mean it was plotted sort of backwards of the way we generally approach it. They took the experimental data and added the corrosion thickness and compared that to the number. And I didn't explain it well. So I think it's there.

MEMBER SHACK: Well, the other question, shouldn't all your experimental results be expressed somewhere in terms of those criteria? Instead of showing it as 600 weight ppm, show that as you projected equivalent ECR, and demonstrate whether or not it's consistent with the criteria.

DR. MEYER: Well, certainly in my presentation I think I did, or at least tried to express all of the data in terms of the criteria that we were trying to outline. I don't see that constraint applying to Mike's work, where he's going to --

MEMBER SHACK: Because he's got a much larger database. You'd like to know that it's

consistent with the proposed criteria.

DR. MEYER: Well, but I used his data. I mean, everything I used came from his program. So.

DR. BILLONE: But really, what I'm going to try to lead towards is presenting data, and this is not -- I'm not doing licensing. And I'm not being the interface with NRR. I'm trying to understand phenomenologically what's causing the embrittlement, how to calculate it, and ….

CHAIRMAN POWERS: I think we understand that. I'm just trying to understand how it gets taken into account in the criteria.

DR. BILLONE: Okay.

CHAIRMAN POWERS: And I think I understand now. It looks correct. I just don't have this correlation between hydrogen absorbed versus corrosion thickness.

DR. BILLONE: It's actually -- at low burnup it's a higher number, and it flattens out at about 15 percent. But I'm not sure it's true of all alloys.

Discussion, discussion, okay. Ring compression I talked about. The way we run our tests is we use 8 millimeter samples. We have a bottom support plate that's fixed, a top support plate that's

moved at a prescribed displacement rate, and the load cell measures the force in response to it.

Standardly we run our tests at room temperature. We get all the way to 20 percent calculated ECR with ductility. We call it quits. If we have any ambiguity, or if we drop to essentially no ductility at 8 to 10 percent ECR we will run the tests at the higher temperatures, which is essentially what's said right here. And at the time we had set, hit brittle at 17 percent ECR at room temperature, we would retest at the higher temperatures.

CHAIRMAN POWERS: Do I take it from that note on the graph that when it says it was tested at 135, that's any temperature between 100 and 135?

DR. BILLONE: Yes. When I say 135, I mean 135. There are other tests that haven't been shown that I'll show where we did the tests at 100. One of those samples that I showed you.

CHAIRMAN POWERS: It's just that the data presentation has not been replete with test error bars, so I was just --

DR. BILLONE: Right. Okay. All right. Accepted. Let me point out that some organizations, some labs, some tests are conducted not by us with a curved or grooved support, and a grooved loading

plate. And that's a more benign loading than the flat plate loading. The important thing about a screening test is whatever you start doing at the beginning you keep doing at the end. The only thing you want to change is the material. Alloy to alloy, degradation due to hydrogen, degradation due to oxygen, keep the tests the same. Otherwise you lose the advantage of the screening test.

For the double-sided oxidation tests, for two reasons you'd expect that you get your maximum tensile bending stress at the ID right here, the inner surface right there. You would expect your crack to start here, and progress to the OD. So under bending loads you're getting hoop bending stresses that are tensile here, compressive there. You go 90 degrees either way and it reverses. Your maximum tensile stresses are out there.

But anyway, I want to get on to what we call offset strain, and why we have an error associated with offset strain. This may be too small for you to read on the screen. I have to apologize for that. But this is a material we know and can predict how it behaves. To make sure I'm going to pick this piece. This is 15 x 15 Zirc-4. It's not oxidized. We're just doing benchmark tests. And we

essentially -- we would displace it a total of 2 millimeters. And you get the elastic response, which exactly -- the slope of this measured curve exactly matches the calculated stiffness, or spring constant of the ring. So we're not getting any interference with the machine.

And then you get into the elastic/plastic regime. You stop the test, and you unload. The reason we want to do that is because in a real test, you're not going to have this luxury. You're going to come to some point in displacement and you don't know where it is. You're going to crack, and you get a load drop. You don't have the luxury of unloading. So this test, experimentally we went up to this value. Traditionally you take this point of maximum load, and you come down at the dotted green curve, which is the same slope that you loaded with. That's what you do in tensile tests. That assumes you haven't changed the material, shape, or anything else, and it's going to spring back with the same spring constant that it loaded as. That's how you interpret all mechanical properties tests. That gives you your error, because in fact it's a little springier, and this blue line is how it actually unloads. And the traditional offset displacement is 1.32 millimeters using this slope to

mathematically unload the curve, which is what you do. The actual permanent displacement determined by this curve is about 1.19. This is also exactly what we measure if we measure the new and old diameter.

So there is inherent in this procedure, by having to use -- this is the only slope you know from the real test that you're going to run. You unloaded this slope, and for all the alloys we've tested under these conditions, every single one of them, E110, M5, the maximum difference between these two numbers is less than 0.2 millimeters, which corresponds to 2 percent change in displacement, or 2 percent strain. So what we're saying is anything below 2 percent strain is either brittle, because it's in our noise where we're not sure about the unloading slope, or we really need to measure after the test what the change in diameter is. So this is what I mean by offset strain. It's the linearly unloading that curve at maximum load at this particular rate, the same rate that you loaded at. This is what I mean by permanent strain. This is something we actually measure, diameter before and after the test. You can only do that if you interrupt the test after the first through-wall crack, and the crack is very tight.

Dana, does that clarify what we mean by

offset strain? We're calling that ductility. And for this, it obviously is. I mean, it's the highly ductile sample. So this is not failure, it's really just a benchmark. Okay. On discussion. Now I have to do it. This is really part of -- I'll get back to this. You have to be very careful comparing data sets from different organizations, because people's definition of "displacement" is different during compression tests. People's interpretations of curves are different. This interpretation is from over two years ago. I wasn't as smart as I am now, and in fact, well let's look at this. This load drop is not enough to signify through-wall failure. This load drop is enough of signify through-wall failure. You're looking for something like 30 to 50 percent load drop in the through-wall failure. But, let's ignore that point.

The point is some people use offset displacement, which would be this 0.38 millimeters. It would correspond to about 4 percent strain. The real point should be out here, which would correspond with about 6 percent strain. Some people use total elongation of the first crack, which would be about 10 percent strain. In other words, they don't subtract out the elastic part. And some people use total

displacement until you've completely destroyed the sample. And that would be an answer of 20 percent. So you could take two identical materials oxidized under identical conditions, given the same load displacement curve, and you could get four different answers, depending on how people define things. We think that for a ductility screening test, this is -- well, not this one actually. This is a tough curve to analyze, but this one at 6 percent is the right answer.

Okay. ANL results. I'm going to show you results …. This is like a drama. This is like, you know, the rise in ductility, the fall in ductility, the rise in ductility, the fall in ductility as we do different things. Basically, we started our testing at 1100 degrees C, not realizing that was the most benign temperature and the least interesting, but it's one that everyone tested at, and that's why we know why everyone tested at 1100. The alloys behave very well at 1100 degrees C. So we went from 1100 to 1000. And as I mentioned, our samples are all slow cooled to 800 degrees C and quenched from these temperatures. Oxidation temperatures. And we got good room temperature ductility to greater than 17 percent calculated ECR, which in some cases is the same as

measured ECR, in some cases it's very different. And I'll mention why.

The 1200 degrees C samples, again, the same procedure. We got severe room temperature embrittlement room temperature. The test was conducted at room temperature. And about 9 percent for Zirc-4, and about 9 percent for ZIRLO and M5. I mean, far below the 17 that we had set our sights on. We got significant ductility improvement by retesting at 135 degrees C. Embrittlements greater than 17 percent ECR. By "embrittlement" I mean the ductile to brittle transition ECR, where you go from ductile behavior to brittle behavior.

So that's the rise of ductility. And then you knock it out when you add hydrogen. This helps a lot, and adding hydrogen severely embrittles material. So, for 300 to 600 ppm of hydrogen, which is the range we tested, your transition ECR will be less than 10 percent, even if you test at 135 degrees C. You're just going to knock out. And again, this is oxidizing at 1200 degrees C, 300 - 600 ppm of hydrogen. You're going to lose a lot of that improvement that you got in this step.

And okay.

CHAIRMAN POWERS: So I take it 300 ppm

hydrogen corresponds to a layer some 35 microns thick of corrosion oxide.

DR. BILLONE: Yes, 35 to 40. Yes.

CHAIRMAN POWERS: And that corresponds to something more than 7 percent ECR?

DR. BILLONE: No. Three and a half or something like that. Harold does it in his head better than me. We haven't tested this for the intermediate -- we haven't tested intermediate.

CHAIRMAN POWERS: So I'm still struggling. Now, your criterion is like 17 percent ECR.

DR. BILLONE: Uh oh.

CHAIRMAN POWERS: Subtract three --

DR. BILLONE: How is this going to damage --

CHAIRMAN POWERS: I get something on the order of, what, 13 and a half percent oxidation that I can tolerate during the LOCA. Yet, add 10 percent it gets embrittlement. Severe embrittlement.

DR. MEYER: I did the number -- I can't remember the numbers. I did the numbers, and the procedure works for the pre-hydrided test that he's done, as well as for the high burnup test that he's done.

DR. BILLONE: Oh, the test that -- oh.

Never mind.

CHAIRMAN POWERS: Okay. So if I went through all the numbers everything would work?

DR. MEYER: Yes.

DR. BILLONE: I haven't done that exercise, I'm sorry.

CHAIRMAN POWERS: Neither have I, so we're in the same place.

DR. BILLONE: All right. To finish this part of the advanced alloy program, which again is two-sided oxidation of the small samples, we're in the process of setting up to oxidize ZIRLO first, then M5. We're going to start at 1200 degrees C, jump to 1000 degrees C for reasons that you'll see in a minute. That's where alloy differences become rather significant. Then finish it off at 1100.

For our high burnup BWR Zirc-2, we see significant embrittlement in the balloon region associated with significant secondary hydriding and oxidation. That's all been room temperature testing, the four-point bend test. Because in the balloon region you have non-uniform wall thinning, it's thinnest near the burst opening, gets a little thicker as you go around the back, you have two-sided oxidation, you have high secondary hydrogen pickup

towards the ends of those regions. We did manage to produce for you one sample that if you only allowed it to be at 1,204 degrees C for one second it seemed to do pretty well, has high ductility. The high burnup PWR Zirc-4. The LOCA integral test, we're going to base that on the baseline ductility data we've already generated for as-fabricated and pre-hydrided, 15 x 15 Zirc-4, and the high burnup Zirc-4 rings that I will show you that were oxidized to about 1200 degrees C.

This is too wordy. Okay. Apparatus, and I'll just say that we have a particular test train. I know this is the top because steam flows out through here essentially. That's the bottom, so you've got to rotate it 90 degrees clockwise to get a feeling for it. It's a significantly long test train, and the sample's only 25 millimeters in the test train. It's all Inconel. What's going to be important is the next slide, is that when we run oxidation tests, we don't want to weld thermocouples directly to the sample. You only weld them during your benchmark testing.

This is an essential thing to understand about our program. This is a blowup of the sample region. And what we have above the sample on the Inconel is thermocouples welded at 320 degrees C, around it. That stays with the test train. And what

changes from test to test is there's an Inconel stem that's threaded down the middle, where you can assemble and disassemble this unit.

So what's a thermal benchmark? A thermal benchmark is you start out with bare cladding, or pre-oxidized cladding, and -- well, bare cladding, and you weld two thermocouples 120 degrees apart, because we only have channels for five readings. And then you put in the new sample with two other thermocouples welded 120 and 240. And that's your benchmark. And what you're benchmarking really is the temperature ramp of this, which is much faster than the temperature ramp of the much heavier Inconel. They come pretty close to a steady state temperature, but the temperature ramps. So you do your thermal benchmark at the beginning, and then you keep running tests. After each test, you do a weight gain measurement. If that weight gain starts to get off, you know your test train is shot. Or it's on its way. Because with quench, this test train can warp over time. The thermocouples can pop off, which is pretty easy. So it's -- in giving you a data set, my experience in the literature, and at conferences, if someone just shows you a graph of post-quench ductility versus any parameter square root of time

ECR, and doesn't tell you how they prepared their sample, controlled their temperatures, monitored their temperatures, the data set isn't meaningful to me personally.

Okay, this is an example. Let's start with 1000 degrees C. This is an example of our two-sided oxidation tests in which I'm showing you the three thermocouples that are permanently fixed to the Inconel. And it's got the slower temperature rise. It doesn't matter for a long-time test like this. And these are the -- I'm sorry. This one, I'm off one. This is on the sample, that's on the sample. Maybe I'm right. These two are on the sample. And so we do this at the beginning, and we assume this is our temperature history for ever after, every test we run until the weight gain starts to get too high or too low. And then we know we're off in temperature.

All right. 1000 degrees, interesting because, you all know this already. If we look at the offset strain versus measured ECR, measured ECR is just simply the measured weight gain divided by the wall thickness times the constant conversion factor. And we all know M5 has a very low weight gain, because that oxide layer grows very slowly on M5 at 1000 degrees C. The point is that you don't get a very

good correlation. This is Zirc-4 at room temperature, and you get the expected behavior as you increase the oxide. With M5, what you don't appreciate is there's a huge difference in test time between these points. As a matter of fact, it's even crazier than that. This is 3,400 seconds, with a higher weight gain than the 4,100 seconds. So let's look at M5 in a different way. Let's not look at M5 in terms of how fast the oxide layer grows, and therefore how the weight gain increases. It's not relevant. The question is how fast does it embrittle, does oxygen get inside, and that correlates much better with the square root of time for an isothermal test, which essentially the Cathcart-Pawel prediction, that is the time dependence. It's a diffusion model, so it's the square root of time dependence.

So if we re-plot M5, we see that it comes down and embrittles at the higher test times. And let's see if I can get this straight. The M5 is a little bit thicker.

MEMBER KRESS: Is it supposed to have a square root sign?

DR. BILLONE: But essentially these two points are oxidized for the same time. These two are oxidized for the same time. The ECRs are a little

different just because the wall thickness is different. So each of these points you can match up as being oxidized for the same amount of time.

Okay. Let's quickly look at what happens if we go to the extreme case of 3,400 seconds. Why is the weight ECR so high for Zirc-4, the measured. This is the -- these are to scale, these two pictures. And again, the M5 is 0.61 millimeter wall thickness. This is 0.57. That's why there's a difference in wall thickness. But anyway, you grow a really fat, thick oxide layer on the Zirc-4. The M5 layer sort of hits a point and doesn't seem to grow very much anymore. So what is important is, as you go out in time at 1000 degrees C is how much oxygen is getting into the base metal.

A better picture of M5. And an amazing thing is with all the complexity of this picture, which Jean-Paul Mardon could explain much better than I can, except I can pick out the oxide layer, I could pick out what's trying to be an oxygen stabilized alpha layer. And then you've got all these precipitated layers that are stabilized by oxygen and niobium. And what's interesting is the microhardness across this sample is almost identical to the microhardness across the Zry sample. And the

microstructures -- I mean, this is within experimental error. If I look at the middle 200 microns on this picture of 0.2 millimeters, and look at the microstructure across that middle where you're going to get your ductility, they're essentially the same for the two materials in terms of -- DPH is diamond pyramid hardness. It's a Vicker's hardness number. And to me it's amazing, unless the microhardness is just not fine enough to pick it up. But the two oxidized for the same period of time would have essentially the same microhardness, even though they're different alloys and have very different microstructures.

Okay. ZIRLO is a lot closer at 1000 degrees C to Zirc-4. Again, these two points of extreme were for the same test times, and they start to fall apart there. But if you go to lower test times in terms of measured ECR and offset strain they're pretty close.

CHAIRMAN POWERS: Maybe the definition of ECR is confusing to me. Because you showed the data for M5, and Zircaloy 4 at 1000 degrees, and when plotted against measured ECR things didn't correlate. They looked very different.

DR. BILLONE: They looked very different.

Right.

CHAIRMAN POWERS: When plotted against calculated ECR, they look very similar.

DR. BILLONE: Right. And I'm going to get you -- by the end of the talk, hopefully before the end, I want to get you away from ECR as a meaningful metric.

CHAIRMAN POWERS: I know where you're going, but unfortunately I've got some criteria here I'm looking over that -- they focus a lot on ECR. And so I've got to understand this a little better.

DR. BILLONE: Okay.

CHAIRMAN POWERS: If I take some zirconium, bare, clean, pristine, stick it into the steam and measure the amount of hydrogen coming out, does that give me the -- can I convert that into a measured ECR?

DR. BILLONE: Where are you taking the hydrogen? What's coming out --

CHAIRMAN POWERS: Yes, out of the experiment.

DR. BILLONE: That's a very poor way of doing it. There's a lot of error associated with it.

CHAIRMAN POWERS: Assume my measurement is fantastically perfect.

DR. BILLONE: Oh, I'm sorry. Okay. We actually use the heating to melting, and measuring how much hydrogen comes out to determine the hydrogen content. Assuming you have a good measurement of hydrogen content, finish the question.

CHAIRMAN POWERS: Well, what I'm trying to understand, exactly why there's a difference between measured ECR and calculated ECR.

DR. BILLONE: Oh. There's very little difference at 1100, very little difference at 1200. There's very little difference for Zirc-4 between measured and calculated, because the calculated is based on a correlation for Zirc-4. M5, the oxide layer just stops growing. I can't explain it.

CHAIRMAN POWERS: Okay. So your ECR is not an effective amount of oxygen taken into steam and put someplace. It is in fact the amount of oxide layer?

DR. BILLONE: No, it's literally the change in sample weight due to all the oxygen that's been picked up, most of which is in that oxide layer. All the oxygen that's picked up adds to the -- you know, you take the sample before the test.

CHAIRMAN POWERS: That's kind of what I thought going into this, because --

DR. BILLONE: That's how we measure weight gain.

CHAIRMAN POWERS: See, if I measured the hydrogen, I would know how much oxygen was put someplace.

DR. BILLONE: I know, I know.

CHAIRMAN POWERS: Okay. But I would assume then that there should be a very close correlation between that and what Cathcart-Pawel comes up with.

DR. BILLONE: For Zirc-4 there is. For Zirc-2 there is. For ZIRLO it's not too bad. And ZIRLO is nominally 1 weight percent tin, 1 weight percent niobium. For M5 and E110, they're the Zry-1 niobium at 1000 degrees C, and probably maybe even 950 to 1,050, somewhere in that range. There's something that I can't explain to you, that I think maybe they can. You have to go to extremely high test times to get higher weight gains. And as a matter of fact, you have trouble doing it. We did, anyway. Because that oxide layer just falls off. And it's -- the oxide layer is where most of the oxygen is, therefore it governs the weight gain. It's just not increasing with time very fast, yet the oxygen that's diffusing into the metal continues to go along. And the

evidence for that is if you essentially plot these results versus the square root of time, just think of this as the square root of time, and all these tests are run for the same time. You end up at about the same place for both alloys. This has a really fat oxide layer and a high weight gain, and this has a really thin oxide layer and a low weight gain. It's just -- it means it's not a good metric. For me, anyway, working in the laboratory.

DR. MEYER: So that's the reason, Dana, that we don't use a measured or true oxide as the limit. And this time in our proposed criteria, the Cathcart-Pawel equation would be included in the criteria. I mean, you need to use that in order to be calculating the time rather than the true amount of oxide. Because it's the time that's important for the diffusion of oxygen into the base level.

MEMBER SHACK: Is this an oxygen penetration depth now? Is that what I'm looking at when I plot it against the calculated Cathcart-Pawel?

DR. MEYER: I'm sorry. Is an oxygen what?

MEMBER SHACK: Is it oxygen penetration depth. Is that --

MR. SCOTT: You could say that, because ECR is equivalent cladding reacted, so it's some

weight of Zircaloy on the surface.

MEMBER SHACK: I mean, it's got a square root of prime in it. It has an oxygen diffusion.

DR. BILLONE: Yes, no, that's because the oxide layer moves, grows as a square root of time. The oxygen stabilized alpha layer grows as a square root of time, ergo the beta layer shrinks as kind of the square root of time. But in addition, you have a source of oxygen always there, whether you have a thick oxide or not to diffuse into that beta layer. And that also goes as a square root of time. So it's not a coincidence, but you don't need more -- let's just pretend from Time Zero you had this oxide layer, and it never grew. It's probably enough oxygen to continue to diffuse into this metal, the metal's very hungry for oxygen, and increase the solubility of oxygen and decrease the ductility. And that's why ECR measured, which is based on weight gain, that's what I'm saying is not a good metric. I mean, these two -- this is a classic case. These embrittle at about the same rate in time as you go to higher time values at 1000 degrees C, but their weight gain behavior is totally different, and that's what skews the results on this slide. It doesn't matter how thick this is, except for Zirc-4 there's a nice correlation between

how thick this is, how thick this is, and how much oxygen made it in there, for Zirc-4. And I'll show you that later in the embrittlement correlation.

CHAIRMAN POWERS: I guess --

DR. BILLONE: For M5 there's not.

CHAIRMAN POWERS: I guess what you're telling me is that if I did the experiments Cathcart and Pawel did, but I did them with M5 --

DR. BILLONE: You get different answers.

CHAIRMAN POWERS: -- I would not find a good agreement with the correlation I developed with Zirc-4?

DR. BILLONE: Correct.

CHAIRMAN POWERS: Very interesting.

DR. BILLONE: That's correct. And 1000 C. The correlation's valid.

MR. SCOTT: 1000 C is the lower limit of Cathcart-Pawel. It's no good below that.

DR. BILLONE: It's the lower limit, but actually works pretty good, the weight gain part, not the rest of it. The weight gain part really works pretty good to 950, to tell you the truth. Not how thick is the oxide layer, how thick is the alpha layer. That's where it starts to break down. But, all right.

ZIRLO, again, shows about the same embrittlement with time, and yet at the highest test time of 3,400 seconds, you can see the ZIRLO not only has a thinner oxide layer than the Zirc-4 -- this is at 20 percent calculated ECR, this is about 22 percent measured, this is about 18 percent measured -- but the interface is rougher. This is a precursor to breakaway oxidation. And this material's picked up about 10 weight part per million hydrogen during all this time, and this material's picked up 100. So, this is 3,400 seconds single-sided oxidation. You're not in breakaway, but you're in a transition regime where your oxide isn't growing as fast as initially it should be. It's going to start to break up, perhaps. We haven't tested for a longer time. But that's part of my focus, that the difference in measured is really, again, the difference in the thickness of the oxide layer.

My real focus is that you're going to have a microstructure. This is now etched where this is the oxide layer. Maybe. Yes, that's the oxide layer. You're going to have a microstructure that looks very, very, very different from Zirc-4, and even different from M5, and you're going to have a range of microhardnesses that are the same as the ones I just

showed you in this middle region. And somehow, somehow, roughly the same amount of oxygen made it to that middle region and caused the same degree of embrittlement. It's kind of interesting, because if you get into the metallurgy, and you get into the details of modeling that, it's really tough to do. And it's possible that there's some simplifications just by the fact that you're running these tests, and you're getting about the same answers for the same test times.

Okay, skip that. I want to skip the 1100 degrees C results. You've got them in there. They're just not interesting. All three alloys have weight gains that are consistent with the Cathcart-Pawel prediction. All three alloys behave very well. They all kind of flatten out to the same 3 percent offset strain at high values.

I want to go to 1200. Okay. This is our thermal benchmark for 1200. I'm just showing you the thermocouples that were welded at the sample. The 1200 degrees C test, double-sided to get to 20 percent ECR is only 400 seconds. And that's where how you get there matters a little bit. If you're talking about a 5 percent ECR test, a significant part of that 5 percent was picked up here. If you're talking about

20 percent ECR, this doesn't matter so much. Our temperature ramps are designed to not overshoot.

Okay, this is room temperature embrittlement. We've already talked about this. This is M5 versus Zirc-4 ring compressed at room temperature, and you can see the curves nosedive down. Just again, the microstructure, and I've reversed sides. I apologize for that. This is the Zirc-4. But you could see the rather thick oxide and alpha layers. And actually, at 20 percent ECR, your oxide layers -- it doesn't matter whether it's 1000 degrees C, 1100 degrees C, or 1200 degrees C, it's 20 percent measured ECR, your oxide layers are about the same thicknesses. But again, very different microstructures, but very similar microhardness values. And this is ZIRLO, and Zirc-4.

Now, this is what I wanted to get to. Let's compare Zirc-4 to itself. This is same sample, with multiple rings cut from it. So, it's the same oxidized sample. We didn't have a whole low left. But this is your baseline curve, and this is offset strain. For the room temperature data this is retesting the adjacent ring at 135 degrees C. You get significant enhancement in ductility in this regime. Eventually you're going to lose it at maybe 18 -- oh

this is measured. Okay, that's the problem. All right. You're going to lose it at a measured ECR of about 20 percent. It would be a calculated ECR closer to 18 percent. So this is all as-fabricated material, artificially pre-hydrided -- not pre-hydrided yet. I'm sorry. As-fabricated material. And all we're doing is elevating the test temperature. So the same is true of ZIRLO. You get even more enhancement. And the same is true of M5. So if you're looking for ductility at 1200 degrees C, the higher ECR values, you've got to go the higher test temperature.

CHAIRMAN POWERS: Just a clarification. You have plotted these things for M5 against the measured ECR. If you were to plot them against calculated ECR, how would the curves change?

DR. BILLONE: Oh, very little. 1200 degrees C --

CHAIRMAN POWERS: Everything's fine.

DR. BILLONE: Everything's fine. Another hundred degrees, everything's fine. 1000 degrees, very different.

CHAIRMAN POWERS: Not so fine.

DR. BILLONE: I think I do do that in the next slide. Okay, so this is Cathcart-Pawel ECR. I just wasn't quick enough in punching it. And now

we're going to switch. We're going to just compare Zirc-4 to M5. And just to show that the M5 for the same amount of test time shows greater improvement in ductility when you increase its temperature to 135. So, these are both 135, but again, it's almost -- and this may not be true of all alloys, but if you can sort of capture the behavior of Zirc-4, with the massive amount of Zirc-4 data, blindly apply it to the new M5 and ZIRLO data coming out, just to see if it's a conservative bound, or lower bound, maybe not a best estimate. It's a game that we're in the process of playing.

And again, this is -- I'm going to add one more data point, which is the test we did for Paul Clifford and Harold Scott. We didn't want to do a meaningless demonstration test, so we took ZIRLO up to 21 percent calculated ECR, and we essentially just barely got the transition between ductile and brittle behavior, up around 21 percent. And for Zirc-4 we expected in terms of CP-ECR to be about 18 percent. So that's just the new data point we threw in there.

This I want to skip. Fascinating stuff. That's for discussion. Okay, that wraps up everything we did for the 17 x 17 PWR alloys, fresh or as-fabricated, or the French call it as received. Now

we're going to add some hydrogen and see what the effects of hydrogen are on modern Zirc-4. And then we're going to switch over to 15 x 15 Zirc-4, which is our baseline for a Robinson test. And you can cut me off anytime you need to.

CHAIRMAN POWERS: We are scheduled to stop for lunch at 12:30.

DR. BILLONE: Fine.

CHAIRMAN POWERS: And I see no real reason not to do that since you're the speaker right after that.

DR. BILLONE: Right. Let's do that. I just have a couple of slides on the pre-hydriding. Again, we pre-hydrided in 4 percent hydrogen-argon mixture at 400 degrees C. Anyone who's done pre-hydriding knows it's very tricky. It depends on whether you get impurities. If you get any oxide at all, even a fine film on the surface, that's what controls the kinetics and slows everything down. If you keep the surface clean, the kinetics are very fast. And we got some rather wild axial circumferential gradients in hydrogen concentration in our samples. We turn this into a benefit because we were cutting small rings. We could take one 4-inch sample and have anywhere from 400 to 800 ppm of

hydrogen. So what it made us do is after each ring compression test, we had to measure the specific hydrogen in there. And that's why --

CHAIRMAN POWERS: -- do a blind test here.

DR. BILLONE: Well, nobody else does tests this way. They perfect their method of hydriding, and they run a bunch of tests at 300. You learn everything you can learn at 300 weight parts per million. Well, because we were amateurs at this at the time, and we've improved since then, we ran tests at a fixed ECR, fixed test time, for hydrogen levels in the range of 150 to 600 weight parts per million. And basically what we determined is that if you fix the ECR and run a bunch of tests at 8.5 percent, measured, I'll convert this to 7.5 percent CP-ECR, then at 400 weight parts per million hydrogen that's where you lose the ductility. At 350 you have ductility. At 400 you've lost it. And I'll show you that curve.

CHAIRMAN POWERS: Let me ask you this question, just as a matter of experimental technique. You're always doing 8 millimeter long specimens?

DR. BILLONE: Right.

CHAIRMAN POWERS: Have you ever --

DR. BILLONE: Sometimes we do 5 and 6,

because that's all we have left, 5 and 6 millimeters.

CHAIRMAN POWERS: Well, what I was wondering is did you ever take a specimen that's fixed of the same composition and run 8, 16, 32?

DR. BILLONE: No. Our Russian colleagues did that before we got started with their E110 and maybe some Zirc-4. And of course, it's not conclusive necessarily to take fresh cladding, not oxidized, and find out that there's no length effect. But they did some oxidation. I guess based on --

CHAIRMAN POWERS: Well, really what I was wondering --

DR. BILLONE: We haven't done it.

CHAIRMAN POWERS: What I was wondering is --

DR. BILLONE: On purpose. We haven't done it on purpose.

CHAIRMAN POWERS: -- if you're -- when you cut the ring out, you're changing the -- you can change hydrogen content in the cut ends of it.

DR. BILLONE: It's pretty low temperature cutting, and I don't --

CHAIRMAN POWERS: It's pretty low temperature cutting except where the teeth hit the metal.

DR. BILLONE: Okay. How about my next slide to address that point. No, we haven't explored -- it's more in the spirit of, okay, this is how we're going to do our screening tests. We're keeping the length the same, we're keeping the test methods the same, and we're going to just change the material. Other labs have done an exploration of different length size. Sometimes we run out. We don't have enough material, so I would say 5 to 10 millimeters just as a range that we've tested. Sometimes we cut too big, and sometimes we don't have enough material.

All right. The point is that what I show you is all you're going to get because we have no more 17 x 17 Zirc-4. It's very hard for the vendors to come up with it because they switched to ZIRLO and M5. And we can't continue this. So we continue these kinds of studies with 15 x 15 cladding, which is easier for us to get. It's our Robinson-based line.

Here's something that was interesting to me. This is a 25 millimeter long sample, one inch. And before we oxidized it we cut a 2-millimeter ring from this end, a 2-millimeter ring from the other end, and in round numbers there's a 440 to 540, there's 100 ppm of hydrogen gradient across it. It may be worse than that because you don't know if it's a linear

gradient or not. What was interesting is when you bring this up to 1200 degrees C for about a minute, you figure the hydrogen diffusion -- first of all, you've got 100 ppm of hydrogen as a concentration gradient. And I haven't mentioned the circumferential gradient that's also there. You'd expect the hydrogen to homogenize. You're in the beta phase, you've got hydrogen, you've got high diffusivity, and it don't, because when we finish the test, we then cut three rings, ring compression test, and measure the hydrogen, and lo and behold, this is the -- goes from low to high. The hydrogen has not moved along axial distance. It has not moved along circumferential distance. Definitely moved across the radius, particularly since the beta layer sucks up hydrogen. So this has implications on how we select our high burnup LOCA samples. If you pick, for example, corner rods, which have extreme variation in circumferential hydride -- in the density of hydrogen as you move around the circumference, whatever you start with you're liable to end up with, and so picking your sample to be prototypic is very, very important. And again, these are the results I mentioned. Fixing each test the same ECR, and also having the benefit of up to three rings from each sample at different hydrogen

levels, and the hydrogen didn't move. Basically we were able to map out at our lower value, and again, this is 7.5 percent, and this is 10 percent in terms of CP-ECR. But essentially we know what no hydrogen does, and this is -- with the Zirc-4 we have this is the best we could do was to generate these points. And then when you get to about 400 ppm you've lost all your ductility. You go to a higher oxidation time, and you're going to lose your ductility sooner.

The shape of these curves are strictly artistic. They're trend curves. We don't have enough data points in this region to really shape these curves. It's how we think the material would behave. But, the ductile to brittle transition is as good as we can define it with the limited amount of material we had.

CHAIRMAN POWERS: If you were to put --

DR. BILLONE: And by the way, 135 degrees C test temperature. This is the highest -- this is the most optimum test temperature we can do.

CHAIRMAN POWERS: If you were to put error bars, I guess they would be on the hydrogen content?

DR. BILLONE: They would be on the hydrogen content because we haven't done enough duplicate samples. But, yes. There are error bars,

and you'll see with the irradiated stuff typically for our Robinson samples, it's something like, near the mid-plane it's 550 weight parts per million +/- 100 weight parts per million.

CHAIRMAN POWERS: I guess what I was fishing around with there was that the offset strain, if you have -- suppose we knew the hydrogen content exactly, it was given to us by God.

DR. BILLONE: Or CEA.

CHAIRMAN POWERS: Or CEA. And we did replicate tests on the offset strain, there would not be much variation from sample to sample?

DR. BILLONE: Shouldn't be, right.

CHAIRMAN POWERS: Okay. So your error bars would all be horizontal for those?

DR. BILLONE: And also, if you do have a circumferential variation that stays in the material throughout the oxidation, then randomly how you orient the sample will have an effect. Okay. All right, let's switch off to now, that's the end of our advanced alloy program up to now, because the next stage is to test the high burnup Zirc-4 -- I'm sorry, the high burnup ZIRLO and M5. So let's drop back, and let's build up to our LOCA program, and our testing of irradiated cladding, which is H. B. Robinson cladding.

And we're going to focus now on 135 degrees C tests, post-quench ductility tests, of samples that have been oxidized at about 1200 degrees C.

CHAIRMAN POWERS: Is this an appropriate place to break for lunch?

DR. BILLONE: Yes. It's excellent.

CHAIRMAN POWERS: Why don't we do that then. We'll recess for lunch, and we will resume at the scheduled time, which I think is 1:30.

(Whereupon, the foregoing matter went off the record at 12:22 p.m. and went back on the record at 1:29 p.m.)

CHAIRMAN POWERS: Let's come back into order. First I want to make a couple of comments. The Reactor Fuel Subcommittee have always been well attended, and I think we're among the best technical subcommittee meetings that I enjoy as a member of the ACRS. The technical presentations have been uniformly outstanding during my tenure on the committee. From all sides of the house, certainly Ralph has brought us an award-winning speaker today. Rosa Yang brings speakers of the quality of Robbie Montgomery and Joe Rashid, who I think make outstanding presentations. What we do find is useful is some audience participation, and so that if you do want to make

comments during the course of the presentation, we do have microphones located around the room. I think on either side of this post. And the only criterion is that you do state your name and speak clearly when you make your comment.

The subcommittee meetings are open to participation by all members of the affected community, and the ACRS has found this to be very valuable to get this broad input into our information-collecting regards. The security forces have chosen to impose some new rules to make that cumbersome, and I apologize to everyone. Like many large institutions, they're very good at making rules, very poor at figuring out how to accommodate that. And I hope that that has not made this participation any less satisfactory for you, and I assure you that I will ensure that it will not have this cumbersome character for it in the future. With that, Mike, pick up where you left of.

DR. BILLONE: I think I better pick up, and pick up a little faster than we left off.

CHAIRMAN POWERS: Yes, we're going to try to hold you to schedule a little bit.

DR. BILLONE: I will, I will, I will. Okay. Let's switch gears. Let's switch from the

advanced alloy 17 x 17 Zirc-4, ZIRLO, M5. Let's go to what we're using as baseline for our high burnup cladding. And these baseline studies prove to be very important in planning our in-cell tests. Remember, they're inexpensive, you can run lots of them, you run very few in-cell tests.

So, as-fabricated Zirc-4. The dimensions are up there. It's a thicker wall than you're used to, thicker than a lot of the 15 x 15 currently used. And we did a lot of work with temperature control and temperature monitoring. And I'll show you those results. We also did a lot more work with pre-hydrided 15 x 15 Zirc-4. We're also using this material for our advanced alloy -- no, for our spent nuclear fuel program to study radial hydrides.

What I'm basically going to show you is that we -- this material we dropped down to lower ECR levels, and at 5 percent Cathcart-Pawel ECR we got embrittlement at 600 ppm of hydrogen. We haven't run any tests with 600 to 800 ppm of hydrogen where we got any ductility at all. We just haven't run tests at low enough times. 135 degrees C post-quench ductility versus hydrogen content. We fixed the ECR at 7.5 percent, which essentially means we fixed the time of the test. And we got embrittlement, at the higher

oxygen level we got embrittlement at 400 weight parts per million hydrogen.

MEMBER KRESS: Now on your pre-hydride --

DR. BILLONE: Yes.

MEMBER KRESS: The 400 weight parts a million, that's the amount pre-hydrided?

DR. BILLONE: Right. And we don't pick up any hydrogen during the test.

MEMBER KRESS: Don't pick up any hydrogen at the 7.5 percent.

DR. BILLONE: Well, with these alloys that I'm talking about. And we're excluding E110. They grow a protective oxide layer in the sense that hydrogen and oxygen, that this associated the outer surface of the oxide layer, the hydrogen doesn't go through. It's swept away. So we're talking strictly what you put in at the beginning. When we get to the LOCA tests of the samples like this, it's a different story. But, okay.

It's very, very important -- I'll say it one more time. Anybody that shows you data in terms of ECR and ductility, you have to know at what temperature is that ECR accumulated. We are very conscious of not overshooting our goal temperature. You can obviously ramp up much faster and overshoot,

and then come down. And so we ramp up very fast to about within 100 degrees C, and then you have this slower rate. It's important in two-sided tests because you've already picked up about 1 percent ECR here. And at 5 percent ECR you're just barely touching 1200. So when I say we're running these tests at 1200, you have to keep this in mind if I'm going to show you results for 5 percent ECR because it picked up most of its oxygen at a lower temperature. And we know that the test temperature's extremely important for the solubility and the embrittlement. So, all data sets are linked with this. This is not a problem when we leave the world of ECR and go into the world of how much oxygen did we pump into the beta layer, and how does that correlate with embrittlement. Because for that you'll have a rate equation that could integrate over any experimental temperature history, as well as a calculated history for a LOCA. As long as we are willing to leave this simple concept of ECR -- for mechanistic understanding, Ralph, I'll add that -- then it doesn't matter whether a CEA goes straight up like that very fast, and we go up more slowly, we'll be able to integrate out that effect in a proper correlation.

Okay. Because we had more material we ran

more tests. We ran duplicate tests. And basically, now this is in terms of measured ECR. This is the real permanent strain measured diameter before the test, diameter after the test. This is the real thing. And basically we're able at room temperature to show at about 8 percent measured ECR, 7.5 percent CP-ECR, which is my next figure so I won't say it. You lose ductility. You drop below that 1 percent criterion we have for this parameter of permanent strain, with a 2 percent criteria for offset strain that you get off of graph. This is a physical measurement.

Now, increasing the test temperature to 100 degrees C is a significant improvement in that you get up about 12 percent, and then 135 you get up about 14 percent in terms of measured. Because we're going to use this to plan our in-cell tests we don't care about measured ECR. We care only about predicted ECR. That's all you can do for test planning.

CHAIRMAN POWERS: Do you understand why the ductility changes so dramatically with a relatively small increase in temperature?

DR. BILLONE: No, but I understand that the same results are produced at other laboratories, so I have a lot of confidence in the quality of the

results.

CHAIRMAN POWERS: I don't doubt it. I just wondered --

DR. BILLONE: But I don't have an explanation for why the prior beta phase is so sensitive to temperature, because the as-fabricated material is not this sensitive. The alpha, the stuff that you make in the factory that's alpha phase material doesn't show this high sensitivity.

Okay. Let's change now, because we're in a predictive mode. We want to use these test results to try to predict how the high burnup cladding is going to behave. So this is the same data set I just showed you, only in terms of the square root of time essentially, or CP-ECR. And now I'm going to switch back to offset strain. And these are trend curves that show roughly where you hit embrittlement for the different test temperatures. Different way of plotting the same data set. But more data points than we had for the advanced alloys.

Okay. And this is the, again, well more data points in one case. This is the pre-hydrided stuff. Again we had variation in hydrogen. These three data points are all from the same 25 millimeters sample, three rings cut. Each one had different

hydrogen content. And I cringe because I want this point and this point to be reversed, but I just can't change Mother Nature. That's the scatter. But anyway, this is roughly -- we barely got samples to test it, but roughly 400 degrees C, 400 weight parts per million hydrogen. You back off a little bit, you start picking up some ductility. This is all an artistic rendition.

And again, around 600 weight parts per million you lose it at 5 percent ECR. This is important because our Robinson samples, some were around the middle of Grid-span 2 are at about this hydrogen level, and some were -- Grid-span 3 is the midplane there. Some were around Grid-span 4. You're up to here and you're as high as 800. So where did we pick our samples to do our studies? We picked our samples from Grid-span 3 with about 550 weight parts per million to look for ductile to brittle transition. So, again, we're using all this to help pick the axial location of the samples as well as to pick the test time.

Okay. So let's get to the high burnup stuff. We took -- we're going to show you ductility of high burnup oxidized 15 x 15 Zirc-4 rings. And the way this works in-cell is quenches is kind of violent

on our apparatus. If we quench, we most likely knock out one of the control thermocouples. And so the way these tests are run, you run several tests with slow-cooling, and then you repeat one of those tests with quench, and then your test train is finished. And then you go on and build another test train to do another series. So, you're not getting post-quench ductility of all these points for very practical reasons.

We very carefully selected a rod from within the assembly rather than an edge rod because of this issue of non-uniform hydrogen distribution. And we looked at a variety of things. I'll show you the characterization. Again, then cut these 25 millimeter long samples from near the midplane. They had about a 70 micron corrosion layer, and about 550 weight parts per million hydrogen. I said +/- 100, +/- 90, close enough. And very significantly there was about a 10 micron fuel cladding bond layer that contains oxygen. That's going to play a role in what I'm going to show you. There was one sample higher up on the rod with 95 micron corrosion layer, 800 weight parts per million hydrogen. I didn't know this till after we tested it, actually. This doesn't really have a prayer of ductility. That's the one we quenched.

CHAIRMAN POWERS: Of course.

DR. BILLONE: So, after the tests were run, we improved our benchmarking tests. And we're now claiming more like 1190 degrees C rather than 1200 degrees C. Let's not quibble about 10 degrees C. And these are the calculated CP-ECRs. We had no idea whether we'd have any ductility at all with 550. So we ran low and high. This is the sample with the high hydrogen. That's the one we quenched. We knocked the heck out of the ductility. That was zero.

So, I'm going to show you what I'm going to call post-oxidation ductility -- I hope I don't use this acronym very much -- and post-quench ductility of this. Okay. Just for your own record, this just shows we took the samples from near the midplane of the rod. This shows that there is some variation, but it is very small, in the corrosion layer. Yong Yan's a metallurgist. He did not use the word "corrosion". He doesn't know that - he doesn't think there should be a distinction. This is what you grew in reactor, about 71 microns +/- 5. And traditionally we take eight segments around the circumference to determine that.

All right. This is going to be very interesting because the very next test we run after

this meeting's over is going to be a test with a long segment with fuel in it, one-sided oxidation, it will be sealed at both ends. And this is fuel. There is extra oxygen in the fuel. There's a model and macro for how that extra oxygen might get into the cladding. No one really thinks it will, but no one's really tested it. This is the fuel cladding bond, which is an oxide layer. It's about 10 microns in round numbers. And this we found is reduced without the fuel here when we run on one- or two-sided oxidation tests. And the oxygen from this layer does get into the cladding and form a bond layer. So, I'm very excited about this next test which you will not see the results of because we haven't run it yet, because it will have all the features of high burnup fuel, and we can stop arguing does it matter or does it not matter.

Okay. We looked carefully at the hydrides to make sure that, you know, we didn't have huge densities of hydrogen on one side and low densities on the other. This is low mag. You're not going to see much from that. This is your traditional Zirc-4 high burnup profile of hydrides, very dense near the oxide surface, and progressing in. What's interesting is someone asked -- Bert I think asked the question does

the as-irradiated material have any ductility. If you take it out of the pool and then just subject it to the ring compression, does it have ductility. And what happens is when you test it without any additional oxidation, you get a brittle failure -- you get a failure. It's not brittle. You get a failure, a crack across the oxide layer, a crack across the hydrogen layer, and as you move in, the crack starts moving off in another direction, and you actually have very high ductility -- relatively high ductility in a ring compression test for the stuff coming out of the reactor. And that's at room temperature and at 135.

Okay. So we built the new test train. These are the results for -- this is what we went into the test with. This is based on bare cladding, no corrosion layer, no steam oxide layer. We used this for test planning. And of course, because the heat of oxidation is going to be slowed down by the corrosion layer, you're not going to steam oxidize as fast having that corrosion layer, these are all over-estimated ECRs. And so let me show you the results of a benchmark in which we weld the thermocouples, we grow an oxide layer, we cool down, then we ramp up again. And what we're interested in is for bare cladding you have a high heat of oxidation during this

ramp, for cladding with an oxide layer you have a low heat of oxidation. How does it affect this ramp, how does it affect the approach to the steady state, and how does it affect the steady state temperature?

So, this is what we used to correct our data. This is the thermocouple welded above the sample in the Inconel. It's the same in both tests. The blue is the bare cladding, and the red is cladding with about 37 -- let's call it 40 micron steam oxide layer that we grew on it. What's the difference? The difference is you've knocked the heck out of the reaction rate, the heat rate released. And so you reduce this early temperature, and you basically come to about the same steady state temperature.

Let me show a graph in terms of the table. What does it do, bare cladding versus the pre-oxidized cladding? It knocks this early peak down by about 40 degrees C, and it only knocks the steady state temperature down by about 8 degrees C. And in this business, that's pretty good for the steady state temperature.

So, it's really -- let me go back. The data I'm going to show you are all analyzed in terms of this red temperature curve with the steam oxide layer grown on it. Okay. These are basically our

data. I'll show them to you graphically in a second. We ran the tests in this sequence. These are the corrected ECRs. They go from 2.6 to 9.3, instead of 3 to 10. So that temperature correction made a difference of about 0.7 in the calculated ECR. Not huge.

This is the calculated oxide layers that are predicted at both the inner and outer surfaces, assuming they're bare. You can see that where we did get really good data for the 7.4, you see our sample. It's predicted to be 30. We're only measuring a steam oxide layer under the corrosion layer of 17 to 20. So it's a different question -- it's not an embrittlement answer I'm going to give you, but the corrosion layer did slow down the weight gain due to steam oxidation. And if we compare that, the best comparison is to compare what happens at the outer surface to what happens on the inner surface. You grow a thicker steam oxide layer. You only have 10 microns of fuel cladding bond, and that oxygen gets sucked up by the metal.

Okay. And again, we did some quantitative net on this one too, the sample number 4. Same basic results. I mean, eventually this kind of catches up a little bit. You predict 38 microns, you measure 28.

This is just measuring the steam oxide layer, which is very subtle to do. Okay. What I'm going to show you, I'm just going to point out what's in red. Based on quantitative metallography, and your estimates, and published estimates of what the oxygen content is at each phase boundary, what we call our measured weight gain converted to ECR is 5.6 percent when you have the corrosion layer versus 7.4 percent assuming it's bare cladding. That doesn't mean you get that much benefit in terms of ductility, or it just means that's the difference in the weight gain between this kind of corroded cladding and bare cladding.

And finally, the other thing we did, we had trouble determining boundaries between corrosion layer and steam oxide layer, so we went to SEM imaging to get a better picture of what was going on. This is just to show you grossly this is the sample coming out of the reactor, coming out of the reactor pool. It's basically monoclinic oxide, but it's a little bit dark. And you're looking through a hot cell, colors are distorted. After the test, this corrosion layer has picked up oxygen from the steam, become more stoichiometric, become more white in appearance. We also lost a lot of it during the test, probably during the cool-down.

CHAIRMAN POWERS: What's the non-stoichiometric range for monoclinic oxide?

DR. BILLONE: Pardon?

CHAIRMAN POWERS: What's the non-stoichiometry range for monoclinic oxide?

DR. BILLONE: It's only -- it's like instead of ZrO_2, it's $ZrO_{1.98}$ to 2. It's a small difference, but it has a huge impact in terms of what phase you're in. And what stabilizes the good oxide is being hypo, a little under in oxygen. And what drives you to the bad oxide is whatever drives you to $ZrO_{2.000}$.

Okay. This is why we went to SEM imaging. This is a low mag image of the cross-section, just to show you, I want to show you the outer surface layer. By the time we mounted these samples, they've already come out of the furnace. They've already been cooled down, come out of the furnace, now prepared. They started with a 70 micron oxide layer. We can't find 70 micron corrosion layer. We can't find 70 microns anywhere. It's disappeared partially, and it may be during cool-down that it goes, some of it goes. Some of it does get absorbed into the metal. The corrosion layer is a source of oxygen. But our main concern here is although we could -- in different images we

could pick out the alpha layer, the oxygen stabilized alpha layer, and the beta layer, this transition, we weren't sure where the boundary was of the steam oxide layer based on metallography, or optical microscopy. So, you go to SEM, and you get a very clear contrast between how the corrosion layer appears in the SEM, the steam oxide layer that grows underneath it, the OD alpha, and the beta phase. And we were able to use that to improve our calculation of how much oxygen pickup there was for this sample. So we went back to the optical metallography and redrew these boundaries. And it effectively reduced our steam oxide layer a little bit.

Now, in saying this I'm using very simplistic language. Let me go back to Ralph's diffusion expertise. I can't guarantee you that the oxygen atom -- steam atom, steam molecule that arrived here where the oxygen disassociates, that that oxygen is the one that comes through all the way to the surface of this, and forms --

CHAIRMAN POWERS: It surely does not.

DR. BILLONE: I know. I can't. But in terms of really simplistic models, it acts that way. But it does not -- I'm not offering you a mechanistic explanation. I just want to make the point that the

corrosion layer slows down the weight gain.

CHAIRMAN POWERS: Sure.

DR. BILLONE: But not -- you don't get the full benefit of that in terms of ductility because it's also a source of oxygen for diffusion. So even if you didn't have any steam and you heated this up in a certain environment, non-oxidized environment, you will eventually embrittle the metal, even without steam.

CHAIRMAN POWERS: You're generating vacancies at the metal oxide interface, and those are coming out at you.

DR. BILLONE: Right.

CHAIRMAN POWERS: And the oxygen's hopping --

DR. BILLONE: It's hopping in.

CHAIRMAN POWERS: -- into the vacancies. And so the oxygen that got generated on the surface is lost to history --

DR. BILLONE: It's lost to history, correct.

CHAIRMAN POWERS: -- some other oxygen that pops in.

DR. BILLONE: All right. If you look at the ID, there's better definition because you only

started out with 10 microns of fuel cladding bond. So it's pretty easy to pick out what you grew with steam even thought that's not a great picture compared to the next one I'll show. That's a better picture. This is SEM. The trouble with SEM is you can only look at little spots. And so this is the steam oxide layer, and that pretty much grew as if you didn't have a fuel cladding bond layer of 10 microns. The 10 microns is kind of lost. It's too small to really pick out. But we were able to identify those layers much better with the SEM.

All right. This is Dana's favorite sample. We wanted to compare steam oxide layer alpha layer and beta layer for the pre-hydrided non-irradiated, exposed for about the same amount of time, and with about the same amount of hydrogen. This is non-irradiated pre-hydrided. We want to compare these layer thicknesses to what we measured for the irradiated stuff. So this is bare cladding, and this is how -- we've already seen this picture. And basically it's the same kind of data that I showed you before where on the outer surface if we compare these two samples, you obviously grew a thicker steam oxide layer on the bare cladding than you did on the corroded cladding. And even to some extent grew a

little bit more on the bare cladding on the ID than you did with the irradiated cladding.

CHAIRMAN POWERS: Argonne is apparently the only place in the world that thinks corrosion is good.

DR. BILLONE: Corrosion is bad because it dumps hydrogen in the material. It's not as bad as we used to think when we thought it was completely transparent, and you had the double whammy of having a lot of hydrogen, and a really rapid oxidation rate, and a rapid embrittlement. You're right, I'm the only one that says that.

CHAIRMAN POWERS: No, your colleague Dr. Shack tells me corrosion's been very good for his career, too.

DR. BILLONE: Oh, that's true. That's exactly what this -- this is just to show in terms of the oxygen stabilized alpha layer, and I guess I probably did not move -- no, this is okay. The red points are the irradiated. This is what Cathcart-Pawel would predict for bare cladding. This is the one data point we got from our 600 ppm sample. Even at our lowest test time, where it looked like we had no steam oxide layer on the outer surface -- and this is the sum of the inner and outer surface alpha layer

-- we were still pumping oxygen into the material, and stabilizing the alpha phase, even though it didn't look like any steam oxide. So, in terms of alpha layer growth, the corrosion layer didn't protect you from that, basically. Or if it did, the protection was small.

Okay. Let's plot the ductility results, remembering that all these open circles are slow-cooled, and only one was quenched, and that's unfortunate. I'll take responsibility for that. But basically, at our lowest test time, with 550 ppm of hydrogen, we got almost as much ductility -- more ductility as you would in the stuff coming out of the factory. We didn't see any intrinsic hydrogen embrittlement. We had a fairly low level of oxygen embrittlement. And this arrow means the test was stopped before the sample failed. This is done at 135 degrees C. You've got thermocouples. You don't have as much room to squash the thing.

We ran two tests, two rings, at about 4.3 percent ECR because I didn't believe the results. The first ring -- I predicted we would have zero ductility right here. First result we got was about 12 percent, but it didn't look like it really failed. And I thought they had mixed up the samples, so I insisted

on running the second ring, and the second ring had about 38 percent ductility. So, this is a non-failed sample. This is a failed sample. And that's why I'm putting the arrow up. That's what the arrow means. And then eventually we slowly coaxed down to zero ductility at about 9 percent predicted ECR.

Now the unfortunate thing. The last test we ran was with this sample, with 800 ppm hydrogen. That's what we quenched. And of course it's got zero ductility. The question is if the 800 ppm of hydrogen, yes. Is it the quench? I don't know. Next time we do this we would want to take a sample like this sample here which has pretty good ductility, slow-cooled, and we're going to want to quench it at 800 degrees C.

CHAIRMAN POWERS: So, what you really should have done is have an arrow horizontally coming from that 8 down toward 4 off that black point?

DR. BILLONE: Well, I plotted it as it is because this is not plotted as a function of hydrogen. It's all the more reason to get away from --

CHAIRMAN POWERS: But at 800 weight parts per million hydrogen, it may have had zero ductility, 7 percent CP-ECR, or 5 percent --

DR. BILLONE: Yes. Right. Oh yes, you're

right.

CHAIRMAN POWERS: Okay.

DR. BILLONE: Because at zero, it doesn't mean that's a transition. But I'm not going to put the arrow, though.

All right. Let's compare what we just did, but let's not do it in terms of CP-ECR. Let's do it in terms of something else. You don't need this graph. Or yes you do. I'm sorry, you do need this graph. But let's re-look at our data in terms of measured ECR. Just as an exercise. So, now we're going to talk about non-irradiated pre-hydrided Zirc-4 at two measured ECR levels, and the HBR Zirc-4, the high burnup Zirc-4 at two measured ECR levels. And we don't have enough data points, but the point is that when plotted in terms of measured ECR and hydrogen content, you really can't almost pick out the fact that this is the irradiated slow-cooled, and this is the non-irradiated quenched. And this is the irradiated slow-cooled, big deal, 800 is brittle in both cases. So as far as the samples where we got quantitative metallography to determine measured ECR, measured weight gain, they fall within the scatter. That's the honest way of saying it. They fall within the scatter of our database for non-irradiated

material.

Okay. What we're trying to say is what is the deal with the corrosion layer. What are the effects on steam oxidation kinetics and embrittlement? It's clearly a source of oxygen for growth of the alpha layer, ergo it must be a source of oxygen for oxygen diffusion into the beta layer, even without steam. And I still claim, based on just looking at the OD and the ID, you're getting partial protection from steam oxidation in terms of weight gain, in terms of how thick of an oxide layer you're growing. And this is primarily my evidence, not just comparing it to calculation. The net effect of post-quench ductility improvement, there appears to be a small benefit of cladding with the corrosion layer versus bare pre-hydrided cladding in terms of how much time at temperature it takes to embrittle it, assuming that these two have the same hydrogen content. It doesn't mean corrosion is good. So be very careful. Assuming these two have the same hydrogen content, there seems to be a small benefit in terms of post-quench ductility.

The fuel cladding bond is clearly a source of oxygen for alpha layer growth. We see it, and diffuse it into the beta layer. It's essentially non-

protective, or very small protection with respect to steam oxidation. And what's interesting with slow cooling, anyway, we don't see what has been proposed, which is that even without oxygen, if you got enough hydrogen in there that you would embrittle these samples. If you just took them up past the phase change temperature up to 900, 1000 degrees C, and then cool them down, you would get embrittlement due to hydrogen. That's observed in certain tests. We don't see that in our tests, but again, our tests are slow-cooled. And what I'm talking about is the low ECR tests which most of the oxidation occurred between 1000 and 1200 degrees C.

CHAIRMAN POWERS: Do we know what the hydrogen-zirconium phase diagram looks like?

DR. BILLONE: We know what the hydrogen-zirconium phase diagram looks like, but the hydrogen-zirconium-oxygen phase diagram is a little more challenging.

CHAIRMAN POWERS: What does HZR look like?

DR. BILLONE: What is the hydride ZRH? 1.66 or something like that. What does it look like. I know what it looks like in the alpha phase for normal reactor conditions. The problem with when you take it up into the beta phase, the beta phase has at

1200 degrees C a hydrogen solubility of as much as 5000 weight parts per million, and so if you're at 600 to 4000 or something, and you cool down slowly or quench, you never see any hydrides. There is an effect of the hydrogen, increasing the oxygen solubility. There's probably an additional effect which the Framatome presentation will cover very well and answer your question in much more depth. Because they have data they're going to show that I'm not allowed to talk about until they show it. So let's leave it to them.

Okay. I want to do this quickly because these experiments didn't work. They weren't as controlled. Coming out of the February 10 meeting, and I'm taking you up to that meeting that the data on the previous slide were presented, we decided that we really needed longer test times at 1200 degrees C to investigate high temperature embrittlement. We're also -- most of the rod is exposed to one-sided oxidation. It's only the balloon and burst region that's two-sided.

Ramp effects become less significant because you're running much longer time tests. And you notice our early ECR tests never got up to 1200. So there was a recommendation from the technical

advisory group to do these tests. Let's go to the results because we presented these results on May 10, and what I learned on May 11 is we probably shouldn't have run these tests because no one's done them successfully. But it's proprietary information, and so it wasn't available to me. Essentially what happened is the way we run them, we have an argon purge gas flowing on the ID. We had a little bit of steam leakage. We were worried about hydrogen pickup in the steam leakage. We sort of forgot the fact that the only thing keeping that hydrogen in the beta layer, that 550 ppm, is the fact that you've got that bond layer on the ID, and once that bond layer is consumed, you're going to lose hydrogen to the argon purge. It sounds really trivial, but I was banking on that not happening, or I didn't know about it at the time. So basically, these samples were not controlled in the sense that what hydrogen you started with is not the hydrogen you ended up with. That's why in our next test we're going to not mess around with small samples defueled. We're going to go to larger samples, truly one-sided steam oxidation, and have all the effects in there.

We did do some good thermal benchmarking. We're proud of that. And this just summarizes the

results. It includes our two-sided oxidation results, but basically the 550 went to 360 to 190, and then up to 770. So it's something varying in time during the test. It's not a controlled parameter. The clue was we had these data points for the one-sided test before we had the hydrogen values, and they didn't make sense because the embrittlement should have been much steeper than that. But the embrittlement wasn't steeper because this had lost more hydrogen than the one before. I'll find a way to make this useful in my embrittlement correlation, but it's going to be a stretch.

Okay. So the new test that we're going to do, let's go back to the same sample. This is where we did our two-sided oxidation test. Up in Grid-span 4 we had already cut a 12-inch long, 300-millimeter roughly, LOCA sample. We were going to use it for a LOCA test. Now, since we've already characterized that this has 550 weight parts per million for hydrogen here, 800 weight parts per million hydrogen out here, what you have is a hydrogen gradient in the sample. You do one ECR, you do quench, and then you get ductile to brittle transition as a function of hydrogen content. Let me move this a little faster.

So these are the numbers I was talking

about. This is the bottom of the sample, and that's the top of the sample. This is the midplane. And we only have uniform heating over these four and a half to five inches. So that's where the rings will be cut. And that's what we'll do with each sample that we oxidize. We'll cut rings for metallography and ring compression.

Let me try to wrap up. Test plans for high burnup ZIRLO and M5. We do have these samples at Argonne. The ZIRLO is from North Anna. It's not heavily corroded, the samples we have, 18 to 32 micron corrosion layer. I just threw down something for hydrogen, 200 to 300 weight parts per million. I'm not sure -- we're going to measure the hydrogen, as well as what the corrosion layer is. The M5 is what we call European M5, European reactor. That's probably typical. It's about 18 micron corrosion. I don't know the hydrogen content. I just estimated 100 to 200. And we're planning on loading those this week, and transferring them for sectioning, and then the steam oxidation tests.

We're going to do two-sided oxidation tests. I have to write this up as a test plan and separately send it to Framatome and Westinghouse, kind of get their blessing, and NRC at the same time. But

basically, we're going to do the 1200 degrees C test first because that's most embrittling, 1000 degrees C test next because that's an interesting alloy. That shows the differences in alloys at 1000 degrees C. And when we get time and more samples we'll do the 1100. And the idea for each of these is to do a 10 percent calculated CP-ECR, see where you are, are you ductile or are you brittle, and then either move up or move down accordingly. And I'm very excited about those tests.

CHAIRMAN POWERS: If I recall, your -- what you want to do is a quench test on something that's in the steep part?

DR. BILLONE: Something that's got about 10 to 20 percent slow cooling ductility.

CHAIRMAN POWERS: Ductility. And see if that drops way down?

DR. BILLONE: Right. And I pray it doesn't, honestly. Otherwise our expenses go way up.

CHAIRMAN POWERS: What you'd like to know is your slow-cooling is fast enough.

DR. BILLONE: Right. What I'm banking on is our experience with as-fabricated cladding that's not -- doesn't have hydrogen. And that may be the flaw. And that whether we quench at 800 or slow-cool

makes no difference whatsoever in the post-quench ductility. You don't have hydrogen in the sample, and these have hydrogen. And we're going to wait for the Framatome presentation to find out what the effects of slow-cooling versus slow-cooling/quench versus quench directly from the oxidation temperature. It's a fascinating area.

Okay, just quickly. LOCA integral tests. We've completed four high burnup fuel samples, the BWR Zirc-2. We did non-destructive examinations on all to determine how much swelling -- I don't like the word "swelling" -- what the diameter profile was, and also characterizing the burst. We cut up two of those samples to do metallography, hydrogen content, and oxygen content. And as Ralph, the picture Ralph showed, the 20 percent maximum ECR, meaning ECR in the burst region, that sample failed at three locations during handling. So we really don't expect any room temperature ductility from three of these tests that were run at the 20 percent ECR.

A few samples that are available for four-point bending, one should be ductile, and we just ramp to burst and then cool down. Never made it to 800 degrees C, we just wanted to study the burst characteristics. And the 20 percent ECR, we already

basically know from me messing around with the 2 percent with -- this is five minutes at 1204 degrees C versus what I showed you at the beginning, which was two minutes. So we pretty much know it's going to be brittle.

We plan to do four tests with the high burnup Zirc-4 cladding. There are certain things we need to do in the hot cell to get ready for that. And as I showed you, we just got our out-of-cell four-point bend apparatus going. We've got it benchmarked. We've tested a few LOCA samples. And we found out that somewhere between one second at 1204 degrees C, and two minutes is where the ductile to brittle transition is for a balloon and burst sample.

Just quickly, these are the four tests we ran. What's really interesting to me is except for two things there was almost no difference in the ballooning characteristics between the non-irradiated stuff that I showed you and the high burnup stuff. The difference is basically the shape of this opening. And you can see fuel in this. This is the one that was just ramped to burst, and then cooled without quench. This was ramped and held for five minutes, and then slow-cooled. This was partially quenched, and this was the good test that was the whole

sequence. And so we cut up this, and we cut up that. And what's really different about high burnup fuel versus fresh fuel in terms of cladding hopefully is the following diagram. Almost the following diagram. When we profile the oxygen content, convert it to ECR, and that's the blue. It does peak in the burst region as you expect. And it tapers off as you go to the one-sided oxidation region. Again, this is thin cladding, double-sided oxidation, and you're moving out towards single-sided oxidation of thicker cladding.

What's interesting is that the hydrogen peaks were not out here at the edge of the balloon. The hydrogen peaks were in closer to the burst region. It pretty much tells you your answer that -- well, I don't want to talk about criteria. As far as this level of ECR, and you'd have to do a test at a lower ECR, you not only knock the heck out of the ductility with oxygen, you really knocked it out with hydrogen. Because now you've got both oxygen and hydrogen essentially peaking in a very narrow region. So it's really highly unlikely that high burnup fuel would have any ductility, unless maybe you went to the one second at 1204 degrees C, maybe you can make it there. So that's a major difference between what Ralph showed

for non-irradiated. He showed these sharp spikes in hydrogen out beyond the balloon region, the neck region. Japanese results and our results are pretty consistent that the hydrogen peaks move in when you have fuel and a fuel cladding bond in our case, and fuel cladding bond in their case.

All right. I'm not going to --

CHAIRMAN POWERS: Do you understand why they move in?

DR. BILLONE: Well, it's not the fuel. Because the Japanese drill the fuel out. And it seems to occur when you have a fuel cladding bond like our 10 micron oxide bond, which is something -- the Japanese samples are more like 45 gigawatt days per metric ton, and it's marginal whether all the samples have a bond. And sometimes they see the peak move in. Sometimes they see the peak move out. And sometimes they see no peak. And in their case, they're right at that transition burnup where the fuel cladding bond is partially forming, or fully forming, or not formed at all. So based on their experience, and our experience being similar, it seems like when you have a -- and this is not an explanation. This is just like a coincidence. When you have the fuel cladding bond present, that peak seems to move in. And when you

don't have it, it seems to move out. So it's not an explanation, it's just -- so, for burnup below 40 gigawatt days per metric ton, you still expect the peaks at the neck. If you get above 45 you expect them to move in.

I am ending right here. Let me end this right here. I've shown you the data we've generated to date. I've indicated that we will be generating high burnup M5 and ZIRLO data. This will be defueled, small segments. We will be getting some fueled M5 through the EPRI Framatome contract, which will be available for the LOCA program. And we're juggling, based on what we can do in the hot cell, between the M5 and ZIRLO tests, and the LOCA integral test. So we basically -- if we get stalled someplace on one set of tests, we jump over to the other set. And that's the end of my long presentation. If you have any questions I'd be amazed.

CHAIRMAN POWERS: Any of the members have questions they'd like to pose at this time? Anyone in the audience want to interrogate Mike? Comment about what he has to say. You're truthfully free to do so if you do have questions.

DR. BILLONE: Don't leave me any more time, because I have another presentation I didn't

give. I was going to just skip it.

CHAIRMAN POWERS: Okay. Seeing none, you're done.

DR. BILLONE: Okay, thank you very much for your attention.

CHAIRMAN POWERS: Let's see. I guess Odelli you're going to talk to us?

MR. OZER: Yes.

CHAIRMAN POWERS: It may take Mike a little bit to get unwrapped up here.

DR. BILLONE: All right, Odelli. I'll leave it to you how to get out of this.

MR. OZER: Oh, thanks.

CHAIRMAN POWERS: We've got a resident geek here that can help you.

MR. OZER: Thank you, Mr. Chairman. And I appreciate this opportunity to present our views in front of the subcommittee. I'd like to preface my presentation by stating that our presentation materials, the handouts, were prepared before we had a chance to listen to the presentation this morning by Ralph in which he proposed how to address the new criteria. We find these criteria very interesting, and we certainly appreciate his attempt to try to minimize the impact on the industry. Certainly we

think that these criteria deserve some additional evaluation.

Now, as far as the Argonne program, we think that it's a very interesting, very good program. There's a lot of good data being generated in there. There are some important accomplishments, and we heard all of these earlier this morning, and in Mike's presentation. Essentially confirm the historic best estimate cladding oxidation kinetics. By the way, I'm going to try to cut my presentation rather short in order to give additional time to the presentations that will follow. What I would like to do is just identify maybe some of the concerns that we have, and I think they will be addressed in much more detail in the following presentation, and then their potential impact will be addressed by the third presentation that we have by Robert Montgomery.

But anyway, as far as the accomplishments of the ANL program, they confirmed the Hobson ring compression test results at 135 degrees Centigrade. They identified the reasons for the differences in the western niobium-based alloys versus the eastern ones. And we heard that this morning.

CHAIRMAN POWERS: What you're speaking here is the difference between M5 and E110?

MR. OZER: Exactly.

CHAIRMAN POWERS: Okay.

MR. OZER: Yes, there was this concern that all niobium-based alloys may be behaving as E110, and Argonne was able to demonstrate that that was not the case. And then they were able to identify the root causes of it.

CHAIRMAN POWERS: Yes, I think our actual concern is we didn't understand why, and we didn't know whether suddenly M5 might suddenly evolve over into being like E110 or something like that.

MR. OZER: Exactly.

CHAIRMAN POWERS: Having understood a little bit of why helps a lot.

MR. OZER: They found, as we just heard, the BWR cladding to balloon and burst for irradiated material, and a very similar way as for unirradiated material. The oxidation kinetics and embrittlement is similar. I do need to point out, however, that we still are waiting for the first integral PWR test to be conducted. So a lot of these assumptions are really being made on the basis of integral BWR tests.

Successful approximation of irradiation effects via pre-hydriding. This is very important for us because if this is true, then we can use pre-

hydriding as a surrogate for irradiation in testing material properties..

CHAIRMAN POWERS: And that seems like a terrific idea, but the criteria that have been advanced doesn't seem to include that kind of precaution.

MR. OZER: I think Ralph was saying that it's implicit in there, and that's why I think we need to study it some. I really couldn't comment on this at this time.

CHAIRMAN POWERS: But I think the idea is that you use this corrosion oxidation and subtract it from your criterion to account for this effect. But a much more direct way seems to be just what you have up there, is that you pre-hydride and oxidize the material.

MR. OZER: Exactly. And we feel that there is -- still we are evaluating the applicability of the current acceptance criteria to high burnup cladding materials, especially the advanced claddings. The issues are that it appears to us that schedule considerations are being given higher priority than trying to resolve some of the questions that have come up. And there has been some gaps in understanding that have been identified. The program is very

interesting, but it has also raised a lot of questions that we think need to be addressed.

The Fuel Reliability Program and the industry have been very active in the Argonne program, not only through participation and technical advisory groups, and providing both fresh and irradiated cladding specimens, as well as full length PWR and BWR irradiated rods. That's really a very expensive proposal for these tests. But in addition to that, we have continued monitoring the data and analyzing data from other international LOCA-related programs, such as Halden, CEA, JAERI, and some other European countries have produced LOCA-relevant data. This is not only to provide a reference to the ANL results, but also to evaluate the potential impact of other experimental conditions, such as heating/cooling rate differences, and alternate post-transient mechanical test results, and assessment of measured versus calculated ECR.

Briefly, our main concerns are due to the fact that NRC-RES has stated its intent to really begin the rulemaking process by September 2005. We are concerned that this schedule is too ambitious and premature. You know, these are just some samples of questions, and I'll be going through those in some

more detail later. But we feel that a complete and well understood database is really needed before we can move to rulemaking. And obviously, the current database is still evolving, and the limitations in the ANL program, we're not sure that they are being considered adequately. We didn't have really much of a chance to review the proposed correlation.

This is one slide that I would like to skip in view of what Ralph has presented this morning, because he has tried to address a lot of these concerns that we had in this area. But you know, as far as evaluating whether the data is mature enough to initiate rulemaking, we feel that a cladding ductility test obtained from the high burnup fuel rods in ANL program are not conclusive regarding the effects of irradiation of fuel survivability during a LOCA. Many of the observations are not consistent with the results obtained at overseas facilities, in particular the effect of heating and cooling rates, and quench or no quench on residual ductility. The ANL, there are some boundary conditions at the ANL. If you remember, the samples are being heated externally, usually they are small samples, and Mike mentioned the possibility of using flat versus curved plates during the compression tests. And the difficulty of running one-

sided tests versus two-sided tests.

CHAIRMAN POWERS: On the issue of that, my experience with the equivalent of ring compression tests is all based on ceramics, brittle materials to begin with, and it works very well for those. And that was my understanding, that it worked so well for basically brittle materials because you could interpret the results very easily. When you go to these ductile to brittle transitioning materials, is it a case of just not being able to interpret the ductility very well? Or is it --

MR. OZER: Well, we are concerned that when you squeeze the samples in a -- I'm sorry.

CHAIRMAN POWERS: Probably means the battery is dying.

MR. OZER: When you squeeze the samples, we would expect that the crack would initiate from the outside at the three o'clock and nine o'clock position, whereas most of the cracks in the Argonne tests initiated at the six o'clock or twelve o'clock position. Starting from the inside. So we are using really inside generated crack information for something that really will have an impact on the outside, because we're trying to specify criteria for the outside of the cladding. So we feel that there is

a little bit of an imbalance there. There's a concern in there. But I think, again, those may be addressed somewhat greater extent in the third presentation.

CHAIRMAN POWERS: Okay.

MR. OZER: Other questions are things like the protectiveness of the preexisting corrosion layer. And a big question is really the effect of hydrogen on the embrittlement. Are we taking that into account adequately? And you know, we have some doubts in that area. And we feel that these inconsistencies need to be resolved before rulemaking is initiated. We understand that Argonne is going to do some additional tests, but some of these tests will require also repetition at the outside organizations. And they have a completely different -- they certainly will not be ready by September.

And we don't really see a safety concern that would be driving this ambitious schedule at this point. There's no safety significant event that has been identified. In fact, the recent research performed both in U.S. and Japan have confirmed that fuel can survive the quench-related, you know, thermal shock quench events very easily up to and even under conditions greater than the current criteria, current limits specified in 10 C.F.R. 50.46. What we are

looking at is really after surviving quench, you know, how much ductility do we have by relying on the ring compression test.

CHAIRMAN POWERS: I got the impression that the staff had not identified a pressing safety significant issue either, that they were driven primarily by the fact that we're evolving 50.46 in general, but more for the clad generality problem. I mean, they're just trying to make life easy for all concerned here. I mean, I don't think -- am I correct that there was no safety issue drove you here?

MR. ELTAWILA: We agree with you. There is no safety issue, and as you articulated correctly, it's the 50.46 rulemaking process, you know, changes, and the M5 issue that really right now is still out of the regulations. So that's -- we try to make the rule to encompass that because either we will have to change the rule right now to add M5, or we change the rule to make it a performance-based. And maybe at the end I would like to say something about it.

CHAIRMAN POWERS: Yes. I mean, I think it's worthwhile to do that, but I think there's a community of belief here, and that we just have a historic opportunity to make one change.

MR. ELTAWILA: Absolutely, that's what it

is. I would like to make one clarification so nobody gets bent out of shape here. The Office of Research will provide the technical basis for rulemaking. Research does not do any rulemaking. So we provide it to NRR. They might not proceed with the rulemaking. Just for the record.

MR. OZER: But --

DR. MEYER: Could I add something, Dana?

CHAIRMAN POWERS: Please.

DR. MEYER: To Farouk's comment. And I don't disagree with anything he said. We haven't found any safety problems. But that isn't to say that the rule as written does not have some problems with it. For example, the rule as written now talks about a limit on oxidation. And as far as applying that rule goes, you could use a true value of oxidation for M5, and it wouldn't -- now my words are failing. M5 will lose its ductility before 17 percent true oxidation. But it will hold on as long or longer than the other alloys in a time temperature domain. So I would say that while -- I just don't want to leave the impression that the current rule is just fine.

CHAIRMAN POWERS: I think your articulated aspiration in your presentation, that we understand it's not oxide but oxygen in the metal that counts was

successfully accomplished, if not by you, certainly by the combination of you with Mike. So I understand your concern about the existing rule.

MR. OZER: Thank you. Yes, we do recognize that there is a unique opportunity to revise -- to update the criteria. But you know, the bottom line is if we're going to do it, let's do it right. Let's make sure that we have all the data that is necessary for this in hand or available.

We do have some concerns about, as you know, about demonstrating quench survivability using thermal quench tests as directly related to the fuel performance demands during a LOCA event, whereas ring compression tests measuring local properties are not indicative of the load-bearing capability of the fuel. The ring compression tests, we need to recognize that they result in more severe requirements than just quench survival.

So really, in conclusion, we feel that the current data set is insufficient to support the rulemaking at this time. Some exploratory research is still needed. There is no pressing safety significant event. And the industry's position is that the initiation of the rulemaking process should be delayed until some of these inconsistencies that we'd like to

address are resolved, and the validation experiments needed to support are completed consistently with a risk-informed regulation approach.

I just wanted to say that the following presentation before the presentation by Jean-Paul Mardon, Bert Dunn is going to make a couple of introductory statements to introduce Jean-Paul.

CHAIRMAN POWERS: Okay. If I could go back to your last point on Slide 7. The question's not about your point, but do you understand how the quench program in Germany interfaces with this kind of study? Are the two just not related, or is there something to be learned from that program?

MR. OZER: We have not followed the German program because of the assumption that it was more severe accident oriented. But the Japanese quench tests we feel are very relevant, where they do, you know, they heat up the rods, and then they clamp them, and then cool them, and determine whether they survive the quench under those conditions or not. Those are much more representative.

CHAIRMAN POWERS: I think the German program has not been so severe accident related. I think they're driving it toward severe accident relations. But I'm not -- maybe they're in some sort

of a transition regime between the two, but maybe there's something there that we could learn.

MR. OZER: I certainly think it's worth pursuing it, if the information's available to us.

CHAIRMAN POWERS: It's probably the most open program I can think of. I mean, they seem to be willing to put everything up on the website just as quick as they generate it. I mean, it's not like one of these consortium programs where things are embargoed and stuff like that. Maybe there's something there. And certainly the people working on it are extremely expert in oxidation of zirconium.

MR. OZER: We'll make a point of contacting them. Thank you.

MEMBER DENNING: I have a question on the viewgraph that you jumped over, and I think you jumped over it because you really hadn't been exposed earlier to what Ralph or RES was really proposing here. Would you agree that the major comments you've made there are probably overstated?

MR. OZER: That the major?

MEMBER DENNING: The comments, like the second and third. I realize you may not have had adequate time to decide whether you would change this viewgraph. But just from your exposure, do you really

believe the things that are said on the second and third ones now, or do you see this as a fundamental change?

MR. OZER: I think the attempt has been made to not have any fundamental changes, to make it as easy as possible. So if I had my opportunity, I would take this bullet out.

MEMBER DENNING: Yes. And do you think that it also --

MR. OZER: However, I think that there are some assumptions that are being made which if are proven not to be true, then we may need to really follow the hydrogen and oxygen content in materials that we would like to qualify much more closely than the current proposal would.

MEMBER DENNING: So you have concerns about the time-related criterion that's being proposed as to whether it will really be technically justifiable in the long term?

MR. OZER: Yes.

CHAIRMAN POWERS: As I understand -- I mean, what people say to me about this evolution of 50.46, and the choice of the break size, is no decision has been made yet. But I wondered if for those range of break sizes that are under

consideration now, do we have temperature signatures of the type that we currently use for large breaks?

MEMBER DENNING: You mean, like do we uncover the core to the extent implied there and have as high temperatures? And I truly don't know where that -

CHAIRMAN POWERS: I've just never seen calculations of the kind that we get for --

MEMBER SHACK: It depends on the plant. Some plants are definitely high temperatures, and high --

CHAIRMAN POWERS: I recognize that it will always be somewhat plant-specific, and somewhat scenario-specific. But the kind, you know, we all have kind of in mind what the temperature signature of a large break LOCA is if you allow me to draw it with a big enough paintbrush. And I don't know that I've ever seen any for these transition break size, you know, the candidates for transition break size.

MEMBER SHACK: I just don't think there's anything that you could say is typical.

MR. DUNN: This is Bert Dunn of Framatome. At the transition break sizes, most of the calculations show relatively low temperatures. But as you go into the smaller break arena where you're

challenging the high pressure injection system, you will have slower heat-up rates, some degree of plateau. It depends a little bit because you're working against decay heat drop-off versus a relatively fixed ECCS injection. And then you'll have a slow cool-down rate afterwards. And some plants do approach 2000 degrees. And they may push it a little bit higher after they get relief.

MEMBER DENNING: Do you have a feeling about the time regime? Ralph talked about a time regime, and maximum times. I'm not sure, were you here at the time?

MR. DUNN: Yes. I would expect that a half hour would be a fairly long time for even those small breaks I was talking about. Forty-five minutes for most of them would be fairly -- would cover them all fairly well. There are some unique breaks that we might have to look at associated with failure of the injection systems. So it's not 100 percent clear.

CHAIRMAN POWERS: Okay. Rosa?

MS. YANG: Yes. Rosa Yang. Just want to clarify one point reflecting to the question. I think the comment that was presented by Odelli was based on this embrittlement correlation that is part of your handout that was not discussed earlier. And that was

a correlation shared with us in May meeting, and that involved the, you know, parameters like the solubility of oxygen as a function of hydrogen, the diffusion, the kinetics, and all that. So from us, that's more of a fundamental change in the criteria.

CHAIRMAN POWERS: If it's the correlation that I think I know about, yes, you're right, it needed some real look.

MS. YANG: You need quite a lot of parameters.

CHAIRMAN POWERS: Any other questions for the speaker? Well, thanks Odelli. Bert, you're going to lead us out first, here?

MR. DUNN: Yes, I wanted to introduce the next speaker. Framatome's been interested in this program and cooperated with this program for a long time. We have participated in providing information, baseline information, some of our own data. And we want to cooperate with it.

I'd like to add two or I guess three things. Ralph a moment ago said that M5 would not reach 17 percent perhaps. I think he was primarily talking about the oxidation rate at 1000 degrees. I think as you get up to the higher temperatures, 1100, 1200 degrees, that cladding performs essentially the

same as the rest of them. That low oxidation rate at 1000 has been verified in three nations and four separate labs. So it's not just something that Argonne has found. I wanted to put that on the table.

Second thing I'd like to correct, Mike's expectations on the hydrogen pickup. And I'd like to use this to state that hydrogen pickup, or hydrogen content of cladding could very well be, and in fact probably is, alloy dependent. Framatome expects the stiff examples to come in at about 70 ppm hydrogen, even though they've been exposed to pretty -- I guess their exposure is 63 megawatt days per metric ton. So I just wanted to inject that in people's minds.

Jean-Paul is from Lyon, France. He's a metallurgist and has worked in this area quite a bit of time. He's going to talk about two main items. One is the effect of cool-down rate on the results of these types of tests and embrittlement functions. Second thing is some results on tests we've performed on pre-corroded cladding. We have a loop in which we store cladding in real conditions. This is not autoclave stuff. It's real water, and we build up corrosion on that. So I'll invite Jean-Paul to go ahead.

MR. MARDON: Okay, thank you. First of

all I would like to thank my French colleagues. We participate in this study. The experimental work was done in the civil CEA, the Saclay labs. And the scientific analysis has been done by these people. But also this work was done in close cooperation with EDF, and Manley and Nicolas Waeckel.

In this presentation I try to specifically address four main questions. The first one is, "Is the cooling rate impact the cladding residual ductility?" The answer is clearly yes. The second question is, "Is the mechanical test boundary conditions impact the residual ductility?" Again, the answer is clearly yes. My third question is, "Is the type of oxidation test - this means one-sided versus two-sided oxidation - impact the initiation and azimuthal location of cracks?" The answer, again, is yes. And the last question is, "Is the pre-corrosion layer impact the high temperature - high temperature steam oxidation kinetics?" The answer is yes. And "Is this pre-corrosion layer impact the residual ductility?" The answer is no.

Now I come back on the first question, what is the impact of the cooling scenario. And the question is is the cooling rate increasing or decreasing the cladding residual ductility? For that

we have to check several parameters, and mainly direct quench from the oxidation temperature, the effect of cooling rate on the emergency of quench. First of all, I would like to present you the several transient, thermal transients used in Number 2 to show the effect of the cooling scenarios on the cladding residual ductility.

The first one, I come back later in more details on each thermal transient. But the first one then is the thermal protocol used by the CEA in the device which is called Dezirox. And we use a fast heating rate with a heating rate between 50 to 25 degrees Celsius per second. What we reach is the oxidation temperature, what oxidation is found in isothermal conditions. And this oxidation is followed by a direct quench, with a very high cooling rate, which is greater than 1000 degrees Celsius per second.

The second side of thermal transient is a trial test, also performed in the Dezirox device, where we performed the same heating rate, the same oxidation in isothermal condition, followed by the slow cooling rate with a cooling rate around 0.4 degrees Celsius per second up to the temperature of quench of 800 degrees Celsius, where we perform the direct quench with the same cooling rate. You can

compare this thermal transient with the thermal transient used by Argonne, an unirradiated material as-fabricated or pre-irradiated material, which is the standard protocol of Argonne. We perform a slow heating rate, followed by the stop at 50 degrees Celsius, just below the oxidation temperature. This means that the oxidation is performing under anisothermal condition compared to isothermal condition of the Dezirox device, and followed by a slow cooling rate up to 800 degrees Celsius on the direct quench.

And the third trial performed in the Dezirox device is the same heating rate, same isothermal condition, followed by a slow cooling rate from the oxidation temperature up to the room temperature with a very low cooling rate. We stopped around 0.4 degrees Celsius, and we end at 0.25 degrees Celsius per second. And we can compare with the standard protocol proposed by Argonne for irradiated material.

DR. BILLONE: Excuse me, Jean-Paul. Just a small correction.

MR. MARDON: Yes.

DR. BILLONE: For the kind of test you're talking about, our heating rate is an order of

magnitude higher. It's 50 to 70 degrees C per second for the oxidation samples, down there. It's the LOCA integral tests that are 5 degrees C per second. But the data that you're going to be referring to is more like that. But it slows down to 1 to 2 degrees C per second.

MR. MARDON: But we don't discuss this --

DR. BILLONE: Okay.

MR. MARDON: -- on the oxidation. It's also another question. Is the heating rate impact the oxidation.

DR. BILLONE: You're going to focus on cooling.

MR. MARDON: But I discuss only cooling this condition, the cooling condition. In that case, it's 10 times higher in your condition than in the condition of the irradiated.

Now, this figure shows a typical record for temperature versus time for a test performed at 1200 degrees Celsius. I record the Dezirox sample is oxidized on one side, in isothermal condition. And I can show to you that there is no temperature overshoot when you reach the isothermal condition of oxidation. We have -- on the specimen we have already several thermocouples on the OD surface. And we have recorded

the temperature each degree of 5 milliseconds in order to plot the relation of the temperature versus time. In the Dezirox device, the sample is introduced in the furnace in temperature with a steam flow. This means that when we introduce the sample in the furnace, we have an articulation of the temperature up to 400 degrees Celsius. But from 400 degrees Celsius up to 1000 degrees Celsius the heating rate is about 55 degrees Celsius per second. And this temperature decreased progressively to 25 degrees Celsius in order to reach the temperature of 1200 degrees Celsius without overshoot of temperature. We have -- the Dezirox device has been carefully designed in order to obtain no overshoot when sent with very high heating rate conditions. And the stabilization of the temperature is reached after about 35 seconds. And in that case we are in isothermal condition. For example, at 1200 degrees Celsius, after around 50 - 55 seconds, we have reached 3 percent ECR CP.

Now, I'm going to present the direct quench cases. Again, I record we have the test heating rate, isothermal condition, and a direct quench. And in the standard protocol, Dezirox protocol, this transient is followed by several mechanical tests. Samples are taken from the 15

centimeter per second sample. On this oxidized sample, in the standard Dezirox protocol we perform normally a room temperature test, but for several loading mode, first one is ring compression test, second one is three-point bending test, and the third one is impact test. And also we can perform ring compression test and three-point bending test at 135 degrees Celsius and at various temperatures between room temperature and 135 degrees Celsius.

But today I will only discuss the ring compression test. As mentioned this morning by Mike, the loading mode is different from CEA that the one used by Argonne. On the upper part we have a plate, but on the lower part we have a plate cover, which is what I discuss later this point and mechanical notation. The sample is a 10 millimeter length sample, and the displacement rate used in the ring compression test is 0.5 millimeters by minute. Generally we test the failure rate of oxidation temperature when between 1000 and 1250 degrees Celsius, with four or five levels of measured ECR between 3 and 25 percent. And the tests on as-received material and pre-hydrided sample, pre-hydrided in order to simulate in-reactor corrosion.

The first result that we obtained on as-

received Zirc-4 and M5. And this material after direct quench exhibits a similar post-quench behavior at the three levels of temperature, 1000, 1100 degrees Celsius, and 1200 degrees Celsius. In red we have the ductile train as we see the Zirc-4 -- Zirc-4. And in blue the top 10 as we see in M5. We can observe the post-quench ductility decreases when the ECR increases, and that both materials exhibit exactly the same behavior.

CHAIRMAN POWERS: But you -- on that slide you seem to have the identical phenomenon at 1000 degrees Centigrade that Dr. Billone showed on his M5 tests at 1000 degrees. Something funky happens on your next to last sample. I mean, it's identical to yours, Mike. The 10 percent measured has worse properties than the 17 percent measured at 1000 degrees for M5.

MR. DUNN: Talking about the blue dot that's underneath the red triangles which you can hardly see?

DR. BILLONE: To me it's possible, but I don't understand M5 as well as I should because it's difficult to run the same test twice and get the same results. I'm not sure -- it's not breakaway oxidation, but there's not stable, consistent weight

gain the way there is with Zirc-4. I hadn't seen that point.

MR. MARDON: This one?

DR. BILLONE: The last one.

MR. MARDON: This one?

DR. BILLONE: Yes, that one I didn't remember. That test I'd never seen before. I'd just seen the others. For M5 you'd have to run several tests in that regime, if it's important, at 1000 degrees C. If it's important, because that's a very long time test.

MR. MARDON: This one, yes?

DR. BILLONE: That's a very long time. So I don't know what that data point means. I have no comment.

MEMBER DENNING: Well, let me ask a question since we do seem to have some reproduced test data here, particularly at 1100. That, I assume that those differences are a measure of the variability of a similar test. So that can we draw some conclusions that the error bend over on the 1000 would be similar?

MR. MARDON: Yes, probably. The scattering due to the test itself is the same at each level of temperature. But it's true that we have less result at 1000, and we'd have to increase the number

of tests in order to have a better view of this type. But probably we can observe the scattering is independent of the level of temperature.

The following question is the impact of pre-hydriding content on the cladding residual ductility, again in the direct quench conditions. In blue we have the ductile on plane of M5 at the three levels of temperatures between 1000 and 1200 for M5 pre-hydriding at 135 ppm, which is the volume of end of life content in M5 at regular. And for Zirc-4 we have testing the Zirc-4 pre-hydrided at 600 ppm, which corresponds to the end of life hydrogen content, again for the same three levels of temperature. We can observe that the M5 with the lower hydrogen content exhibits significantly higher residual ductility than the Zirc-4 with higher hydrogen content. Whatever the oxidation temperature.

And this result is to be related to the M5 lower in-service hydrogen pickup, because there is not only a lower content in the M5 in-service, but also there is lower hydrogen pickup, which is two times lower than the hydrogen pickup I showed for the Zirc-4. This means that the ductile/brittle transition versus hydrogen is located in the range of 30 ECR percent for M5. Why this ductile/brittle transition

is located around the 5 more or less percent for Zirc-4 with 600 ppm of hydrogen. And we have tried to explain why the Zirc-4 with the 600 ppm hydrogen is greater with an ECR value of 6 percent. On this sample of thermal oxidation at 1200 degrees Celsius, and the 6 percent ECR, we have determined the hydrogen distribution by a nuclear analysis in the CEA. And we have obtained this figure. We show the hydrogen content in the arbitrary unit because at that time the analysis is ongoing, versus the thickness of the materials.

On the right we start with zirconium. Zirconium we can observe there is no hydrogen in zirconium. Followed by the alpha layer, which exhibits also no hydrogen, and all the hydrogen is located in the prior beta phase with a very high level of hydrogen, which can explain the brittle behavior of this material for so low ECR values.

This slide summarizes the hydrogen impact on the hydrogen and oxygen migration and the residual ductility. First, the hydrogen increases solubility of oxygen into the prior beta layer, even at very low ECR values. Second, the hydrogen content increases the oxygen kinetics diffusion into the metal. The third, most of the hydrogen migrates into prior beta

layer. This is supported by the nuclear analysis performed by CEA. This means that we are probably -- the residual hydrogen content into prior beta layer could consequently becomes a more relevant key parameter than the initial hydrogen content in the metal in order to explain the brittle behavior. And the last one is higher is hydrogen content lower is ECR value corresponding to the ductile to brittle transition.

MEMBER SHACK: Have I got a wrong picture. You know, somehow I always picture, the pictures of the hydrides are always in that outer layer. And you're telling me it's really the other way in these tests.

MR. MARDON: This one?

MEMBER SHACK: When I see a micrograph, and I see hydrides, they'd all be on your right-hand side, which would tell me that the hydrogen's there. And yet you're telling me it's all over here in this test. Is there something different about this than there is in those pictures that I see?

DR. BILLONE: Yes, you're looking at pictures of the stuff coming out of the pool that's been exposed to 400 degrees C and it's all in the alpha phase. He's taken this up to 1000 degrees,

1100, 1200, changed phase, and the hydrogen is diffused.

MEMBER SHACK: It's driven it down.

DR. BILLONE: Has driven it down. The beta phase is hungry for hydrogen. That's why they're different.

MR. DUNN: So, have we got that clear? This is post-transient hydrogen distribution. The other one is normal operations. Okay. This is Bert Dunn.

MR. MARDON: Well, the next comparison is the effect of slow-cooling rate case. This case, we have tested some very low slow-cooling rates between 0.4 degrees Celsius up to 0.25 degrees Celsius. We compare with the Argonne results. This figure shows the typical cooling rate obtained on the samples, with the furnace turned off, but with steam during the cooling phase. We start at 1200 degrees Celsius and up to 800 degrees Celsius the cooling rate is all 0.4 or 0.5 degrees Celsius, and the third, this cooling rate decreases drastically in order to reach 0.1 degrees Celsius up to 21 degrees Celsius.

In this condition we perform ring compression tests at room temperature and at 135 degrees Celsius. In blue we have the data obtained in

the slow-cooling case, and in the pink we have the data obtained on the direct quench. This is for room temperature. You can observe that at room temperature there is only a slight effect or no effect of the slow-cooling rate on the residual ductility for ring compression tests performed at room temperature. Why ductility is strongly improved for slow-cooling rate when the ring compression test is performed at 135 degrees Celsius? Mainly for the low ECR value. These results have been obtained on Zirc-4 primarily at 600 ppm, and oxidized at 1200 degrees Celsius.

In order to try to explain why the slow-cooling rate induces an improvement of the ductility, we have performed some analyses, microstructure analyses. These two figures show the optical micrography in polarized light for the slow-cooling rate and direct quench. We can observe that we have two different microstructures with a uniform microstructure and a varied microstructure for the slow-cooling. And these samples conform to material with large ductility, slow-cooling rate, and this sample corresponds to zero ductility with the same gradient. Directly comparable.

Now, on these materials we have performed some scanning electron micrography. This is a

viewgraph from the direct quench. And you can observe that you have in this case a very homogenous structure with a very high microhardness around 400 - 450 figures, which is much higher than the critical hardness for Zirc-4, which is located around 350. This means that the ductility is higher than the critical value, and this material is fully brittle.

In comparison, for the slow-cooling rate with the same weight gain, we can observe that we have a more complex microstructure. In this microstructure we can observe some similar area with a hardness of the same level as this one in some part. But some other part exhibits very low microhardness, around 150 - 200, which is lower than the as-received materials. This means we have a very low oxygen content. And also, on this microstructure, we can observe some hydrides between the alpha layer. This means that the slow-cooling rate induces a two-phase structure with very strong alpha area embedded in the softer matrix, very soft matrix plus hydrides. This produces higher ductility than the homogeneous quenched structure. But mainly this is due to the redistribution of oxygen and hydrogen during the slow cooling. But in order to now understand this phenomena, additional investigations are needed, mainly at room temperature

and 135 degrees Celsius.

MEMBER DENNING: Excuse me. Do you pursue the slow-cooling rates just to get a better understanding of the processes that are going on? Or do you think that there's -- if you look at the prototypical accident, you wouldn't expect to have slow-cooling rates like this, would you? Wouldn't you expect to have much faster cooling rates, or am I misunderstanding the accident scenario as you understand it? Did you understand the question? In the real accident, would you ever expect to see slow-cooling rates of this nature?

MR. MARDON: Can you --

MR. DUNN: This is Bert Dunn. I'm probably a little bit better at answering it. I think the slow-cooling rates are probably not too off the mark for the small break regime. In large break, we would expect a little bit slower cooling rates than what's been advertised here, perhaps for the first 100 seconds post-peak until -- but 800 degrees Celsius is not a bad quench temperature. When we get to small breaks, we will expect a process that's more like a dry-out be-wet situation, and probably quenching would take place with a delta T of I would guess between 200 and 300 degrees. So it wouldn't be complete -- so we

don't have -- we probably shouldn't advertise this as being characteristic, but it's an explanation for differences between behaviors from one lab to another, and it illustrates that it can have an effect. Whether the slow-cooling geometry there sets in before we get to that quench in a small break, I don't think we know right now. I hope that answered.

MEMBER DENNING: Thanks.

CHAIRMAN POWERS: It's my impression that you look at this comparison more to understand whether an experimental convenience is really useful, and how to calibrate those slow-cooled results relative to things that might cool down faster. What Bert introduces is of course another important point, it's in small break LOCAs you don't see the same sort of quenching at all. And that's a good point.

MR. DUNN: That's correct. And as we move forward with one of the other activities of the NRC recently, an activity I support, which is to eliminate the larger breaks, this becomes more important.

CHAIRMAN POWERS: More important, yes. And what I had was actually completely backwards. It is the percolation through the soft matrix that's important, not the percolation through the hard matrix. Yes. It's just interesting.

MR. MARDON: Now the third case is slow-cooling followed by the direct quench cases. In that case we have performed again a test on the Zirc-4 at 1200 degrees Celsius with a cooling rate of 0.4 - 0.5 degrees Celsius up to 800 degrees Celsius. We are performing direct quench in water with a barrier cooling rate. As regards the -- we have performed tests on 1200 degrees Celsius and Zirc-4 with 600 ppm. And recognize that we have only for the previous result observed an effect of the slow-cooling rate at 135. And on this data, in view of the data obtained with the slow-cooling post-quench rate, we have the following results. At room temperature, the slow-cooling samples with quench is fully brittle, and in the same condition, the slow-cooling post-quench condition with testing at 135, the ductility is also very low. Displacement conclusion, we have a similar residual ductility for direct quench case of zero-zero case which is slow-cooling post-quench. This means in most cases we have a brittle material.

This slide summarizes conclusions on the impact of cooling rate on the residual ductility. The slow-cooling rate improved considerably the residual ductility at 135 degrees Celsius for pre-hydrided materials. This improvement is mainly due to a shift

of the ductile/brittle transition. In addition to cooling rate, this shift in the ductile/brittle transition depends also on other parameters, such as hydrogen content, hydrogen and oxygen redistribution, oxidation temperature, ECR value, and may be alloy. This means it's very complicated.

Before some effect of this parameter can be opposite, and the following figure shows the expected impact of the different parameters. On this figure that is ductility versus test temperature. We have brittle behavior on the top, ductile behavior. And for example, in black you have the typical curve obtained for Zirc-4, as we see in material testing around 1200 - 1250 degrees Celsius, where the ductile/brittle transition occurs between 60 and 100 degrees Celsius. And now, we expect that when we increase the hydrogen content, when you increase the ECR value, or when you increase the oxidation temperature, this transition is shifted towards the right, this temperature. This means that the material is not brittle in this condition. And there is only one parameter we can improve this ductile/brittle transition. This is the impact of the decreasing of the cooling rate. We shift towards the lower temperature, the ductile/brittle transition. This

means that same form of material with hydrogen content, and the testing with the low cooling rate we can have a ductile behavior at 135 degrees Celsius. But you can observe it's very complex, and we are to take in account several parameters. This means that additional mechanical tests for various test temperature ranging from 20 degrees Celsius up to 100 degrees Celsius, and also additional detailed microstructural investigations are needed to support this hypothesis, which the mechanicals results is a function of the cooling scenario, mainly for the pre-hydrided materials. Several tests are in progress in CEA.

Now I will intend to discuss a second question, which is, is the test boundary condition impact the residual ductility, the initiation, and the azimuthal location of the cracks in ring compression test. First, I compare the Argonne loading mode for the samples, compare with the CEA/Dezirox device. For me, both loading modes are very similar due to the fact that in the CEA/Dezirox device, the sample is free to expanse due to a very large radius of curvature of the bottom part of the device. This means it's quite similar to the Argonne.

The second question, in what is the impact

of the post-LOCA at this temperature between room temperature and 135. And the other question is, depending on the test boundary condition and the type of oxidation, one-sided versus two-sided, what is the impact on the azimuthal location of the crack initiation. And the second, the impact of the crack initiation may occur maybe on the ID surface or the OD surface.

In terms of impact of the test temperature between room temperature versus 135. In that case we are testing Zirc-4, as-received Zirc-4 oxidized at 1200 degrees Celsius, followed by a direct quench. In red you have the ductile obtained at room temperature, and in green the ductile obtained at 135 degrees Celsius. You can observe that for the fully ductile or for the fully brittle material, there is no impact of the test temperature. This is an important result. And the second, the ductility is improved, but only in the intermediate value of ECR, when you increase the temperature from room temperature up to 135.

We have performed the same comparison for M5 and Zirc-4 pre-hydrided, M5 pre-hydrided at 130 ppm, and Zirc-4 pre-hydrided at 600 ppm. This material have been oxidized at 1200 degrees Celsius, followed by a direct quench. We observe that when we

test the Zirc-4 pre-hydrided 600 ppm at room temperature or 135, the Zirc-4 remains brittle. Same rate increases the test temperature, because the scale is very large. The percent in this region is about 0.5 percent. Why for M5 the ductility is improved? Also for the intermediate value due to the very low hydrogen content in the metal. We compare the solid part in blue for the room temperature test, and in the open sample in blue is obtained at 135 degrees Celsius. In comparison with the same scale on this axis, we can observe that the Zirc-4 600 room temperature and 135 is located on the X axis. This means that material is brittle. Y is the M5 remains obtained.

The other question is where the crack is initiated, where is located the rupture during a ring compression test. For that we have performed in the Dezirox device some oxidation one-sided condition, and other oxidation in two-sided condition. These samples have been oxidized at 1200 degrees Celsius in one side. This is Zirc-4 600 ppm with 3.59 milligrams per square centimeters during quench. You can observe that the cracks is located at three or nine o'clock, and the initiation of the cracks is located on the OD surface. You can observe that this is a view of the

curved plate on the bottom part. You can observe that the sample is free to expanse in this test.

And now we compare with the same materials Zirc-4 600 ppm, direct quench, but with the same weight gain, in the same oxidation time, in two-sided. In that case, the cracks is located at six or twelve o'clock, and the initiation in that case is on the ID surface. We have for the two-sided oxidation, we have an example which shows that the rupture is located -- the crack is located at six and twelve, for a higher weight gain for Zirc-4 600 and direct quench. This means that the initiation and the location of cracks depends on the oxidation protocol, one-sided versus two-sided, and not of the loading mode. If you compare Argonne and CEA, this is independent of the loading mode, but depends strongly on the oxidation protocol.

Now the following question is what is the impact of the pre-corrosion layer on the high temperature oxidation kinetics and the residual ductility. For that we have a testing at 1200 degrees Celsius, 55 seconds. This means about 3 percent ECR. The Zirc-4 cladding were pre-oxidized in Reggae loop, which is a CEA loop, which has a condition very close to the pre-hydrided environment, except as a neutron

flux. And this material was characterized by zirconium thickness on the OD surface of 15 microns, 5 microns on the ID surface, and about 150 - 200 ppm of hydrogen in the metal.

This figure shows an optical viewgraph of the samples after oxidation this time. You can observe on the ID surface that all the ID corrosion, pre-corrosion layer has been transformed in alpha. There is no more oxide, pre-corrosion oxide. Why? On the OD surface, we can observe that from the outer part of the material we have the remaining pre-corrosion layer. Just under you have the columnar zirconium, which is obtained in high temperature steam oxidation. The typical brittle alpha layer and the prior beta phase.

We have characterized these materials. We have in order to compare the effect of the pre-oxidation, we have compared Zirc-4 as-received materials, which has been tested at the same temperature with the same time, 50 seconds, in order to compare the different parameter. The first conclusion is that the pre-transient corrosion layer slowed down the steam oxidation. We can observe that the weight gain is much lower for the pre-oxidized sample than the fresh materials. This slow-down is

effective up to the time for which the oxygen flux settles across the preexisting corrosion layer. But this hypothesis supposes that the corrosion layer is not cracked under LOCA conditions for the balloon or deformed area. It's probably not the case. This is the first conclusion.

The second conclusion. At the early beginning of the steam oxidation, the pre-transient corrosion layer begins to transform by diffusion of oxygen into the metallic sublayers, which are the alpha and the beta layers. And thus its thickness -- and first its thickness first decreases. This is observed on this value. We start with 15 microns of oxidation, and the remaining corrosion is only 5.4 microns. This reaction is followed by a growth at the interface metal/oxide of a steam columnar zirconium layer, which is around 7.2 microns compared to the 23 microns obtained for the fresh materials. But the more important result is that all these results induce the same alpha layer thickness for the fresh materials, 20 microns, compared to about 20 microns for the pre-oxidized Zirc-4 materials. This means what is the impact of this situation on the residual ductility? There is no significant impact of the pre-corrosion layer on the residual ductility due to the

fact that the alpha layer has the same thickness for fresh material than for pre-oxidized materials.

MEMBER DENNING: Can we discuss that a little more, because I think that is an important question. I want to make sure that I understand what you're saying. You're saying that the existence of the pre-corrosion layer didn't affect the ultimate thickness of the oxide layer?

MR. MARDON: This one?

MEMBER DENNING: The final -- well, it's your -- the alpha is unaffected.

MR. MARDON: We start with --

MEMBER DENNING: Yes. Right.

MR. MARDON: Oh, this. The layer of 15 microns. This layer, pre-transient layer slows down the steam oxidation because we have a very low weight gain compared to the weight gain obtained on the fresh materials. This phenomena occurs up to the time for which oxygen flux settles across the preexisting corrosion layer. This means that the corrosion layer remaining is 5.4 microns compared to the 15 microns. But this difference between 15 microns and 5 microns is transformed into alpha layer. The oxygen coming from the zirconium pre-corrosion layer is transformed in alpha layer. And after, when the flux of oxygen

settled across the preexisting corrosion layer, we have the typical high temperature oxidation which occurs, and in that case you obtain 7.2 microns of columnar zirconium coming from steam oxidation, compared to the 23 microns obtained, result of pre-corrosion. But in fact the oxygen coming from the pre-corrosion layer plus the steam oxidation gives exactly the same thickness of the alpha layer for the fresh material than for the pre-oxidized material. And this is the important parameter for the ductility of the materials because this means that there is no effect of the pre-corrosion layer on the ductility of the materials. And I show you this result on the following graph.

MEMBER SHACK: But couldn't you have an effect on the prior beta layer? That is, are you sure it had -- the prior beta layer has the same oxygen-hydrogen content, since it's the one that gives you the toughness, suppose you affected its properties? You know, I'm not sure that the conclusion that the alpha layer is the same thickness leads me to the conclusion there's no impact on ductility if it affects the prior beta layer.

MR. MARDON: Yes, there probably are some results, some tests that are important, but we have

not at that time measured the hardness of the beta layer. But according to the results obtained in this slide, we show that the pre-oxidized samples with the 150 ppm at 135 degrees Celsius up to room temperature are located close to the result obtained on as-received materials, show clearly that the ductility of the pre-oxidized samples from room temperature to 100 is very similar to the one obtained on as-received material, or as-received material plus the pre-hydriding of 150 ppms. This means that the beta layer is probably the same.

MEMBER SHACK: Is your weight gain here the total weight gain, or just the weight gain in the transient?

MR. MARDON: This is the weight gain in the transient. Yes, the transient. Yes, because the weight is the same before the test and after the test. And this value is only the weight gain during the high temperature transient. This is clear. And this is the result. And we have exactly the same result on M5. We have also tested M5 at 1200 degrees Celsius, similar time 60 seconds, in order to reach about the same ECR. This material was also oxidized in the Reggae loop, similar OD thickness, 15 microns, similar ID thickness, but less hydrogen in the material due to

the fact that the hydrogen pickup of M5 is two times slower than the one on Zirc-4. This is an important result.

And also we have compared with fresh materials, with the same times. Again, you can observe the corrosion -- the weight gain is lower for the pre-oxidized compared to the fresh material. We obtain also a lower columnar, remaining about 10 microns compared to 15, the total zirconium thickness. And again, the thickness of the alpha layer is quite similar, and the beta layer is probably the same hardness. You have exactly the same conclusions for both materials. They show clearly that there is no impact of the pre-corrosion on the ductility of the material.

This slide summarizes my presentation. We have mainly four main observations based on CEA data. The first one is hydrogen content is a first order parameter on the post-quench ductility. The second one, that -- probably the main result, there is a major effect of a slow-cooling rate combined with a mechanical test performed at 135 degrees Celsius on the residual ductility of pre-hydrided materials. The third conclusion is pre-transient corrosion layer slows down the high temperature steam oxidation, but

has no impact on the alpha layer thickness, and thus no significant influence on the residual ductility for the same oxidation temperature and the same expected ECR value. And the fourth observation is both initiation and location of ring failure depend on oxidation protocol, one-sided versus two-sided, and not on loading mode. To compare Argonne and CEA.

This is -- we propose two recommendations. The first one is to use the same cooling rate and the same mechanical test temperature for testing unirradiated and irradiated materials for two main reasons. In order to avoid to be located in the transient brittle zone associated with natural large scattering, when you have mainly high hydrogen content. And if different protocols are used for irradiated or non-irradiated materials, we have to take into account a correction. This is the cooling rate, the hydrogen content, the protocol of oxidation, and even other parameters. And the second recommendation is additional data are needed to confirm the impact of pre-transient corrosion layer on the residual ductility of cladding with higher hydrogen content than the sample test up to now. That's all.

MEMBER SHACK: Again, wouldn't your 150 to

200 ppm be a fairly low hydrogen level for a high burnup Zircaloy-4? The Robinson is 800, right?

MR. MARDON: Yes.

MR. DUNN: Bert Dunn. I think that's true. That is relatively low. The corrosion layer was relatively low for -- you wouldn't expect --

MEMBER SHACK: So you wouldn't expect a bigger flow of the pre-corrosion in this case, if the pre-corrosion is meant to incorporate the hydrogen.

MR. DUNN: That depends whether it's linear or not, but yes. That's one of the reasons for asking for more data.

DR. BILLONE: Excuse me, Jean-Paul? Your last point, just to support it even more, could you go back two more slides to your graph of ductility for pre-corrosion? One more. That was the one right there. Really, the limitations on your test machine are in the 50 to 60 percent range. So you're just -- I don't think you can use those data to support your point that there's no effect on embrittlement or ductility of one case versus the other because you just reach your ductility limit of your machine. You'd have to be at a lower -- you'd have to be at a higher weight gain to make that point. I don't think you can displace more than 60 percent, which is about

6 millimeters, plus bring back about 7 millimeters. I think you've reached your limit.

MR. MARDON: Yes, you are probably close to the limit of the --

DR. BILLONE: But if you've reached your limit, you can't say that there's --

MR. MARDON: But we are not at the limit.

DR. BILLONE: In that case there.

MR. MARDON: For this one too, also, the limit, this means that maybe the ductility is much higher in the first case, for as-received and pre-hydrided.

DR. BILLONE: It would be a stronger point if you were at 10 percent weight gain with the pre-oxidized. If you were a little -- 20 to 30 percent, even, displacement you could make a stronger point. But it's like when two samples are completely brittle you can't tell anything about them. When two samples are completely ductile, you can't really compare them very well. So I support your last point, but more work is --

DR. BILLONE: I think this was a load -- regarding load versus displacement, we can say that this material is ductile. It may be that we are close to the limit of the device, but in a way, the record

load versus displacement is typical of oxide material.

DR. BILLONE: I believe your conclusion, I just don't believe that those data points support it.

MR. MARDON: Maybe it's not the right value. Maybe the ductility is much higher in that case. But it's the same conclusion for this point.

DR. BILLONE: Okay.

MR. MARDON: According to the load versus displacement figure, we can say clearly that this material is ductile.

MEMBER SHACK: Just another question too, on this one where you have the initiation on the OD at six and twelve, and the other one where you get the initiation on the OD at three and nine. Since none of these tests are prototypical, you know, what I'd sort of like to conclude is that none of these variations in the testing would lead me to a different conclusion whether this material was brittle or not brittle. Do I really get to a different conclusion? Even though I have these differences, I would still like to think that when I'm all done, I get to the same conclusion that a weight gain, or an EPR of something, or an ECR of something percent is ductile or not ductile. I'd be very worried if my conclusion was so dependent on

my test protocol, since neither protocol really represents the real world very well.

MR. MARDON: If we remember the previous presentation of Mike, you see that at two-sided, we expected the location of the cracks at six and twelve, no? According to the two-sided oxidation.

DR. BILLONE: Basically, I have some graphs I want to show when I have an opportunity to get back up there where we've been able to compare our results to CEA results, which included a variety of things. It's basically as-received material. And I don't see any difference in results for as-received material.

MEMBER SHACK: Brittle material is brittle material, and ductile material is ductile material?

DR. BILLONE: Yes.

MR. MARDON: Between these two situations it's very clear, it depends on how you use the ductile, but ductile is ductile, brittle is brittle. But it's right, we expect for one-sided and this type of loading, we expect that we have in that case we have only zirconium on the OD surface. The initiation is located on OD. And the location in here. And for two-sided, we have an idea of initiation on the inner surface. It's expected according to the load.

MR. RASHID: I have a comment on that.

CHAIRMAN POWERS: Sure, Joe.

MR. RASHID: Joe Rashid here. I just want to make a point regarding this observation, that the assessments are always highest at the -- below the load point. And therefore, if you are using correlation that uses the local conditions for either one of these tests, you will be exaggerating the effect of these local parameters, because of the failure occurring in the ID. In other words, for equal conditions OD and ID, failure would be driven to the ID first.

MEMBER SHACK: Okay, now in your terminology, is ductility a local parameter?

MR. RASHID: Well, if the correlation uses, for example, hydrogen content, local hydrogen content, then that would be affected by these two tests.

MR. DUNN: This is Bert Dunn. I think one of the things we're looking for is to have some peer reaction to the differences between an inside initiated crack which we don't expect to happen in the reactor, and an OD initiated crack, where we expect the most embrittlement and highest alpha region to go. We'd just like to be able to move from one to the

other with some degree of confidence. So, all tests.

DR. BILLONE: Bert, you're not really testing real materials. You're testing bare cladding, basically, that's been pre-hydrided. You will with real cladding have a source of oxygen on the ID. It's not steam, but you will have some embrittlement on the ID and a major embrittlement on the OD. So that's not what we're really doing in these tests. We're not trying to reproduce exactly what happens in the reactor for a known mode. We're just defining a screening test. And if the picture on the left leads to 19 percent ECR as a transition, and the picture on the right leads to 18.5, I'd call it a wash as a screening test.

MR. DUNN: So would I. I would agree with that. But right now I'll accept your word that you've checked it and there's no difference.

DR. BILLONE: Well, I've checked it to the extent of the data that you've published, and I haven't checked it to all of the data generated. But I'll show just a couple of graphs where I have compared the two data sets.

MR. DUNN: Okay.

DR. BILLONE: I think what's more profound than this picture is the results shown for the impacts

on cooling rates, and quench temperatures. Far wash out anything that you're looking at here. Because there you saw dramatic changes in samples that were totally brittle that became, you know, reasonably ductile. So that's the biggest difference in test protocol. It's not going to be one-sided versus two-sided, or the test method, it's going to be the cooling method. The cooling conditions. That's what I got out of the presentation.

MR. DUNN: Yes, and we've had discussions on whether path is important before. And you know, with the differences in the cooling rate we have at least discovered one effect where path is important.

MEMBER KRESS: But your protocol in doing the cool-down rate the way you did it would result in a conservative screen, would it not? It would tell you it was brittle when actually it was more ductile.

DR. BILLONE: I don't think the issue of how we run the ring test is important. I think the issue -- the most conservative thing you can do is to quench directly from your oxidation temperature at 1000 degrees per second. That's mainly the CEA test. That's the kind of conservative we can't live with because it essentially gives you zero ductility after 50 seconds at 1204 degrees C.

MEMBER KRESS: That's too conservative.

DR. BILLONE: Too conservative. And it doesn't represent anything that happens in the reactor. So it's fine to do it, but if you can't live with the answer, then you try to get something a little closer to what's really happening.

MR. MARDON: Direct quench is a more severe condition. Slow cooling plus direct quench is the same. It is also severe. There is only one case which is different. This is the slow-cooling rate, which is when you combine with high temperature in compression test which is ductile. But for us, the direct quench or slow-cooling plus direct quench is similar.

DR. BILLONE: Jean-Paul, just let me ask you a question about your opinion. As far as oxygen redistribution during slow-cooling, oxygen has to diffuse a short distance to redistribute. And the diffusion rate is very high at 1200 degrees C, and it falls off 800 degrees C. You've essentially turned everything into alpha. Why would there be oxygen redistribution below 800 degrees C? Do you think there would be? Whether you've quenched or slow cooled?

MR. MARDON: First, I think as you that

if you perform the slow-cooling plus the quench you have time to redistribute the oxygen but it's not the case. It's very surprising.

DR. BILLONE: I know it doesn't make a difference for the as-received material, but with material with hydrogen it seems to make a big difference. Okay.

MR. MARDON: But at that time you have not performed a microstructurally determination of direct quench on very slow cooling, and we have to perform examination of the slow cooling with quench in order to achieve that. Is this microstructure is close to the direct quench microstructure in the slow cooling. But according to the mechanical result, probably it's close to the direct quench microstructure.

MR. WAECKEL: Jean-Paul, you can go back to your Slide Number 20.

MR. MARDON: Slide?

MR. WAECKEL: 20. That shows very clearly there is not a big difference between direct quench and slow-cooling plus quench. And the point we are going to make here is only to show that the slow-cooling from the top is not conservative enough.

DR. BILLONE: I'm sorry, Nicolas. Could you say that again? Slow-cooling from.

MR. WAECKEL: The slow-cooling is less demanding --

DR. BILLONE: Yes.

MR. WAECKEL: -- than any other cooling down scenario, both direct quench and quench plus -- slow-cooling plus quench.

DR. BILLONE: Okay.

MR. WAECKEL: Slide Number 20.

MR. MARDON: This one?

MR. WAECKEL: And my name is Nicolas Waeckel from EDF.

MR. MARDON: This one?

DR. BILLONE: Right.

MR. MARDON: I know we have to change the microstructure of these samples, but probably it's very close to this one, and far to this one microstructure, which is very typical with very soft area, with on this lower than the other steam material which is very unbelievable. It was very high on this, similar to the hardness of the direct quench material. And some hydrides which are probably located in the ductile area, this means they have no impact on the ductility. So probably in that case, saying we have some hydrides in the materials, the ductility is improved.

DR. BILLONE: Is there one more factor to this? Because what I call slow-cooling is more like 10 degrees C per second, which isn't that slow, and then we quench at 800. And some of your tests were run at 0.5 degrees C to 1 degrees C per second.

MR. MARDON: But saying with very low slow-cooling, this means the time between 1200 and 800 is very long.

DR. BILLONE: Right.

MR. MARDON: So for a very long time we have, you know the rest, we have --

DR. BILLONE: Oh, I see. I see what you're saying.

MR. MARDON: Greater behavior. We have more time than -- because you do the very slow-cooling rate of 0.4, 0.25. Same with the very long time in this range of temperature between 1200 and 800 degrees Celsius. The material is brittle.

MR. DUNN: Jean-Paul, relative to the phase change kinetics, I hate to ask -- I probably shouldn't go here.

MR. MARDON: Yes?

MR. DUNN: I already started. I guess I'm going to go there. Relative to phase change kinetics --

MR. MARDON: For zirconium?

MR. DUNN: Yes. Are we completely through them by the time we do our quench here? I just, I don't remember the numbers that well.

MR. MARDON: Due to the fact that it's pre-hydrided Zirc-4 600 ppm, the transition alpha to alpha, beta is very low because --

MR. DUNN: Very low in temperature?

MR. MARDON: Yes.

MR. DUNN: So perhaps even with the quench, the slow-cooling plus quench --

MR. MARDON: Maybe --

MR. DUNN: -- we're still quenching in the middle of the phase change?

MR. MARDON: Probably. What slide?

MR. DUNN: That would be a difference between the two types of accidents we might want to talk about.

MR. MARDON: Yes, probably.

CHAIRMAN POWERS: Any other questions for the speaker? In that case we will take a break until 4:05.

(Whereupon, the foregoing matter went off the record at 3:50 p.m. and went back on the record at 4:05 p.m.)

CHAIRMAN POWERS: We have a presentation now by Robbie Montgomery on his evaluation of experimental results; given, of course, that he can unwind the computer, which I'm in no position to help you.

MR. MONTGOMERY: I think I can handle it.

CHAIRMAN POWERS: You young guys, you know these things.

MR. MONTGOMERY: Just like Grand Theft Auto, right? I guess I need to put this thing on.

Good afternoon. My name is Robert Montgomery. I'd like to thank the sub-committee for the opportunity to come here today and make a presentation on kind of an industry evaluation of the Argonne experimental programs and a summary of some of the key questions that we have after taking a look at this data.

As a consultant to EPRI, we've been asked, myself and Anna Tech have been asked to take a look at the Argonne data and support the experimental program from a technical point of view. And one of our mandates has been to take the Argonne data that's been generated related to post-quench ductility and try to compare it to other data sources that are out there that are available that try to do the same thing. So

one of my objectives of today's presentation is to just compare what other people have done in this area.

There have been tests done not only at Argonne, but at CEA, which you heard a little bit about today already. The Czech Republic and Hungary have all tried to do tests that are similar to what has been done in the Argonne program. And these programs have all been trying to evaluate in some way the impact of irradiation on post-quench ductility.

When I get done with that, I will also try to identify some of the questions that have been raised by the current data on post-quench ductility that's come out of the Argonne program and other programs. We've already kind of hinted at that today in Odelli's presentation, and also in Jean-Paul's presentation, and those are what are the important parameters in the test, the oxidation condition, should it be one-sided or two-sided oxidation, the heating rates and cooling rates, and the loading conditions, should they be flat plates or a combination of curved and flat plates? And also, I won't dwell on this point too much, but the second area of question is how do these results from these specialized post-quench ductility tests, and I've kind of labeled them specialized because they're very local

effect tests, how do they relate to conditions for a hypothetical loss of coolant accident? I'm not going to answer any of these questions. I'm just going to raise a few of them.

All right. Ring Compression Tests have been done not only at Argonne, but at three other organizations. We see that basically they span almost all the types of different alloys that are out there. The Argonne program, you've heard, has gone through most of the more popular alloys out there, as well as prehydrided Zirc-4 and irradiated Zirc-4.

In Hungary they've also looked at as-received material and prehydrided material to simulate the effects of irradiation, Zirc-4 and E110 primarily. In the Czech Republic, they've also looked at as-received and prehydrided material. They've look at Zirc-4, several different variants of Zirc-4, Zirlo, and some Zirc 1 percent nairobium material. And finally, you heard a little bit about the French program that's really been looking at as-received, prehydrided, and now recently some pre-corroded Zirc-4 and M5 material.

I have a few slides here. I didn't know how much everybody in this room would be familiar with ring compression tests, so I just thought I would show

a couple of diagrams here. The ring compression tests typically use short rings on the order of 8 millimeters, some use 10, some use 6, it varies a little bit from lab to lab. And they're run at both room temperature - I would say most of the data that's been generated outside of the U.S. has been done on room temperature data, room temperature tests. The U.S. has focused primarily, or Mike has focused primarily on the 135 degree C test, and NCA has recently started to look at that temperature, as well.

We have data on samples that have been oxidized anywhere between 800 degrees C, rather low temperature, all the way up to 1200 degrees C. And primarily what we're looking at are going to be the low displacement curves coming from the Instron machine with a variety of different grip devices. And I've kind of illustrated here what we're talking about, is we have a loading platen here and a seat or a plate here that the sample rests on. Sometimes these are curved, sometimes these are flat, and we're applying a load, and we're watching the sample deform, and we get a load versus displacement curve. And then that curve is evaluated, and there's different ways to evaluate it to determine transition between ductile and brittle behavior, or a definition of what's a

ductile material, and a definition of what a brittle material is.

Effectively there are four ways that are currently being used to evaluate the low displacement curves. The first one is residual plastic strain or the offset strain. You've seen that. That's the standard method that Argonne uses. That's represented here by these black lines where you have kind of a Hooke's Law line, a line representing the linear portion, and then at the maximum load position, which represents the first fracture point, there's a line drawn parallel, and the difference between those two lines represents the plastic strain, residual plastic strain.

We also have total displacement as one option, and that's where the displacement out to the first crack, or sometimes out to the last crack is used as a parameter, and that would represent this by this green line here, and this green point there.

Mike also referred briefly to permanent strain. Permanent strain is the measurement of the diameter before the test, before the loading, and then a measurement of the diameter after the test, and the difference between those. And then sometimes it would be normalized to the initial diameter here, to get in

terms of a percentage value.

And then finally, our foreign colleagues in the Eastern Block countries, Hungary and Czech Republic, have looked at fracture energy, and that would be representative of the area under the low deflection curve up to the first fracture.

In addition to the types of methods used, there also need to be criteria defined that say okay, so much offset strain represents a ductile material, and so much represents a brittle material, and so I have here a list of the different criteria that have been used by the different organizations in defining the threshold between brittle and ductile.

We have in the Argonne program - Mike, actually I think he used 2 percent today, but it varied between 2 and 3 percent offset strain, or greater than 1 percent permanent strain represents a ductile material. In the Hungarian work, they primarily focus on the fracture energy, and they use a number like 50 millijoules per millimeter as their metric between ductile and brittle material, so greater than 50 millijoules per millimeter fracture energy would be a ductile material.

The Czech Republic generally has used an offset strain of zero, better than zero represents

ductile material, and those that have no offset strain are brittle material. And lastly, in the CAEDF program several approaches are being currently looked at. They're looking at the fracture energy, they're looking at offset strain, and they're also looking at photography features and micro hardness as ways to differentiate between ductile and brittle material. So that's another variable that's in the mix when we try to compare some of these data points together.

This is an example of some low deflection curves developed in the ring compression test performed by KI, the Hungarian organization, research organization, and we see here that the curves that are ductile material with fracture energies greater than 50 millijoules per millimeter are shown here in blue. And typically, they actually give a minimum fracture energy of something like 84 millijoules per millimeter. The red curves are all brittle materials, and they typically get fracture energies less than 50 millijoules per millimeter, and with a maximum of something on the order of about 48. And you can see here that they typically all kind of break fracture here within fairly small displacements.

Now how do you use this material is shown on this slide, where we have these oxidized samples

that have been ring compression tested plotted here on a long scale of time at oxidization versus one over the temperature here for the oxidation temperature, and this is kind of a classical way to look at some of this data. This was the approach that was used by Hobson and some people back in the original criteria development days for the LOCA embrittlement criteria. And we see for zirc-4 that they have defined the material here using a fracture energy where we have the solid symbols represent ductile material, and the open symbols represent failed or brittle material. Those less than 50 millijoules per millimeter, and these would be greater than 50 millijoules per millimeter. And we draw lines on -- typically lines are drawn and we find an empirical correlation that represents the transition between ductile material and brittle material.

And I've indicated on here one line I've added here. This is the old Baker-Just correlation giving you a 17 percent correlation, and this was all as-received material. And you can see that it represents fairly well the transition zone between the brittle material and the ductile material as a function of the temperature and the time. That's kind of an example of how some of the data is put together.

I apologize for the quality of this graph but what I'm showing here is some data from the Czech Republic where we have residual ductility plotted here versus either ECR, and this would be calculated ECR, and then R as a function of hydrogen content. And I realize this is a fairly busy graph, and what you see here, they have run tests between 800 degrees C, all the way up to 1200 degrees C. The solid symbols represent as-received material, and the open symbols represent material that has been pre-oxidized, and hence, prehydrided in some fashion. The hydrogen contents range anywhere from the as-received would be near zero, out to as high as 1500 ppm, or over 1000 ppm of hydrogen for some of these samples.

And what we see, if you kind of boil it down, is that for samples above 1000 degrees which are represented here in green, so that would be the oranges and the reddish colored samples, they typically have fairly low residual ductilities, with any amount of oxidation above zero. And with hydrogen contents typically above 300 ppm or so, you begin to see a fairly significant impact on the residual ductility. So these early data sets that were available, I would say a couple of years ago, a year and a half ago, are already indicating that hydrogen

has a pretty dominant role on the residual ductility capability as defined by these data here.

So that's kind of my quick summary of what's been done elsewhere. Now what I'd like to do, and this slide kind of goes through that summary, but what I'd like to do now is give a little bit more detail in comparing the Argonne results with some of the CEA data, the more current data that's been generated. But before I do that, I'd just like to point out that several countries are using ring compression tests to evaluate post LOCA cladding ductility. They're using different mechanical parameters. Some are using offset strain, fracture energy, total strain. But in some way or another, these all can be related back to material ductility, and we do see that there is some consistency between the different material properties, mechanical properties. I didn't show a slide, but there have been -- and we've done some work to compare fracture energy versus offset strain, and see if you get the same conclusion, that the material would be brittle using those two different parameters for the same data set, and you do.

The data tends to show conclusions consistent with what Argonne has produced, and that is

that the oxygen uptake in the beta layer is one of the key factors affecting residual ductility. And also, that the pre-transient hydrogen content and the distribution in the beta layer are also influencing the residual ductility.

And finally, as I said in the last slide, that some of the data coming out of the Czech Republic indicates that for ECRs of about 4 or 5 percent, and hydrogen contents greater than 300 ppm, you get very low or zero residual ductility at oxidation temperatures above 1000 C.

So the next couple of slides, I think Mike has already shown these, maybe I'll go through these quickly, are kind of a brief summary of the Argonne program, and then I'll get to the results. But what I wanted to point out here is a couple of key points. First, is that the oxidation times that -- there were a number of tests run on the irradiated H.B. Robinson fuel. F07 was derived that the samples were removed from. The targets were something like 3 percent, 5 percent, 7 percent, 8 percent, and 10 percent ECR calculated using the Cathcart-Pawel model. And these were run without quench. I think that's important, as we just heard previously, to note. But also, it should be important to note that the target ECRs and

the times are calculated based on a temperature history from a benchmark test on archive material. There's no direct temperature measurement, so the temperatures for the samples that were evaluated are not really known. They're just estimated.

Because of some difficulties in the Argonne program in testing as irradiated material, the direct weight gain measurements were not possible. There was some flaking of the oxide so that was kind of difficult to do. And then primarily at this time when this slide was prepared a few months ago, we were waiting for metallography on the measured weight gain, and that's really the method that will be used in the Argonne program to determine the actual ECR values for the samples, will be the metallographic image analysis that Mike talked about earlier today.

I think he already went over that the ring samples are 8 millimeters long, and that he typically stops the loading after the first major load drop. He showed this slide already. What I wanted to point out was that this slide is a thermal benchmark, this is the temperature history used to calculate the times, and hence, the calculated ECR values for the samples. But in actuality, the sample saw an importantly different, I think, temperature history than what was

used to design the experiment and calculate the ECR values for these specimens, and I'll come back to that in a minute.

Now these are a comparison of the load displacement curves for the various samples. These are the 3 percent calculated ECR values, 5 percent, so on and so forth. We also have the as-received material tested under ring compression test of 135 C, so you can kind of see the effect, the softening effect due to the high temperature annealing process, removing some of the irradiation damage. And then you then see the effects of the oxygen embrittlement, and possibly the hydrogen embrittlement as you move to the higher and higher oxygen contents or ECR values. So the highest ECR of 10 percent gives us the lowest load and strain or displacement.

Now you saw this previous presentation. The CA data on prehydrided zirc-4 material, and that was at about 600 ppm. I'm comparing here again offset strain versus predicted or Cathcart-Pawel ECR values for the Argonne data in blue and red, and the CA data in green. These are all tested at 135 C ring compression tests. And I should also point out that the CA data are the data that were direct quenched, not cooled and quenched, or slow cooled. Whereas, the

Argonne data are all slow cooled, except for one, and that's the red data point here, which was quenched. And what we see is that there is some difference between the data sets and the first thing we want to look at is the pre-corrosion effect having any problem, any effect here in influencing the offset strain, so what we want to do is look at the actual ECR value for the Argonne samples so we'll plot this reprocess data. Now in terms of the actual transient ECR - this is the ECR that was built up in the specimen, not calculated by Cathcart-Pawel, but actually determined from the metallographic image analysis, which tells us more about the actual weight gain that happened in the specimen, and the oxygen build-up in the specimen. And we see here that the data tended to shift from this region over here to the left, or I'll call it to the right in a minute, but the data are shifted to the right when you look at Cathcart-Pawel, but when you compare it to the actual -- using the actual ECR, there's a bit of a shift. And you get a little bit better agreement with the CA data.

Now I should note that the CA data did not move, because the Cathcart-Pawel calculated values for those samples and the actual samples are the same,

because they don't have any pre-corrosion, they don't have any effect of the pre-corrosion on the oxidation process.

Now lastly, let's look at another difference between these two data sets as was pointed out, is that we have the CA data are direct quench, and these are slow cooled without quench, so let's compare the Argonne H.B. Robinson results to the most recent results that we just saw a few minutes ago. I put this slide together last night that compares all these different types of tests that CA has run with direct quench, slow cooling, different ring test temperatures, slow cooling plus quench, and then just slow cooling, so we have a variety of different ring test test temperatures, and a variety of cooling rates. And the most representative samples or the samples that most represent the Argonne test samples are these diamonds here, which are only slow cooled, and then ring compression test at 135 C, which is the similar protocol as was -- uses the word "protocol" from the last presentation, same protocol as the Argonne specimens, which are slow cooled at 135 C ring compression test. And you can see very good agreement between these data sets here.

This point here is the one point that Mike

said maybe could be a little higher because it was a test that he stopped a little early, so this point actually could go up a little bit, and then would even probably be a better agreement in terms of the data comparison there. And then we see that if we look at the CA data a little bit more closely, if we add a quench, which would be the slow cooling here, plus quench at 135 C, that's these plus or Xs, X symbols here, and then we have the slow cooling plus quench of room temperatures. It's a little bit lower down here. I think that is a pink circle, I believe, if we look closely. And then we have slow cooling at room temperature, which are the blue diamonds here, so you can see the effect of just testing the blue diamonds which are without quench, but testing at room temperature versus 135 C, you see an improvement, and Jean-Paul pointed that out in his previous presentation.

And then finally, we have the direct quench samples, which are the samples that I was showing in the previous slides, and those are shown here, either the 135 C, or the open pink circles, or the open pink triangles. I can't tell exactly which, and then the direct quench of room temperature are the solid pink triangles there. And these have all been

plotted -- I should point out, very important point, I should point out that the CA data are plotted as ECR Cathcart-Pawel, but as I pointed out just a few minutes ago, the ECR Cathcart-Pawel and the actual ECR in the sample are the same for the CA data, so those don't shift.

In terms of the Argonne data, these have been plotted at the actual ECR space, determined from metallographic or estimated, I should say, from the metallographic image analysis that Mike at Argonne has done. If we were to plot them in Cathcart-Pawel space, they would actually move to the right by about 2 percent ECR, so all of these data points and blue circles would move to the right about 2 percent ECR, so instead of plotting at 5 and 5-1/2, these are plotted more at 7 and 7-1/2. This would plot more at about 10, and this would plot more at about 5, so they would all be shifted, and you wouldn't see as quite a good correlation between the slow cooled data sets, the diamonds, and the circles, as you would in terms of when you plot the data in terms of actual ECR, and that's one point I'll bring up a little bit later.

Just to compare low deflection curves between the two data sets, I've pulled together one of the Argonne samples. The actual ECR is about 5

percent, so this would be one of the Cathcart-Pawel samples of about 7 percent ECR. Again, it's a ring compression test at 135 C. We see the elastic portion, the non-linear portion. Here's the offset strain measurement, the fracture point. A similar curve for one of the CA specimens, again the layered portion. Unfortunately, I didn't have the ability to change the scale on this, so the X scale, the displacement scale in this sample goes out to 3 millimeters, whereas this one only goes to 1 millimeter, so can't do a direct comparison, unfortunately. But they go up to almost the same load. I should point out that the sample sizes aren't quite the same, so the maximum loads may not be exactly the same. You see a major load drop in this sample very similar to this sample, and these both showed rather low ductility.

All right. Kind of a summary then. The ANL data exhibits more post-quench ductility than other tests on prehydrided cladding material. That would be both CA and the Czech Republic data. One reason particularly with the Argonne samples is the difference between the calculated ECR value and the actual ECR value, and the effect of the protectiveness that Mike referred to. That tends to shift the data

to the right, if you will, to the more ductile region by about 2 percent is that difference, maybe a little bit less, 1.7, 1.8 percent.

Another effect is the effect of slow cooling. The slow down without quench, that tends to shift the data up, add more ductility coming from primarily the impact of the beta layer on the micro structure, and you saw a little bit about that from Jean-Paul's presentation, some new data coming out that says that you get some partitioning of the oxygen and the hydrogen into localized regions, and the depleted zones, and these depleted zones tend to have a bit more ductility, or lower micro hardness at least at this point we know, and allow the material to have a bit more macroscopic ductility.

The direct quench or the material that's quenched did not have a partitioning effect. ANL post quench ductility data seems to show less impact of hydrogen than some of the other data sets that are out there. The example we selected is that you see a pretty significant recovery of ductility going from room temperature to 135 C in the post oxidation test, which have not been really observed in the CA quench test. As I've showed, the direct quench test, or even the direct quench with cooled, there's not a big

difference between the room temperature and the 135 degree C specimens. This more than likely is related to the ductility recovery coming from the cooling rate effects.

MR. SHACK: Rob, did you mention what the cooling rate was in the Hungarian tests?

MR. MONTGOMERY: No, I did not. I apologize. I don't know the real details of their test. We're working to try to get some of that information.

MR. SHACK: That's the same with the Czech test then, too.

MR. MONTGOMERY: That's right. Just a brief summary of kind of the differences between the methodologies, the protocol between the two labs, where the most current data is coming from. First is the temperature ramp up to the target temperature. As we've seen today, ANL has a fast initial ramp, and then a slowdown where they go to about 1 to 2 degrees C per second, to the final oxidation temperature. This tends to accumulate some corrosion, I'm sorry, some oxidation at temperatures below the target temperature for the test specimens.

In addition, Mike talked about this a little bit, is that the outer surface corrosion layer,

the pre-existing corrosion layer on there, also tends to delay the sample heat-up a little bit. You saw that, and that, again, forces more oxidation to occur below the target temperature, and delays the time it takes to reach the target temperature. That's this bullet here. Whereas, the CA data tends to have a more direct ramp to the temperature without an over-shoot, so they get to their target temperature quite a bit sooner and quicker, and that means most of the oxidation or a higher fraction of the oxidation process is at the target temperature.

What's important about that is that the diffusion kinetics, the oxygen diffusion kinetic parameters are strongly temperature-dependent, and could be influenced by the fact that the diffusion process either occurring at a lower temperature over a longer period of time in these samples than in the other samples.

We also have seen today that the cool down from oxidation temperature, there's quite a bit of difference between the different labs. Argonne tends to furnace cool their irradiated specimens at this point. CA mostly quenches directly from the oxidation temperature, but newer data they're working, they've looked at the effect of slow cooling and then quench,

or just slow cooling.

What we've learned so far and what we've seen is that quench may be impacting the prior beta phase morphology, either the hydrogen content in that phase or the partitioning, or how the hydrogen and oxygen are distributed. That, again, is influencing the results.

One point that really didn't get brought up today, I have it on this slide and the next slide, is that Argonne is primarily using double-sided oxidation tests. Their initial attempt at running a single-sided oxidation test had some problems that needed to be resolved. They had some end-effect problems, and primarily the biggest one is they lost hydrogen in the flowing gas. Whereas, CA primarily uses single-side oxidation tests, with a few tests that they've conducted with two-sided oxidation.

What we did see, though, which was interesting in the Argonne test, was that when the single-sided tests were loaded, they still cracked at the ID, and they still cracked at the top and the bottom, or the 12 and 6 o'clock positions; whereas, the one-sided oxidation just said that all the embrittlement would have been on the outside of the specimen, and the specimen should have cracked at the

3 and 6 degree position, so that's 3 o'clock and 6 o'clock position, or 90 and 270. I'm going to be in degrees.

CHAIRMAN POWERS: Let me ask you a question, and this is not considered a fair question. Okay? But you know about these things, and so I'll ask you. You know, when we run the abacus calculations on any kind of structure, and then compare that to a test, sometimes the code calculates it'll break at a different place than is actually observed in a test. But oftentimes, the difference between those is delta pressure, or whatever thing like that it is. I mean, if it hadn't broken where it actually did because of some flaw or something like that, it would have broken at the predicted location within a little bit. Is that the case here? I mean, they observed a 6 and 12 o'clock break, but if they had not, it would have broken at the, what is that, 3 and 9 --

MR. MONTGOMERY: Yes.

CHAIRMAN POWERS: With delta displacement --

MR. MONTGOMERY: Yes. In fact, Joe pointed out a little bit earlier, you do have the highest stresses at the top and bottom. That's where

you get discontinuities because of the loading platen, the shape of the loading platens.

CHAIRMAN POWERS: Right. Right.

MR. MONTGOMERY: The next highest place where the highest stresses are at the 3 o'clock and the 9 o'clock position. Those are the next highest locations if you look at your abacus calculation. But the stresses are highest at the OD; whereas, they're a bit compressive, or less high at the ID.

CHAIRMAN POWERS: Yes.

MR. MONTGOMERY: Whereas, at the 12 and 6 o'clock positions, the high stresses are on the ID. The question here we have is that that ID material in the Argonne test should have been fairly robust because it didn't have much oxidation. It should have been primarily beta material, and all of the oxidation was from the outside, so the alpha layer and the oxide thickness were all on the outside, so that's the question.

In the CA program, they tend to see that on one-sided tests they get cracks starting from the OD, because that's where the most embrittled material is. And then when they have two-sided oxidation, they get the crack starting on the ID, which is where the most brittle material is.

Our question is, when we look at these data - and I'll come to a slide on that - is there something inherent in irradiated material that should cause crack initiation on the ID, even though that's not where the ECR parameter is that we're looking at, is the largest? That's kind of a question; or is that a test artifact, or the way they're loading the specimens?

DR. BILLONE: I sort of put those results aside because we lost hydrogen, but you're ignoring the fact that you started out with a 10 micron fuel cladding bond layer and it disappeared, so where did the oxygen go? It went into the metal, so you do have some embrittlement at the ID, not from the steam, in those tests. But I don't like those test results, so I'm not trying to justify them. You do see cracks at -- let me get this right now - the 3 and 9 o'clock position. You see that kind of cracking on as-irradiated cladding coming out of a pool where you have the brittle hydride rim, and you have the brittle oxide rim. And that's where we definitely see it, and whatever we saw in the one-sided test, I really would like to complete the test of the long fuel sample where you have an oxygen source from the fuel, the fuel cladding bond. That still may break at the ID.

I don't know, but at least it's a valid test.

MR. MONTGOMERY: Right. Yeah, I think that's what our point is, too, is that we need some more data to evaluate if the ID cracking that was seen in your first run, the one-sided oxidation test is a valid assessment or not. I think that's the point. Should we just ignore that data totally and wait for you to do more tests?

DR. BILLONE: Yes.

MR. MONTGOMERY: Okay. So we won't have to talk about this point then. We've basically hit this point on this slide, but it's our opinion that there still needs to be areas of research that are remaining to be explored, and I've kind of listed some of those here. We really need to understand a bit better the impact of cooling rate and quench temperature on the embrittlement process, both from the fact that we now have seen it, that it can influence the results, and the second point is, we need to identify what we're doing in the labs, how relevant that is to what the actual LOCA events are.

One point that Mike just went over rather quickly, he's noted it, but in other test programs they have not seen that effect, and that is the hydrogen redistribution that does not occur greater

than 1000 degrees C. Argonne observed that, but in other labs they have not seen that. They typically do see hydrogen redistribution going on, so the question is, is that, again, some artifact in their test program, or is that real, and how do we understand it?

I won't talk about the ID alpha layer and the one -- we just talked about that. And then finally, this effect of pre-transient corrosion layer on oxidation kinetics, that needs to be resolved. We need to talk about how to -- need to decide how to differentiate between Cathcart-Pawel value and the actually measured value, which one should be used in defining, evaluating the data and then using the data later on in terms of criteria development.

CHAIRMAN POWERS: Let me ask a question about your redistribution results, or it's really just a question about diffusion. I just don't know how to do diffusion calculations in this circumstance. You've got two things diffusing around you. You've got oxygen and hydrogen, and I assume they're both interstitial. Is that true?

DR. BILLONE: The oxygen solution, the hydrogen solution.

CHAIRMAN POWERS: Yes, interstitially.

DR. BILLONE: They're supposed to treat --

CHAIRMAN POWERS: So you can't -- so you have to be very careful when you do diffusion calculations here. You've got two things moving; therefore, the solute has to move, so you have a multi-component diffusion problem here. And so because one of the diffusants has a really high mobility, things can move contragradient? I mean, that happens in ordinary diffusion problems all the time, something has to move against the gradient. So this is --

DR. BILLONE: But I'm not sure Rob is -- you might be mixing apples and oranges. If CEA and JAERI did a good job of homogenizing the hydrogen, for example, in the axle direction of the circumferential erection, what they're seeing is movement across the radius, we agree with. We did such a crappy job in hydriding, we had huge concentration gradients, and they were pretty much frozen in the samples, so what I specifically was referring to is during the time of the test, there isn't much redistribution of hydrogen in the axial direction.

MR. MONTGOMERY: Or circumferential.

DR. BILLONE: And the circumferential.

MR. MONTGOMERY: Right.

DR. BILLONE: Now that's because we

started with non-uniform distribution, we can make that statement. To the extent that they started with good stuff, which is homogenized, there is no contradiction in the results, though.

MR. MONTGOMERY: Well, it's an issue that needs to be raised, I think, or at least answered. We need to answer it. You've seen --

DR. BILLONE: You said not consistent, and I'm not sure that they looked -- they'd have to start with the same samples we started with, and then take hydrogen measurements.

MR. MONTGOMERY: Yes. Currently, they're not consistent between the two data sets. And we just need to answer --

DR. BILLONE: To me, they're not inconsistent, because we're starting with different samples. We started with very, very different samples, and I thought they only looked for redistribution across the radius. I thought that was the primary point, that hydrogen very quickly -- if you start with a hydride rim or something like that, it very quickly --

MR. WAECKEL: I have a comment, just a short comment. I think --

DR. BILLONE: Or did you do a bad job

hydriding it?

MR. WAECKEL: In both CA and JAERI tests, they use some sample with hydride rims.

DR. BILLONE: Okay. But then all you can look at --

MR. WAECKEL: And we got everything homogenized very quickly.

DR. BILLONE: Across the radius.

MR. WAECKEL: Across the radius, yes.

DR. BILLONE: Yes, well that doesn't contradict what we did. We didn't look across the radius. We looked in the long direction. The radius is .6 millimeters. We looked over the 25 millimeters and over the 30 millimeters of the circumference. There's a huge difference in length of diffusion pattern, so that's why I don't like --

MR. MONTGOMERY: Yeah, but I can't speak too much to the CA data, because I haven't seen it all yet, but I can speak to the JAERI data, and they see the redistribution happening in a second or two. And you're up at oxidation times of 3000 seconds or 2000 seconds, or even 1800 seconds, so you're much longer time, so you allow for some diffusion to go on. So that's what I'm --

DR. BILLONE: We're more like 1200 degrees

C, we're more like 60 seconds to 150 seconds or something like that for the prehydrided stuff. Those were low ECR samples.

MR. MONTGOMERY: Right. But you've seen this even in higher ECR samples.

DR. BILLONE: But JAERI has also seen it in high ECR samples in their secondary hydriding peaks.

MR. MONTGOMERY: You see the peaks, right.

DR. BILLONE: So how could you possibly sustain a huge --

MR. MONTGOMERY: Yeah, but that's a different process. That's a hydrogen absorption process from a gas on the inside of the fuel rod or the cladding.

DR. BILLONE: But why didn't it diffuse? Why didn't that --

MR. MONTGOMERY: It's being absorbed the whole time that the diffusion process is going on.

DR. BILLONE: Okay. I don't see the inconsistency.

MR. MONTGOMERY: Okay.

DR. BILLONE: They were totally different studies with materials.

MR. MONTGOMERY: My job today is just to

raise some questions.

DR. BILLONE: No, that's a specific statement. There was no question there.

MR. MONTGOMERY: All right. Did we answer your question, Dana?

CHAIRMAN POWERS: No, but it's illuminating, nevertheless.

MR. MONTGOMERY: All right. One slide here on the effect of the pre-transient corrosion. There really isn't much data that indicates that the pre-transient corrosion would remain protected for fuel rods experiencing an actual LOCA event, that includes the ballooning and burst process. JAERI has presented some data that shows that the pre-transient corrosion protectiveness is lost once you get any kind of cladding deformations going on.

In addition, as we saw today, there are some thoughts that the pre-transient corrosion protectiveness is really more exacerbated by the slow temperature ramp, and that if the temperature ramps the pre-transient corrosion tends to add to the oxygen absorption and diffusion process, and doesn't really affect the -- or goes ahead and affects the ductility the same way as if the material was not pre-corroded.

CHAIRMAN POWERS: Let me make sure I

understand this. Now I'm operating a little bit from memory here, but my memory is that the experiments were done with a very rapid, very rapid heat-up to like 800 degrees Centigrade, and then a slower kind of ramp-up into the 1200 degree Centigrade region.

MR. MONTGOMERY: Which program?

CHAIRMAN POWERS: This is the Argonne test.

MR. MONTGOMERY: Okay.

CHAIRMAN POWERS: And so that initial heat-up to temperatures where the ballooning would occur, it's very rapid. I mean, so you can't complain about -- that can't be the slow one. It has to be the post-ballooning heat-up.

MR. MONTGOMERY: Yes.

CHAIRMAN POWERS: And is that post-ballooning heat-up slow relative to the heat-up that the clad would experience in an accident?

DR. BILLONE: No, relative to other experiments.

MR. MONTGOMERY: Yeah. You mean you're talking about the Argonne test program.

CHAIRMAN POWERS: Yes. Yes.

MR. MONTGOMERY: The 1 to 2 degrees C per second, I think that's about what you're shooting at.

Right, Mike? You're shooting for 5, but you're actually getting something like 1 or 2. It's a little lower. Typical heat-up rates are on the order of about 5 to 10 degrees C per second, depends on, again, there are many LOCA events, LOCA scenarios out there.

CHAIRMAN POWERS: I understand that.

MR. MONTGOMERY: So we try to do an evaluation, and 5 degrees C, somewhere between 5 and 10 degrees C would be a representative heat up rate.

CHAIRMAN POWERS: Your statement could be entirely correct. I'm not sure it's a critique of the experiment.

MR. MONTGOMERY: Yes, maybe I didn't word it exactly correctly in terms of it's one factor that is a --

CHAIRMAN POWERS: I mean, you could be entirely correct. Yes, this affects the results we see, but it seems to me that once I've gone through the ballooning, not much happens prior to the ballooning.

MR. MONTGOMERY: Right.

CHAIRMAN POWERS: And once I go through that ballooning, then I want to be on a ramp that's reasonable, if that ramp is important. If that ramp is not important, then I don't care. But if that

ramp, in fact, is influential, then I want to be on a ramp that's -- I mean, I'm not sure I can tell the difference between 2 and 5 degrees C per second. I certainly don't want to be at 100. If that ramp ridge is important, and from what Mike was saying in his presentation, that ramp is important. You've got a substantial amount of oxidation in that 800 to 1200 temperature regime.

DR. BILLONE: Well, like 1000 to 1200, yes.

CHAIRMAN POWERS: A thousand to 1200, okay.

MR. MONTGOMERY: Yeah. And some of his sample is 100 percent of his oxidation occurred in that period of time depending when ECR --

DR. BILLONE: It's still a more reasonable rate than 20 degrees C per second or 50 degrees C per second. And, Dana, there's still data that can be used along with other data if you know what mechanistically is going on. You get off the ECR into embrittlement phenomenon.

MR. MONTGOMERY: That goes to the last point here, is that the as-measured, the actual ECR where we all it, as-measured is kind of a difficult term because I know in some tests it's difficult to

measure the ECR value, but using the actual ECR is more representative of what's happened in the sample, as opposed to the square root of time Cathcart-Pawel-type parameter that was tried to use to normalize the specimen. We think that, as I showed earlier, if we try to compare two data sets from two different labs, using the Cathcart-Pawel parameter works as long as the Cathcart-Pawel parameter represents the actual ECR. Once it does no longer represent the actual ECR, but there's a deviation, then you can't use that parameter. You can't use Cathcart-Pawel ECR to prepare data sets. You have to use the actual measured values.

DR. BILLONE: Or you can reword it and say it's conservative to use the Cathcart-Pawel for corroded cladding.

MR. MONTGOMERY: I don't think it's conservative. I think it's non-conservative, to be quite honest with you, but that's a personal opinion. That may not represent everybody's opinion in this room.

DR. MEYER: It depends whether you're talking about for calculating the amount of oxidation, or for calculating the embrittlement, because I think we have concluded, and have confirm from Jean-Paul,

that we really are talking about the time required for the oxygen to diffuse into the metal for the embrittlement phenomenon. And in that case, we want to use a time parameter, which we're doing indirectly using Cathcart-Pawel. And we would not want to use measured values for the embrittlement correlation.

MR. MONTGOMERY: Well, I deliberately tried not to get into this subject, but maybe I'll just leave it at that, because I believe -- our position at this point, or at least my position at this point is, is that in evaluating the data in terms of defining ductile brittle transitions and things of that nature, the actual ECR is a better parameter to use than the Cathcart-Pawel, if there is a protectiveness effect.

Since we didn't really go over an embrittlement correlation, I won't go through these slides. They're there. I'll just skip to the conclusion slides. The pre-transient corrosion we've seen today, been demonstrated pretty well that it does have an impact, and does reduce residual ductility. The real question remains is how much to the extent does the pre-transient hydrogen content impact the residual ductility. That's still a question mark.

Comparison of the Argonne data to other

data sets raises many questions about the post-quench ductility of irradiated material. We talked about most of these, the effect of cooling rate and quench temperature. That needs to be further evaluated with prehydrided material or irradiated material.

Effect of pre-test corrosion layer - is it protectiveness or not under all conditions? The mechanisms for hydrogen embrittlement needs further research. We've seen today that the hydrogen and oxygen diffusion redistributions and the synergisms between those two components have a very important impact on the residual ductility.

The embrittlement correlation we didn't really talk about, but if this is an approach that's going to continue to be pursued, we clearly need more data to really develop a model that incorporates all the key factors that are influencing residual ductility.

MR. CARUSO: Is this embrittlement correlation part of this proposal?

DR. BILLONE: No, Ralph didn't make it part of this proposal.

MR. CARUSO: Okay.

DR. BILLONE: It's our attempt to compare data sets from different labs. It's an analytical

tool to take into account different heating rates and different experiments, try to get at what's fundamentally embrittling material, and get away from ECR.

MR. CARUSO: So it doesn't form any part of the --

DR. BILLONE: It's a correlation, not a criteria, at this point.

DR. MEYER: Yes. Last winter we considered using it directly and during the spring changed our mind.

MR. CARUSO: So it doesn't form any part of the technical basis for the rulemaking.

DR. MEYER: It forms part of the technical basis, because it describes the understanding, and it's only after we get a clear picture of the effect of hydrogen on the solubility in the beta phase for oxygen can we then go and use some simplistic-type empirical correlation with a little more confidence. And I think that's what we've tried to do.

MR. MONTGOMERY: So the last bullet here, although we feel that Argonne has made a lot of good progress, Odelli summarized a lot of their progress made to-date, and they really have pushed the boundaries of our knowledge quite a bit forward, we

still believe that there's some work that needs to be done, and that the technical basis at this point for initiating the rulemaking process is not quite complete, and we need more information. And basically, the bottom line is that the results that are coming out look very good, but we still need to consider them to be preliminary at this point, and that we need additional data to answer all these questions that have been raised by all the good work that they've done. I think that's it for me.

CHAIRMAN POWERS: Are there any questions for Mr. Montgomery? I think you've been interrogated enough.

MR. MONTGOMERY: Thank you for the time.

CHAIRMAN POWERS: One question I did have for you. One of your slides mentioned Japanese presented data on non-protectiveness of the oxide. Can you elaborate just a little bit about that? Have I seen these data, or can I see these data?

MR. MONTGOMERY: I believe you can. They've fairly recently been published in the open literature, and they found that cracking of the oxide, once cracking of the outer surface corrosion layer occurs, and this can occur by straining, that once that cracking occurs, protectiveness goes away, that

the oxygen can get into and get direct access to the metal. And the oxidation rate begins to increase back to the rate that you would expect for bare metal material as opposed to pre-oxidized --

CHAIRMAN POWERS: Do you have a citation for that?

MR. MONTGOMERY: I don't have it handy, but I can get it for you.

CHAIRMAN POWERS: Yes, I appreciate that. I'd just like to look at it. Okay. At this point on my agenda, Bert, you're going to give us a perspective. About time we got some perspective here, right?

MR. DUNN: Thank you for letting me say a few words. I promise this will be very short.

CHAIRMAN POWERS: It doesn't have to be short, it has to be informative.

MR. DUNN: Well, I wanted to put forward our opinion, Framatome's opinion, on the status, and in particular, relative to the rulemaking that may or may not proceed here in the future.

I am the lead contact between Framatome and the Argonne program, and have been responsible for facilitating some of the assistance that we've provided the Argonne program. I want to say that this

program has been open to us. We've had the availability of working within it, through it. We've been listened to, we've learned an awful lot, particularly with Mike, and so that's been quite an enjoyable opinion.

Now I'm going to show the next slide. All I want you to look at now, because it's going to be redundant, you'll see it a bit later, we're basically in agreement with some of the stuff that EPRI and the industry has come out. I may very well be speaking for industry, as well as Framatome here; that the initiation of rulemaking at this time is premature. We want to understand the rule, not the rule - excuse me - the data, and how it comes together. We want to get the theories right. We want enough peer review so that we know we're going in the right direction. We don't want to be back here five years from now.

CHAIRMAN POWERS: Let me ask you this question.

MR. DUNN: Okay. Do you want me to go back?

CHAIRMAN POWERS: Yes, please, go back to the other slide. I think we're confronting a dilemma here. We're on a pathway to do something with 50.46. I'm not sure what exactly is going to happen there.

MR. DUNN: Are you talking about the other rule?

CHAIRMAN POWERS: Yes. Your third bullet is in the works, something is going to happen there. But part of 50.46 includes these cladding criteria, and we have a problem. I mean, it's just a continuous problem. It's expensive for the industry, it's expensive for the staff, and that is that that rule is written probably inadvertently for Zircaloy and ZIRLO.

MR. DUNN: That's correct.

CHAIRMAN POWERS: Okay. So every time anybody uses any slightly different clad, it doesn't have to be very different, they have to come in for an exemption.

MR. DUNN: I'm one of them.

CHAIRMAN POWERS: And so we have an opportunity here, since the train is in motion here, to go ahead and fix that, so you guys don't have to get an exemption every time you turn around. And this is every core load.

MR. DUNN: True.

CHAIRMAN POWERS: I mean, this is not I get it once, and then I go away and forget about it. It's every time you reload the core, you have to get an exemption.

MR. DUNN: Once you get the first one then the --

CHAIRMAN POWERS: It's easier, yes, I know, but still somebody has to process some paperwork, and I am always told by the industry processing a piece of paperwork is more expensive than I'd like to think it would be.

Okay. It seems to me that the choices are go ahead and live with it, make an interim change that we may not be as comprehensive as we like, or wait and make a change in five years or something like that. And you're voting for making the change in five years.

MR. DUNN: I'm not sure I'm voting for making the change in five years. I think I may be voting for starting the change process in six months to a year from now. We've thought about this, and as a company, I'll speak for Framatome here, we've decided that it's more important to be sure we capture the right type of criteria and the right effect, as best we can do it. We can never be perfect.

CHAIRMAN POWERS: Sure.

MR. DUNN: Than it is for Framatome to no longer have to make its exception for the M5 material. We didn't actually connect these two up. They kind of got connected up by happenstance and I think maybe

Ralph or Farouk can tell you how they got connected up. We submitted a recommendation through NEI, I think it was Working Group 2 that helped us submit it two or three years ago, and gotten it on the docket to try and change to Zircaloy-based alloys that were approved by the staff. And we would have liked that to have gone ahead. It got derailed a little bit because of some internal workings in the staff, I believe, or between the commissioners. But still, my answer to you is, I'd rather do it right, and know that it'll last a while, than to do it wrong and have to come back to this.

CHAIRMAN POWERS: Let me ask you this question then. When you say "do it right", are we looking for a rule that accommodates M5, and maybe E110, I don't know, but certainly accommodates M5, or should we be looking for a rule that accommodates - I don't know - M6, Zircaloy-10, whatever is coming down the pike that I don't know anything about right now.

MR. DUNN: Well, that is of interest to us, is to have the formulation be at a nature and follow or suggest procedures for the establishment of whatever the criteria might be. It could be responsive. I think I would cut it at Zircaloy-based, Zirconium-based alloys. I'm not sure the -- I came up

with at lunch time --

CHAIRMAN POWERS: Silicon carbide reinforced.

MR. DUNN: Silicon carbide clad we need to worry about now, but the --

CHAIRMAN POWERS: It's very interesting stuff.

MR. DUNN: It is very interesting. It's a wonder how axial offset is going to live with that stuff, but --

CHAIRMAN POWERS: If you've ever looked at the mysteries of making silicon carbide, you would not be so enthusiastic.

MR. DUNN: Actually, I think they're all BMW people that are involved in that. Okay. Should I go ahead?

DR. DENNING: Well, no. Let's follow that just a little bit further, because I'm kind of curious in terms of what your concern is about the future. I mean, is it your concern that there will be a rule that will turn out to be too restrictive later, that you'll come in with some new clad material that isn't going to pass the test?

MR. DUNN: Actually, I think it's both. It could be that, or it could be --

DR. DENNING: And is it the other way?

MR. DUNN: It could be the other way, as well.

DR. DENNING: Now what kind of benefit could you see from a less restrictive -- I mean, if you came up with something that's less restrictive than what RES is kind of proposing here, what kind of benefit would that provide to you? What kind of advantage could you take with that in either a safety domain, or an economic domain? I'm kind of missing why this particular thing is of such a level of concern to you.

MR. DUNN: Well, if it were -- actually, what Ralph has proposed today on the surface looks fairly decent in terms of its consequences to the industry. Okay? When we came in this morning, we were thinking that something unknown was going to be coming out, and it was going to somehow relate to the embrittlement correlation. And the details involved in embrittlement correlation need to be established and validated within the community of manufacturers, if you will. And we need to look at whether that correlation would be responsive to future alloys. What could we expect to have happen; it goes away completely and is totally inappropriate when we come

in with a clotinary alloy or something like that.

The proposal that we heard today, we don't know where the regulatory side of the organization is going to go. If they were to go like that, my thought is that that's probably going to be pretty good, provided it works. It provides the protection that we want to provide.

The other side of it is, I don't -- I mean, five years ago I got called into the staff here on the 1 percent niobium question, and literally people were standing us up on our heads and shaking us because of some inappropriate testing that had been done in East Germany, the Bohmert issue. Okay? And one of the reasons that we got called in is no one understood why E110 and M5 were behaving differently. We want to try and avoid that kind of a situation developing, as well, because that's always bad for the industry.

DR. DENNING: I understand, I think, where you are.

MR. DUNN: Okay. So if I didn't say it, I want the correlation to do the job of the correlation, the criteria, the procedures we come up with to do the job that they should do, because I think that's the best thing for nuclear power and my

company.

To face that a little bit, when we licensed our new alloy, we recognized the need to qualify the criteria for the new alloy. Now we didn't do that according to the ring compression test, because at the time what we felt out of the literature we looked at, and what we were familiar with, was that surviving quench during an accident was the basis for the criteria.

We still have arguments between Ralph and not so much myself but one of my compatriots in the company who likes to argue all the time about whether ductility is the appropriate metric, or whether it's necessary, or whether, in fact, the staff threw it away in 1985 when they approved best estimate LOCA. So those arguments still go around, but we did look at each one of the five criteria. And we established a reason why those criteria still apply to our alloy, so being interested in this is not new to Framatome.

It's not new that we've participated in a program. And I want to remind the committee that industry has contributed a great deal to the program. It's not our program. We haven't contributed the majority to it, but we've contributed quite a bit. And in speaking of strictly the Argonne program and

the non-irradiated, we've gone through and worked out a memorandum of understanding so that we could share research. We've provided a substantial amount of M5 and Zircaloy cladding. We've shared test procedures and information, and guidance with Argonne directly to make sure that they knew what we were doing in other locations in the world, and this is not just us, this is EDF and CEA. It is primarily French, but it's been there, so we've -- and some of the particulars there are the first tests on prehydrided Zircaloy cladding, which indicated that things got pretty brittle at 600 ppm hydrogen, sharing test procedures. And finally, today you saw some information on cool down and corrosion layer effects.

Now the cool down information actually was provided informally about four or five months ago, but it wasn't provided to the extent that you saw from Jean-Paul today. And we've just now completed the corrosion studies. So in the irradiated program, we're working on a cooperative memo. We have worked out and assisted in supplying the fuel cladding samples for M5 to Argonne. The industry, Westinghouse, has worked out supply de-fueled samples of Zirlo. Our M5 samples will be at 63 megawatt days per metric ton. That's a fairly high exposure. It's

not up to 70 or something. We worked our supplying and actually have now moved the rods into the spent fuel canal in a special basket ready to be shipped. At least two full-length rods, there may be little parts of those rods that are going to be used in a hot cell examination, but there'll be 70 megawatt day per metric ton rods provided to Argonne, M5 for their program. Those will contain fuel, and those will be used in integral tests, and maybe in long section single-sided oxidation tests.

We're going to continue the formal and informal briefings between CEA, and EDF, and Argonne. And we're working to try and keep the program, you might say going, and on the best track we think we can. So enough patting myself on the back. I just wanted to be sure people knew we were there, or at least remind you of what we were doing.

Going back to the first slide I showed, we think it's premature. We think the cladding embrittlement database is immature at this time. It does have holes in it. It hasn't been cross-connected, cross-tested enough from lab to lab, et cetera. We haven't got as much cross-checking as we'd like to do. Testing continues. We continue to be on a fairly steep learning curve with this one.

CHAIRMAN POWERS: That seems, for your point earlier about cross-testing of specimens, that seems to be a fairly crucial step in any kind of mechanical properties testing, isn't it? I mean, it seems we never understand anything, that we do one of these round-robin tests. Is that roughly correct?

MR. DUNN: I'm not sure we understand it after we do the round-robin tests. The ring expansion program that tried to look at the Oak Ridge test, which I happen to like a lot versus the gauge tests, et cetera, I don't know what happened with that. It just kind of did something, and then evaporated. I think it leaves quite a bit, as well as we can do it, it should be done, because then we -- you always are going to learn something.

I know I'm not on your subject. I'm not a metallurgist and stuff like that, so I get a little difficult. We stepped into this one. My biggest concern on getting rid of large break LOCA is that we will lose the opportunity to learn something. By doing large break LOCA for 35 years, we actually have uncovered problems, and addressed them. Now it's been an expensive way to do it, but we throw them away completely, where will that discovery come from? Okay? And that's one thing that happens with

processing.

In peer review we get discovery, as well. Okay? And maybe peer review is more important when we were talking about the correlation as a basis. Okay? I still think that correlation needs to be peer reviewed. I would love to have a good correlation on embrittlement, because I really don't know that we have it today.

Okay. Then you come down here to the potential change in the rule. I had a note for myself that I was supposed to say something earlier. Oh, one example of something - we heard a lot about oxygen, how important oxygen is, as it diffuses into the prior beta region, or into the beta region at the time. Anybody show us an oxygen map today? Did we see a radial survey of what the oxygen content of a sample was post transient? I don't remember one.

DR. BILLONE: I've got a calculated one and I've got micro hardness. That's all --

MR. DUNN: We've got calculations, we've got micro hardness. We haven't taken the step to go do a micro probe where we can actually measure it, but that could be done. I don't know. It would be nice to have that. Okay.

We talked a little bit about heat uprate.

If, in fact, we make this transition, the important zone for heat uprate is going to switch back to 1 degree C per second, or maybe even less than that, a half a degree C per second, depends a little bit upon how the transient goes. But in your small breaks, your critical small break is going to heat up on curves and be very slow. Now they're all on curves, but with the small break you can almost come close to reaching quasi-equilibrium sometimes against the boil-off rate, and come on down. I'd like to make sure we've got that type of an influence in our knowledge-base, in our consideration-space before moving forward.

Finally, some of the other reasons is when we go forward with rulemaking, we're going to establish plans, we're going to establish expectations, we're going to establish schedules. Okay? Even though we're going to continue to do testing and understand this thing over the coming year, I'm afraid those plans and schedules may put a little artificial constraint, an artificial pressure to come up with a set of criteria that just work. We agreed earlier so I won't read that, but there's no safety issue identified.

It could very well be in terms of

something that would be easier, is the quench survival would be appropriate, perhaps restrained quench survival test, similar to what the Japanese are doing. At least at Framatome, we're not convinced that the ductility is the necessary metric.

CHAIRMAN POWERS: It seems to me when we started the discussions this morning we hit this general area going back to we don't really know how the core responds to a reflood that you get from a stress strain relationship sort of thing. Is that what you're talking about there?

MR. DUNN: Well, yes. I mean, people say that. I think if you were to take a look at the reflooding test, for example, you can get some idea of what some of the reflood-related phenomena were. Perhaps, asking Larry Hochreitter to give us some information from his current test rate, which is sitting there and there's at least partially a bundle, and can tell us some information. And then we go with the current Japanese position, which was to -- I think you looked at the grids and they put a restraint on it that was enough to bend a grid or something on that order, and so you would have some relief after you pass that. And maybe that's what provides you with your margin to knowing what happens.

We, I think, do know a fair amount about the process and how we'll proceed. In 1970 we hadn't done very -- we'd only done the FLECHT test. We hadn't done LOFT, we hadn't done a lot of these tests. We've done a lot more. Whether that answers all the questions, I guess I'm not prepared to tell you today, but I do know one thing, is that the cladding survives this easier than it does the ductility, so the ductility does provide some margin. I would agree with that, when that was said this morning.

CHAIRMAN POWERS: There's no question that the ductility criteria was intended to bound.

MR. DUNN: Yes. Okay. So what am I asking, and that's the bottom line. I'm just asking that there be no rush to go into the rulemaking. And I guess I'm talking not to Ralph now, and who's at the table over there. I'm talking to the back row, and to the subcommittee, because the subcommittee can influence all sides of this question. We do think that there should be some substantial more review data, peer review available.

CHAIRMAN POWERS: Any questions to the speaker?

MR. ELTAWILA: Can I make a clarification and ask Bert a question. I just want to be sure that

it's understood here that 10 CFR 50.46, Part A and Part B, can proceed separately. They are not linked together. So your discussion here talk about Part B only, or you're also addressing the 50.46 changes in general?

MR. DUNN: I brought the 50.46 changes up, but I understand it's not going to be forced on people, that it will go in as an option, so we may still have plants for a number of years that are doing large break LOCA with the old criteria. But again, we have said that large break LOCA is so risk-insensitive that we're willing to go license plants without it. We should be able to say maybe those current criteria are all right for that period of time.

MR. ELTAWILA: So to understand it myself, so you are -- what you are asking for delay right now is for the fuel criteria in 50.46, and not for the break size definition in 50.46. Or you're asking for both of them?

MR. DUNN: No, no. I am supportive of the removal of large break LOCA from the design-basis. In 1978, I remember people saying - or in 1979, I remember people saying that one of the reasons -- that we thought large break LOCA bounded everything. Well, in '79, and I found out very personally, because I had

to testify on it quite a bit of time. I was in the wrong place at the wrong time with a little bit of information. We should have recognized that we did not have a certain human aspect tied down well enough, and we should have done something about it.

Now we had that information here, we had that information -- well, I'll just -- I'm on my soapbox now. I'm sorry. Large break LOCA doesn't bound everything. We do that other one, and we can concentrate on what will really happen.

CHAIRMAN POWERS: Any other points? Thanks, Bert.

MR. DUNN: You're welcome.

CHAIRMAN POWERS: At this time, I'm going to ask if Mr. Shadis from the New England Coalition can make his comments.

MR. SHADIS: Thank you. Do you want me at th is microphone over here?

CHAIRMAN POWERS: Oh, why don't you come up and sit up front. And we're going to put you in the spotlight here.

MR. SHADIS: How is this from here? I think people can here me without --

CHAIRMAN POWERS: No, it's not us hearing you, it's the keeper of the record that has to hear

you.

MR. SHADIS: Oh, thank you. First off, my name is Raymond Shadis. I work for a group called the New England Coalition. My job with them is to identify and track safety and environmental issues at New England's nine nuclear power stations, five operating stations, four in decommissioning. And I really appreciate the very focused, professional presentations done today on all sides. It was totally informative.

A couple of things - first, I heard both NRC staff, the Chairman of this subcommittee, and folks on the industry side say there is no safety issue to be dealt with here. But if you read the second slide in the first presentation from NRC staff, we're talking about the goals in 50.46. And if the goals are to maintain a coolable geometry, keep the fuel pellets inside the cladding, don't let the cladding fragment and so on, if those are the goals, and there is a challenge to those goals, or if we're not doing enough to make sure that those goals can be met, then I take exception, there is a safety issue. From the public point of view, we have been promised over and over that there exists something called defense-in-depth, that there are multiple fission

barriers, and I think one of those is the cladding. We're talking now about the integrity of the cladding, whether or not embrittlement of the cladding automatically signifies failure in an accident. I don't think so, but it could, and so from that perspective, we see it as a safety issue.

I was very glad to hear Framatome say that they were going to be providing some samples taken from a reactor to be examined by Argonne. We were a little taken aback that before that presentation all that we heard about that was that there were samples taken from Robinson. And really, for what it's worth, layman's point of view, the issue is not fresh samples, or samples that have been irradiated, it is samples that are taken from a reactor environment.

You have your one, two, three phenomenon that you're considering, irradiation, the presence of oxygen, the presence of hydrogen, but I'd offer that in the reactor environment there are other phenomenon that may predispose the fuel to embrittlement, and may predispose the fuel to fail under the effects of embrittlement. And just the only few things that came to mind, and they may be such small contributors as to be negligible when they're squared off against these big three phenomenon. But if they are, then I think

in your deliberations or somewhere it ought to be stated that they are negligible, just to give a little comfort to the laity.

I look at, for example, water chemistry, Noble metals treatment or hydrogen treatment, pH content, cooling water, not all of it is completely demineralized, maybe copper content. If you're looking at incipient cracking, microfissures, the stress corrosion cracking, intergranular stress corrosion cracking as it begins as a setup, as a nodal point for fuel to fail. You're only testing and examining what are they, 8 millimeter or so, segments of fuel, then you're not really dealing with the mechanics of fuel being stressed over time, not a 12-foot segment, whatever that is in meters, two and a half meters, whatever it is. Fine.

There are questions of low-end thermal cycling for the fuel. I would presume that if you heat the fuel up to 1200 degrees Centigrade that you will, in effect, anneal it, remove all those stresses, but I think it may be important to examine whether or not there are some residual effects, whether or not you're starting with a fuel that is bowed, or bent, or deformed in some other way. So I'd ask you to, in your consideration of this, to just reach back and

take in some of those what may be minor phenomenon. And that's all I have to offer. I really -- once again, I totally appreciate the focused comments that we heard today. Questions?

CHAIRMAN POWERS: A couple. First of all, I appreciate your comments here. I want to come back to your first comments on the goals that have been set down, which include maintain coolability and assure the fuel doesn't -- are you comfortable with those as being goals? Does that look like the adequate description of this element of defense-in-depth, recognize the clad is an element of our defense-in-depth?

MR. SHADIS: I think under the specific criteria that are referenced, sure. It's a reasonable articulation.

CHAIRMAN POWERS: Okay.

MR. SHADIS: May I just mention, also, in terms of real-life plant experience, that we're now dealing with a lot of plants that have gone for extended power uprate, including one in my territory. And the record so far is not perfect by any means. There are problems surfacing in particular with flow induced vibration, so where fuel can be predisposed to fail or other reactor internals predisposed to fail,

it becomes all the more important to make sure that that cladding integrity is preserved. I just offer you that.

CHAIRMAN POWERS: And there really are lots of chemical phenomenon on surfaces that I certainly don't understand. You mentioned noble metals also, boric acid deposition, things like that. That's a good point.

MR. SHADIS: Yes. Well, when you're dealing with little segments --

CHAIRMAN POWERS: Well, we don't have much choice but to deal with --

MR. SHADIS: Oh, I understand that, but you have a certain symmetry that is there, and that affects the way that you measure. But corrosion, and I have a phenomenon with control elements called shadow corrosion. Corrosion doesn't always fall uniformly on the fuel either, I would guess, or the accumulation of crud or whatever other kinds of phenomenon may play on the way that heat is distributed through fuel or stresses. Somehow those are -- just on first glance, they don't appear to be addressed in this narrowly focused discussion, and I'm suggesting they should be.

CHAIRMAN POWERS: You've got more

comprehensive -- right now you're seeing a work in progress, and I assume that with more comprehensive documentation, lots of things would flow in the description and cover things more completely. And you've certainly given us a nice list of things to make sure they cover.

MR. SHADIS: And I would qualify this, too. And again, on a layman's perception, in reading the list for the modifications for 50.46, it does not appear that there are any great show-stoppers there. There doesn't seem to be, in reading them, that there's any reason that that rulemaking could not go forward and still have the exploratory process for improvement go on. That's the best I can tell you.

CHAIRMAN POWERS: Any other questions for the speaker?

MR. SHADIS: I'm good at doubling recipes.

MR. OZER: I have a question, Mr. Chairman.

CHAIRMAN POWERS: Sure.

MR. OZER: Odelli Ozer, EPRI. I was surprised by your discarding the Robinson data in favor of the other material that we'll be getting. You must realize that this is 12 rods that were exposed in a reactor to rather high exposures, well

beyond the currently licensed limit. So they did experience boron cycling, whatever, and just because we consider that they were limiting, they were exposed to this very high exposure.

Similarly, the Limerick rods were from the Limerick reactor. Again, they were exposed to the limits, currently licensed limit, and they're a part of our characterization program that we're holding for normal in-reactor operation, and some of these rods were also made available for LOCA testing. So why are you unhappy with the --

MR. SHADIS: I'm not unhappy with it. It's my fault, and I'm sorry that I was unclear about it. And I neglected to mention the Limerick donation also. What I was saying was up to the point that we heard that Framatome was coming in with yet more rods to be examined that came from a reactor. I thought it was a very limited sample, nothing wrong with the sample, probably a wonderful sample, and it may actually, if miracles happen, it might actually be representative of all the plants. But when you take a look at the operating experience of the different plants, the different physical characteristics of these plants, my take on it, and again a layman's take, was that a larger sample would be worthwhile.

MR. OZER: Your reference then was just to the M5 samples that were received up to now by ANL. Is that correct?

MR. SHADIS: Well, yes. What I was saying was that's a welcome addition, and the larger the sample that you take from operating reactors with all the variations, all the inputs varied, the better. That was my comment on that. I'm sorry I wasn't clear about it. Does that answer your question?

MR. OZER: Thank you.

MR. SHADIS: Thank you.

CHAIRMAN POWERS: Thank you very much.

MR. SHADIS: Thank you, sir.

CHAIRMAN POWERS: At this point, Ralph you and I suppose Farouk are on the hook to offer any responses that you have.

MR. ELTAWILA: Yes. I would like to make some comment, Mr. Chairman. I don't have a prepared speech or anything, so I might go all over the place, but I hope you bear with me. But before I start, I really want to thank all the presenters. They have done a fantastic job, and the presentation was very clear and very informative.

CHAIRMAN POWERS: You need to come to our fuel subcommittee meetings. This is the standard.

MR. ELTAWILA: Absolutely. You were speaking of the standard. It's very hard --

CHAIRMAN POWERS: It has nothing to do with the membership of the committee. It has to do with the participants, and the man to your right, and the woman to your left are largely responsible for this high technical level.

MR. ELTAWILA: Absolutely. And they are congratulated for the working they are doing. At the beginning, I mentioned this is a cooperative program and we are going to hear some difference in interpretation. I don't know if it's me only, but I really got the feeling that until Bert Dunn started speaking, that the industry is distancing itself from the Argonne program as if they have not participated in that program. And that came as a surprise to me. The Argonne program is not a matter of interpretation of the data, it's criticizing the underlying of the whole test program. And that came as a surprise to me, so I really hope that the industry -- I think I appreciate Bert at the end say that we were involved. There was a tech committee. That was a lot of interaction with the industry about the formulation of the tests, so there were ample opportunity to express their views about some of these tests, and at any time

if there was a need to change direction, that would have been done, but at this stage, raising substantial issue that's going to cost us millions and millions of dollars, I find myself, that's expressing my opinion, we're going to finish our program if the industry wants to pursue additional tests, provide data to support the rulemaking - that will be their prerogative to do that. But we feel that we have enough data to support a rulemaking.

The other things that I'm surprised at, they are coming here saying we don't want to see a rulemaking. We don't know even what's the rulemaking going to read. An example, a rulemaking can be say, maintain coolability by maintaining the temperature at 2400 degree and oxidation calculated by Cathcart-Pawel, and then provide a sample, as an example of doing that see Reg Guide. A Reg Guide can have a specific issue.

As an example, the industry can always come back and say we are going to have our own justification for different bases for a performance-based regulation. But right now, the rationale for saying do no go with the rulemaking because there are not sufficient data, I wonder who is going to produce the data? I don't think it's going to be NRC, because

we have a program. If there is a fatal flaw in this program, I would like to know about it. But at this stage, we have enough work that we are ready to proceed, and I think we have to let the rulemaking process identify -- you know, you issue it, you look at the language and see if the language is going to provide the assurance that we want, the safety of the nuclear power plants. I think I'll stop at that and see if Ralph wants to --

DR. MEYER: Thank you. I had just four more specific comments than that, that I wanted to make. And the first is to acknowledge that this was the first time that we've spoken in public and to the industry about the specific direction for the proposed criteria, so there was an element of surprise that I acknowledge here. And I was pleased to see that Odelli pointed out that some of the criticism that was contained in the slides was based on an assumption of a different direction that we might have taken. And the assumption wasn't a bad assumption, because we had six months ago talked about the correlation at some length, as if we might adopt it. But those discussions subsequently took place internally, and we brought them here for their first public outing, so I wanted to acknowledge that.

Now I wanted to make a couple of other points. In another one of the comments on Odelli Ozer's slide was a lament about the integral test with the Robinson rod not having been performed yet. They will be performed very soon, but I would like to point out that the integral test will not produce, nor directly affect the embrittlement criteria themselves, because they come from a mechanical test. They certainly will have an impact on supporting information and other discussions that take place should we go forward with the rulemaking, but the absence of those tests do not handicap our efforts, at the moment, to try and formulate embrittlement criteria.

The third comment that I would like to make has to do with prehydrided material. It's obvious that there's considerable value to taking unirradiated cladding, charging it with hydrogen, and then testing it as if it were a substitute for the real irradiated thing. And we're doing some of that ourselves, we're doing it primarily to calibrate test plans for tests on the real irradiated cladding.

We have had discussions with the industry in public meetings about the desirability of trying to validate some prehydrided surrogate for irradiated

material, such that in the future one wouldn't have to go to hot cell with expensive programs to make some tests of this sort. And I simply want to emphasize that such a validation has not taken place, may take place in the future, but it requires testing on irradiated fuel rods in comparison with prehydrided rods to show that the equivalence or similarity is there. So I just want to sort of blunt any thinking that might jump too quickly to the use of prehydrided unirradiated cladding, as if it were irradiated fuel cladding.

And finally, the fourth comment that I want to make is - and I won't belabor this point, but it's about Bert's comment about the quench survival versus ductility, which is the true basis for the regulation. And we have, indeed, had many interesting and friendly discussions about this subject. And I simply, for the record, would mention a letter from Farouk Eltawila to Rosa Yang dated February 25, 2004, that discusses this subject, and records our opinion that the basis remains ductility. It doesn't mean we couldn't change it, but such a change would, indeed, be a fairly radical change from the current rule and its basis. And we have, as I said before, have attempted to make the course correction as gentle as

possible. Thank you.

CHAIRMAN POWERS: Thanks, Ralph. Rosa.

MS. YANG: Is it okay I respond to some of --

CHAIRMAN POWERS: Rosa, go ahead.

MS. YANG: Okay.

CHAIRMAN POWERS: Just pull up a chair and sit down, Rosa.

MS. YANG: Okay. All right. This is Rosa Yang from EPRI. First, I'd like to respond to Farouk's surprise about our presentation. I think just like Bert said, and I think in Odelli's presentation, Robbie's presentation, we're part of the program, and we strongly supported the program from the beginning until now. I think as you said, and many of us said, that the interpretation is different. We have different interpretation. I think it is understandable that the reason we've come up with the presentation we have, because as recently as two months ago, we were all meeting in Argonne looking at the program results, and we were very excited about the results. That's probably one of our best meetings. I couldn't say best, better meetings. It was just really insightful, and on one hand you're very excited about results, on the other you recognize

you're on a very steep learning curve, because the results are so different from different labs. And that's not a criticism. I think that's a normal process of any scientific research. And I think it's important we recognize that. And maybe even more -- maybe that's why we were misled or whatever, is the embrittlement correlation, which was part of the handout I see, but Mike did not address. That particular correlation was discussed in great detail.

In fact, we went ahead and mapped out the plan on how to implement that correlation, with all due respect. I'm sorry, but we had -- and since that meeting, we have followed through with experiments in CEA trying to understand it. And I think the presentation that Jean-Paul Mardon did, a lot of it are very, very recent results, really trying to understand the differences. And I think, Farouk, I'd like you to see that as our very, very positive contribution. And that's what -- I guess, I feel a little bit wronged, and we're doing our best trying to understand it. So we are an active member, and we're very proud of Argonne achievement.

And I also want to say, many of us in this room have write very, very strong letter that contribute to Mike's being the award winning

metallurgist and scientist. I want to go on record for that.

So having said that, that's maybe kind of where we are coming from. So, indeed, we're a little bit surprised by what Ralph presented this morning. I would say pleasantly surprised, but I just want to add my own personal opinion. I don't quite see how that's performance-based, but I'll learn that later.

CHAIRMAN POWERS: You're not the first to struggle with what is and is not performance-based.

MS. YANG: Okay. But overall, I want to say why --

MR. ELTAWILA: It will be performance -- Rosa, it would be performance-based if provided as an example, but not as a requirement. As I indicated, you can have a general criteria, maintain coolability by such and such, and such, and you provide example in the Reg Guide. And always, you can have the option, technology change, new information comes, you update the Reg Guide rather than the regulation itself.

MS. YANG: Okay.

MR. ELTAWILA: That's an approach that you can go.

MS. YANG: Let me just finish with one thing about the if we are ready or not. I think there

are a lot of data being presented, but it's clear to see their inconsistencies. And I forgot, either Dana or Bill said very eloquently, none of these tests are the real test, but we need to figure out, we don't want any of the artifact to bias what we're trying to protect, so there is -- I think it's premature. I mean, the whole embrittlement criteria is not a linear relationship, and it's wonderful to use an empiric, something simple and empirical, but until we understand it, I just don't see how we can be ready. And I'm even more puzzled when Ralph said we're going to continue to look at the empirical embrittlement correlation, and that would kind of form the basis for the rulemaking criteria. That suggests to me that there's some work to do. Thank you.

CHAIRMAN POWERS: Any further comments? Then I'll turn to the committee. Now -- oh, I'm sorry.

DR. BILLONE: My comment was really going to be I'm hungry, so I'd rather turn it over --

CHAIRMAN POWERS: You're hungry. At this point, I turn to the members of the committee. What were, as I understand, what the RES wants feedback on is what is expressed on pages 28 and 29 in the slides Dr. Meyer used at the opening, which he presented some

possible criteria. It's challenging, I know, to separate the technical information from the criteria, but I think in the end we're going to have to write a letter that addresses those criteria, so to the extent that you can address those in your comments, I would appreciate your doing so. And I'll begin with you, Mr. Denning. Professor Denning.

DR. DENNING: Since I'm the least experienced here.

CHAIRMAN POWERS: No, I didn't say that. I said the shortest tenure on the committee, not the least experienced.

DR. DENNING: Well, in this area I certainly am, but based upon what I've heard today, certainly there is the normal amount of ambivalence that we seem to often have here, in that the question of are we ready to go forward, I think in one respect it's clear that we are, and that is that we're operating plants, and we've been operating for quite a period of time with a great deal of confidence that they're being operated safely under criteria that are pretty similar to those that RES is talking about here.

With regards to is there really adequate technical basis for the three general criteria there,

I did have some concern based upon what I heard today, as to whether this time-related criterion has been fully supported by the experimental work that is going on, and is still going on.

And then let me raise just one other issue, and that is the question of whether we should go forward with part of 50.46 and not with all of it together. And I do see some relationship here between the two. I haven't given much thought to that, and it's apparent that the NRC has decided that one can go forward with the two independently, but there certainly is some overlap between the two, and it does seem rather strange to me that we would go forward on part of it, and not the other, when there is as much close relationship as there is. So I guess that I think that certainly we need a lot of discussion before the ACRS is going to be ready to support the RES proposal here, particularly in light of the fact that industry has kind of taken the other position, and if the industry is saying we shouldn't go forward yet, then it's a little bit hard to say we ought to proceed forward, because I think that the whole 50.46 activity is really in response to the industry's feeling that by changing those regulations, there might be room for safety improvement or financial

improvement.

I also suspect that if the industry had known what RES was going to say today, that their presentations might have been a little different. I don't know, but I did have a feeling that they might have been at least a little bit different. And I think there was a little bit changing in midstream even of the positions they took, so that's kind of where I stand at the moment.

CHAIRMAN POWERS: Dr. Kress.

DR. KRESS: First off, I think the database and the experiments have been well-conceived, and well done, and I'm really pleased to see this being developed. I originally thought I might see some sort of correlation of embrittlement versus the uptake of hydrogen and oxygen effects on the phases, but I'm very doubtful that such a correlation would be very useful, frankly. I don't think we can do that. I really like the empirical approach. Let's do a correlation with this time and temperature, and the time and temperature is, I think, a one-to-one correlation with the Cathcart-Pawel. By the way, the pronunciation is Pawel. And I believe there is a one-to-one correlation with temperature and time. I mean, time and temperature is the parameter that we judge.

And I like the thought that one sees the square root of that as related to the various changes in phases, and the various movement of the material, so I think it's a good approach. And I think it's supportable by the data we have.

Now I do think that the cool-down effect needs to be tied down just a little bit better, and related to the actual LOCA conditions we may have. That's the one place I think needs to be tied down just a little bit more before we go to rulemaking, but I think that's something that can be done rather quickly. So given that, I'm in favor of going forward with the rulemaking. I think we also need to -- I understand the tests that you do with the clad with the fuel and everything else removed was an expeditious way of doing it. I do think you need this confirmatory test with the actual fuel. But I'm like Ralph, I think that's just going to be a confirmatory test, and it's not going to change your criteria. I think the way you've developed the criteria based on the actual use of the clad and the Cathcart-Pawel relationship is good, so I'm encouraged that you've captured the ductility requirements by this, which that's a very nice way to do it, because to me, it is the least painful way, and it does capture it. And so

I'd say go ahead with the rulemaking, and you've pretty well got the right idea. That's basically my reaction to it.

CHAIRMAN POWERS: Dr. Shack, I can't ask you to comment.

DR. SHACK: I'm sorely tempted.

CHAIRMAN POWERS: The cat can keep your tongue. We eschew your counsel with this.

DR. KRESS: One thing I did want to say, though, about the slides on 28 and 29. Even though I think you have the right idea, I think they're phrased wrong. I think I would re-look at those, and I think the guidance on asking the people to do the testing needs to include more protocol on how the testing ought to be done.

DR. MEYER: Yeah. I don't know that I mentioned that in connection with this, we fully expect to develop a regulatory guide, because there are a lot of details involved.

DR. KRESS: And that would include the --

DR. MEYER: Oh, yes. But the regulatory guide would, of course, not be unreal.

DR. KRESS: That's probably the way --

DR. MEYER: The rule itself would be stated simply, and NRR will --

DR. KRESS: Yes. I don't think that the rule stands alone by itself without a regulatory guide.

DR. MEYER: Right. I agree.

DR. KRESS: Okay.

DR. SHACK: Just it's not quite a comment, but I would like to see a lot more of the data expressed in the way the criteria is set up. I see weight gain, actual ECR, Cathcart-Pawel ECR. I mean, I would tell my contractor to shape up and go off and analyze all that data that's out there, and express it in a way that you can compare it against these criteria and find out when I draw the lines, I color all the dots that are more than 45 minutes orange, does everything that sits above the limit in the color orange. There seems to be a whole lot of data out there. I can't tell whether it's consistent with these criteria or not. There may be a scrap of paper sitting on Ralph's desk somewhere that shows all this, but I haven't seen it.

DR. KRESS: Well, I particularly liked that, I forgot, was it Mr. Ozer that presented the integral value of the energy and showed the line that Cathcart-Pawel separated the flow -- I thought that was a good approach at showing that. And I think he

also mentioned you could get a one-to-one correlation between the energy and the Cathcart-Pawel time.

CHAIRMAN POWERS: Maybe in deference to Oak Ridge, I point out the bounding line was actually Baker-Just.

DR. KRESS: Well, okay.

CHAIRMAN POWERS: We didn't even know what that other line was.

DR. KRESS: The line was --

(Simultaneous speech.)

DR. KRESS: But I was impressed that that made a good line to separate the two. That's one of the reasons I thought they were on the right track.

MR. ELTAWILA: Can I make one comment?

CHAIRMAN POWERS: Please.

MR. ELTAWILA: I think for the industry, you have to understand the process of when we go out for a notice for rulemaking, that starts focusing the discussion. Right now we are just trying to test the program, so proceeding with the rulemaking itself will focus the discussion, will identify what's needed, industry can provide its views, NRC can provide its view, and we'll start interacting with each other on the rule itself rather than on what kind of tests we're going to run. So just think about it in that

regard, that the earliest we can issue a notice for a rule will be March of next year.

DR. MEYER: And that's even -- the best thinking now is that would be an advance notice of a proposed rulemaking, not even a notice.

MR. ELTAWILA: That will help us just achieve the goal that we want achieved. Right now just test the program without an end in sight, and that's what I'm concerned about.

CHAIRMAN POWERS: Well, there's no question that a tremendous amount has been accomplished. I, myself, believe that I have shared agonizing over this issue of what to do about the non-Zircaloy clads for the last eight years or something like that, so this is not a precipitant program by any means, but rather a rather deliberate program. And I presume the understanding has expanded enormously here in the presentations. What I noticed, and I think this was reflected in many of the speakers, is nothing that we've seen here changed the goals that we have for this particular aspect of the rules; that is, the coolability, keep to criteria and things like that seem to be a constant that's not going to be changed, and we're comfortable with that.

Some have questioned whether ductility is

the bound to take for that, but we don't have really any warm alternatives on this immediately in the offing. Now that may change because I think Dr. Eltawila said that what happens when you have a notice of rulemaking is we go from a joint program where we are free to make independent interpretations, to a program where we try to unify those interpretations. And quite frankly, my experience with that process is just about everything becomes fair game in that discussion.

We have a question before us as to whether the proposed criteria are the ones that can at least start that process, and it seems to me in thinking about things, they have a certain attraction to them, but they run rough against the more explicit quantitative criteria that appear in the current rule, so they're a little bit jarring when you look at them.

One issue that had seemed to be absolutely universal agreement on was that this hydrogen synergism and its effect on the data was a recognized phenomenon, that even the quantification of it I didn't see radical differences in the data and whatnot. And the question that I think we need to confront the rest of the committee with is do we treat that more explicitly in the rule, or do we address it

via this corrosion layer subtraction process at the end?

The break away avoidance step in the criteria is an interesting one, and I don't know that we explored it too thoroughly, but I got the impression that that was an area that there was also broad agreement, that you want to avoid this break away and the excessive amounts of oxidation, plus the possibility of additional hydrogen uptake in that, and so I didn't -- the really thorny question that comes up here is whether this minimalist approach is appropriate, and it's pretty clear why you would want to undertake this kind of a minimalist approach. There's an opportunity open to us presented by changing 50.46 to couple these things together and do that, and you can do it and get rid of a problem that exists even if you don't accommodate anything that comes down in the future; that is, I can almost guarantee you there's going to be another kind of clad that's going to come down within the next 10 years that will have even better properties and more wonderful features to it, cheaper to make and everything else, and it will run orthogonal to anything we set up in the short term. And that may be a cost that we endure versus setting up a far more

generalized rule. And I think that's one issue that the Full ACRS is going to have to confront. And in advising the staff on what they might choose to present to the full committee, I think it's that strategy aspect of your proposal. It's much less of the technical details, the things that will show up largely in a Reg Guide, how you do the experiments -- you're going to have to show something about the technical foundations for this approach, but I think that's a supportive and not a major thrust. I think you need to say here's why I came up with these criteria, and why I'm taking the approach I am, because I think that's what the full -- we're going to need the wisdom of the full committee to formulate a response in that effort, because I think that's one of the questions that we're confronted, not just these three criteria that you've laid down here. So in developing your strategy, that's what I would think you would want to present.

I don't know, are we planning to get presentations, Rosa, from the industry at our full committee meeting?

MS. YANG: We haven't been informed.

CHAIRMAN POWERS: Okay. Before the committee can write a letter, the full committee has

to hear something. And in some cases it can be done by the members summarizing. This is not an area that I would like to summarize and whatnot, and so there will be a presentation, and my recommendation is that you did a pretty good job here in Odelli's presentation and Mr. Dunn's presentation in addressing this strategy issue. I don't think we've got -- I have a feeling the technical details are going to get worked. The technical details that upset the apple cart, they're important to bring up. The technical details over things that -- how you do ring compression tests and stuff like that, those get worked in the Reg Guide, and strategy is the issue here. And from your presentation, is that strategy now or is it strategy later? And I think Odelli and Mr. Dunn did a pretty good job in articulating their position. Some of the stuff that Robbie presented where he looked at here are the kinds of things that you guys need to question, the supporting database can again be supportive of those positions, and I think they're well articulated. That's my advice.

MS. YANG: Thanks. What kind of schedule are you looking at?

CHAIRMAN POWERS: This is scheduled for September, and I think we put two and a half hours on

the agenda. And I think we will absorb it all, so you're looking at something like maybe a 45 minute presentation, something like that.

One of the issues I think we're going to need clarification on at some time is the coupling between this change to the 50.46 rule and the thermal hydraulic change, particularly the temperature transients that can be expected in there. I don't know that it's essential to have that for this discussion in September, but if we go forward by any path, that coupling is going to be -- we're going to have to really understand that a lot better than I do right now.

DR. DENNING: In that regard, Dana, should we be asking -- I had not realized before it was said here that they could be separated, and I think the NRC is recognizing them as being separated. I'm kind of curious as to what the logic is, and whether we ought to have a brief presentation on how - I don't know, is it NRR - how they see the two being coordinated, and how they think that they can separate them.

CHAIRMAN POWERS: My impression is, and I'm going to let Farouk talk in answer to your question in a second, but my impression is the thermal hydraulics can proceed independently of the fuel

criteria. It's not entirely clear to me that the fuel criteria can proceed independently of the thermal hydraulics. With that, did you have a comment, Farouk?

MR. ELTAWILA: I think that's exactly -- and just from a practical point of view, the Commission will be issuing SRM on the Part A of the rule very shortly, so we are way ahead in the thermal hydraulic part than the large break LOCA, or the LOCA break definition. We're way ahead in that area than in the fuel area. They might be coupled together at the end or something like that, but we are proceeding right now, and we are here at the beginning of the process.

CHAIRMAN POWERS: Almost assuredly, no matter what decision the ACRS makes, I can assure you this is an issue that we're going to get to discuss face-to-face with the Commission. And it's the strategy issue that they're going to be interested in and whatnot, and so we're going to have to spend close attention to that because what view -- the collective view presented to us is what we're going to be presenting to the Commission, and so don't send us up there naked, please. I really get embarrassed when I have to stand up there and tell them I don't

understand what the strategy is, so help us out to any extent that you can there.

Again, let me say that I like coming to these subcommittee meetings. Mr. Atherton is back, and he asked if he could say a few words, and I certainly want to accommodate him.

MR. ATHERTON: I'm not sure that this would be an appropriate time, perhaps tomorrow would be --

CHAIRMAN POWERS: If it deals with RIAs it's fine for tomorrow. If it deals with LOCA, it's fine for today. If you can't tell, then go ahead.

MR. ATHERTON: Okay. Is it okay if I --

CHAIRMAN POWERS: Yes, please sit down.

MR. ATHERTON: I'll try to be brief. Some of what I planned to cover has already been covered, and so I don't wish to go over that again. My name is Peter James Atherton. I work part-time as a Nuclear Safety Consultant, and in doing so, I provide technical advice and guidance to people in and around various nuclear power plants.

The basic concern that these people express to me is not an interest in what's happening within the nuclear power plant generating station, but the radiation releases that come out of the plant, and

so for the last several years I've been involved in the environmental aspects of radiation releases working my way backwards to the plant, which is the source of the radiation releases, and this is the first opportunity I've had to get involved and listen to what's happening within the reactor pressure vessel itself, so I have a few comments I'd like to; and that is, one, the public is concerned about radiation releases. The brunt of the radiation releases are a result of what's happening within the reactor pressure vessel. And what I haven't perceived is a relationship between what you're doing here in the rulemaking, which is my first involvement with this, and ultimately what the people outside the plant and in and around the plant are concerned with, and that is the radiation that is released by the power plant into the environment.

And the comment I have is, is there any way to -- since the public as a general rule is probably not going to get involved with the technical details of this rulemaking, some sort of a statement or a paragraph, or something that addresses the relationship of what you're doing to the radiation release or the lack of it, that would ultimately prevail if this rulemaking takes effect, I think would

be very helpful to them.

In doing that, I would also suggest, and I didn't hear anything about this at this meeting, and perhaps it's addressed in a different way, I used to work -- I was hired by the Atomic Energy Commission. I used to work for the Nuclear Regulatory Commission in the 1970s, and common mode failures was a big item back then. And I don't see that language being used today; that is, if you have a causal factor for one fuel rod to fail, what's to prevent other fuel rods from failing simultaneously with that same causal factor? That was not addressed, and from what I saw, it does not appear to be a subject matter at this time, but it is something that would ultimately effect the public should you have multiple failures within the reactor pressure vessel.

I'd like to say something that's very positive for me, and I happen to be a real-time advocate that is if you use computer codes to do this, that, and the other, I'd like some sort of verification that they're accurate, and they're going to work. And I have problems from an environmental perspective with the use of computer codes without any kind of testing to verify that the dispersion model for the radiation releases at the stack are what the

model says they are. And what I was really very happy to see is that you've gone through at least some semblance of destructive testing. I guess you all realize that fuel failures are not something you want to deal with. You'd like to keep the fuel in-tact, and you seem to be going in a direction which I would like to say I agree with concerning empirically testing these fuel rods to make sure that they will endure or withstand, or perhaps they won't and you've got to do something about it. So I'd like to raise that as something that I'm very thankful to see.

And I just want to from a perhaps -- I've been criticized for being naive or idealistic, but the NRC's basic mission to the public is protecting the public health and safety, and generally that has to do with radiation releases. And whatever you do here and the technical details of how you do it, that impression seems to be lost as a basic mission of NRC. And I would be interested personally in seeing that incorporated in what you're doing in some way, shape, or form.

And I have one question that I'd like to ask of the French, and it has nothing to do with anything that's happening here, but it's something that I've been curious about personally, and was

wondering if I'd be permitted to ask that.

CHAIRMAN POWERS: Well, I guess you'll have to ask and I'll tell you if you're out of order.

MR. ATHERTON: It's very simple. Why do the French only have pressurized water reactors for nuclear power generating stations?

CHAIRMAN POWERS: I think that's outside of our scope. You might get to them privately on that.

MR. ATHERTON: I just wondered if they knew something we didn't.

CHAIRMAN POWERS: Let me just comment that the issue that you are most concerned with, that is the radiation release, is what's embodied in the goals of this activity. And Dr. Meyer listed them down, I think other speakers listed them down. This is the coolability, whatnot. There is some radiation releases of course associated with rupturing the rod, but you want to stop it at that. And it's the coolability and maintaining of coolability, and those are the issues that you want to look at. And that's why I was so curious of a previous speaker, are you comfortable with those goals? Because that's why they were -- that's why those goals came about, is to stem radiation release at this barrier and our defense-in-

depth strategy fails.

MR. ATHERTON: Well, the defense-in-depth back in the 1970s was the fuel cladding, the primary if it's a pressurized water reactor, the primary coolant system, boundary, and then lastly, the containment.

CHAIRMAN POWERS: It hasn't changed much.

MR. ATHERTON: The problem with say pressurized water reactors which I'm most familiar with, is that perhaps under the high pressure and temperature that they operate at, there's been a tendency in the past to have the steam generators fail, so any problems with the fuel is not necessarily contained within the primary system as long as it leaks passed the steam generators into the secondary system.

CHAIRMAN POWERS: That's another issue that we're not really dealing with today.

MR. ATHERTON: Okay.

CHAIRMAN POWERS: Stay tuned. It's on our agenda.

MR. ATHERTON: That's how the radiation essentially gets released to the environment, and that would be something I would be interested in.

CHAIRMAN POWERS: We're trying to stop it

before it gets to that stage. Okay. Thank you very much. Well, let me conclude by again saying how much I enjoy these meetings, and the high quality of the technical work here - I mean, it's really excellent technical work, both in the experimental side and in the interpretation, analysis, and critique side. I think those are just really excellent contributions to my understanding, these help me a lot. I'm not giving you part of my salary for that, but I did appreciate it very much. We will recess for the day and tomorrow we will turn to the RIA issue, which itself is an interesting, though not quite as pivotal an issue in reactor safety.

(Whereupon, the proceedings in the above-entitled matter went off the record at 6:28 p.m.)